ACCEPTANCE OF THE ROEBLING MEDAL OF THE MINERALOGICAL SOCIETY OF AMERICA

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An occasion such as this must always be a very pleasant one to the proud recipient of the award, and I think it is doubly so when, as in the present case, the award comes from colleagues in a branch of science other than one's own. You have gone outside your own body today in awarding me the Roebling Medal, for no one, however kind and generous, could describe me as being a mineralogist. I am glad that you do not expect the recipient of the medal to pass a simple examination in mineralogy before he can accept his prize, for I am sure I would fail in that test. It is not merely a matter of knowing the subject, it is a deeper and more fundamental difference in outlook which divides the physicist from the mineralogist. Professor Niggli put this point very clearly in his address to you when he received the medal last year. An exact scientist endeavours to simplify and generalize problems so that comparatively unequivocal decisions can be made. Followers of descriptive sciences such as mineralogy and petrology can never approach their problem from a one-sided standpoint, but must consider how products of nature came to be, "what ultimate causes (irrespective of chemistry and physics) gave them their peculiar aspect and relationship to other occurrences." I have been deeply interested in the structure of minerals but have always viewed them with a physicist's idealization and simplification. I remember that I once incurred the disapproval of Spencer, my compatriot who received the medal in 1940. I had described the atomic arrangement of a beryl crystal, but had omitted any reference in my paper to the composition of the particular crystal I had studied or the locality from which it came. Spencer as a mineralogist naturally thought that this greatly lessened the value of the analysis, but of course as a physicist I was only interested in the perfect arrangement of a crystal of ideal composition, the scheme of structure common to all beryl crystals, and it never occurred to me to ascertain what particular variety of beryl some kind friend had given me to analyze. Again, I remember an occasion when I gave a lecture at the Sorbonne in Paris on the structure of silicates. My chairman was Professor Mauguin, holder of the chair of mineralogy, and he was deeply shocked to hear me begin my lecture with the statement that there were only six minerals in the earth's crust if one neglected tiresome details. When the lecture was over, he led me firmly to a great collection near the theatre where endless specimens were displayed in their cases, and sweeping his arm dramatically said to me 'Les six minéraux!'

I feel it all the more an honor that you should have seen fit to make this award to one who can make no pretense of knowledge of the descriptive side of your fascinating subject. My contribution has been an attempt to increase our knowledge of the fundamental features of atomic arrangement in idealized mineral structures. The analysis of crystalline arrangement by *x*-rays is a borderline subject, and to me one of its greatest rewards has been the contacts I have made with colleagues in other branches of science—chemistry, metallurgy, biochemistry, and mineralogy. A prophet is said to be not without honor save in his own country and amongst his own kith and kin. Indeed we see today an instance of a prophet receiving a much prized honor in a country not his own and from scientists who are not kindred physicists.

An occasion like this is an excuse for personal history. I am particularly pleased at getting the medal because all the crystals which were analyzed in the beginning of *x*-ray analysis were minerals—rock salt, zincblende fluor, pyrites, diamond, calcite and others. This was partly due to our needing large and perfect specimens for the *x*-ray spectrometer which my father designed, the instrument with which all the data for analyzing these crystals were obtained.

But I think an even more important reason was that the only people who had kept alive the study of crystals were the mineralogists. It is perhaps fair to say that the chemists knew that there was more than one kind of crystal—they knew of two kinds, needles and plates. But the rest of the scientific world with a few eccentric exceptions had never thought of the laws of formal crystallography and the possible ways of making a pattern in three dimensions. When Laue discovered *x*-ray diffraction, my father was intensely interested because he had speculated deeply on the nature of ionization by *x*-rays. I became interested through discussions with my father, but I think the decisive factor, as far as I was concerned, which led me to interpret Laue's photographs in terms of the crystal structure of the zincblende crystal he used, was a paper on Pope and Barlow's theory of valency volumes. This paper was given to a society of young scientists, and it introduced me to the geometry of three-dimentional structures and to such ideas as close-packing of spheres. But here I have a terrible confession to make. I was so ignorant of anything to do with crystallography that when I tried to explain the genesis of Laue's picture by reflections of the *x*-rays from atomic planes I invented my own system for naming these planes. I was blissfully unconscious that crystallographers had already given some thought to this point of detail!

I was soon put right by Hutchinson, at that time lecturer in the mineralogy department at Cambridge, and later its professor. In my young days the professor was Lewis and he had given strict orders that no mineral should ever leave the safe-keeping of the collection at Cambridge. I shall never forget Hutchinson's kindness in organizing a black market in minerals to help a callow young student. I got all my first specimens and all my first advice from him, and I am afraid that Professor Lewis never discovered the source of my supply.

After the 1914–18 war we developed more powerful forms of x-ray analysis in my research school at Manchester, and cast round for subjects on which to try our new methods. I chose the silicates because fine crystals of them were available and because they were a good deal more complex than anything we had yet attempted. The work had a quite unexpected reward; the natural order of the silicates suddenly became clear in all its beauty and simplicity. I always regard this as one of the most exciting and aesthetically satisfying researches with which I have been associated. The significance of the formulae of silicates, the rather bewildering way in which atoms could be replaced by others, and the relation between the different families, was seen to be part of a simple geometrical scheme of linking silicon-oxygen tetrahedra together. One interesting feature of this new work was the complete justification of the mineralogist in basing his classification on crystalline form and not on chemical constitution.

Many contributed to the final elucidation of silicate structure. I always think the turning point was marked by the successful analysis of the pyroxene diopside. I brought over to the U. S. A. the measurements which West and I had made in Manchester, when I came to Cambridge as a visiting professor in the Massachusetts Institute of Technology in 1928. I was fortunate to find a clever research student willing to apply himself to the problem and he found the solution. I feel proud that I enticed Professor B. E. Warren to take up x-ray analysis, for you will be familiar with the fine work on crystals and glasses which he has done. At Manchester I had an active collaborator in Zachariasen, now professor in Chicago, who had the reputation in the laboratory of turning out a crystal structure a day when most people took months to complete an analysis. The silicate structures afford one of the finest examples of 'Pauling's Rules' for the coordination of ionic compounds. Then again one of my staff at Manchester, W. H. Taylor, solved the important feldspar structures. He is still with me at Cambridge and I am glad to say that he has turned his attention again to feldspar structures. His original work showed that the same general scheme ran through them all, whether monoclinic or triclinic, as indeed the mineralogist had always divined. He has done some beautiful work on the mixed potassium-sodium feldspars, and is now tackling such problems as microcline, sanidine, and the bewildering complexities of the plagioclase series. Finally, we cannot exaggerate the debt we all owe to V. M. Goldschmidt whose brilliant work on geochemistry has opened up a new branch of science. In receiving your award for the contributions which *x*-ray analysis has made to our knowledge of minerals, I feel strongly how short a way I would have gone had I not been in the company of these pioneers.

To refer again to the address which Professor Niggli gave when he received the medal last year, he told us that as a young man he had been strongly advised by his friends to apply his obvious scientific ability to one of the more fundamental sciences. They told him that the study of the lithosphere was already so advanced as to leave only routine work still to be done, and Niggli declared that he had not regretted his refusal to accept this advice. I had a similar experience. Soon after we all returned to our laboratories in 1919 scientific friends advised me to drop the study of crystals. They pointed out that all crystals would soon be worked out, and that I would find myself out of a job. I did not take their advice and I also have no regrets. I remained faithful to my first love, the study of the atomic architecture of matter, and I feel that it has provided me with an absorbingly interesting scientific life, bringing me into contact with other scientists in many other subjects. If it were necessary, I should be still further fortified in my belief that I chose well by your generous award of the Roebling Medal today, a compliment I deeply appreciate and for which I tender my warmest thanks.