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ACCEPTANCE OF THE ROEBLING MEDAL OF
THE MINERALOGICAL SOCIETY
OF AMERICA FOR 1971

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Mr. President, Professor Pabst, guests and fellow-members of the Mineralogical Society of America:

First of all I want to thank the selection committee and their chairman, Professor Kullerud, for proposing my name, Council for endorsing it, Professor Pabst for presenting me in such generous terms, and President Goldsmith for giving me this beautiful medal.

As you see I am well acquainted with the various cogs in this numismatic machinery: indeed I have served on several occasions in the various capacities. Each time I found my duty to be a very pleasant one. Today, on the receiving end for a change, I am experiencing mixed sensations: I think I know how President Truman felt when, on assuming the awesome duties of his office, he commented that many men in the land would have been better choices, but inasmuch as he had been given the job he would accept it and do his best. Since you have given me the medal, I accept it with gratitude, but in my heart I shall divide it with those who made me what I am and those who have shared my toils: my father and mother; my professors at Liège and Stanford; my co-workers; my students, "predocs", "docs", and "postdocs"; and especially my dear wife and comrade, collaborator, and colleague.

Let us salute the memory of Colonel Roebling, in whose honor this award was established. Roebling, the engineer, prodigious mineral collector and bridge builder, who completed the Brooklyn bridge in 1883, six years before the Eiffel tower was erected in Paris! The Brooklyn bridge must have been a fabulous achievement at the time, for I remember that, when I left my hometown of Waremme in 1925 to sail to U.S.A., my father (born in 1875) said to me, "You will see the Brooklyn bridge!" Little did I know then about Roebling and what his name would come to mean to me in my later years.

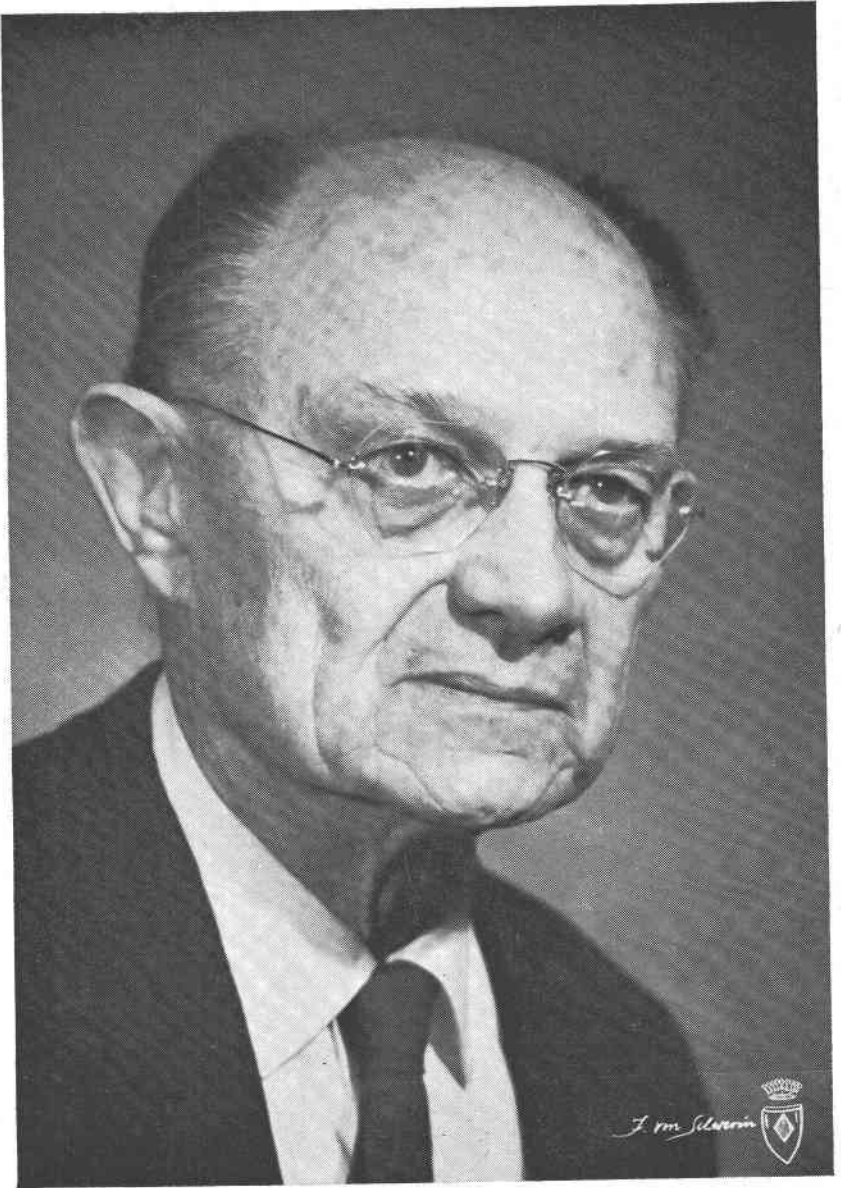
Of more immediate concern to me, at the time, was another great bridge builder—the Belgian-American Educational Foundation, established in 1920 to commemorate the work of mercy done by the Commission for Relief in Belgium during the first world war. For five

years (1914–19), under the chairmanship of Herbert Hoover, the C.R.B. performed the diplomatic and logistic miracle of feeding the starving population of German-occupied Belgium and northern France—nearly ten million people, including me. Without the C. R. B. and without the fellowship I received from the C.R.B. Educational Foundation, I should never have come to America and Stanford. In this my hour of euphoria, may I publicly renew my thanks to these two humanitarian institutions, of which U.S.A. can justly be proud. Let me also gratefully acknowledge the support I have received, through the years, from the National Science Foundation, another remarkable institution. To those of us who considered it anomalous to have the civilian university research of our country funded by the military, the creation of the N.S.F. came in as a godsend.

As I look back over forty-five years of research, I feel I can repeat, as my own, the statement that my former professor Austin Flint Rogers made to me when he retired, "My scientific life has been an everlasting picnic!"

H. W. Morse, at Stanford, infected me with his enthusiasm for research when he let me work on his artificial three-dimensional spherulites. Recognition of the rôle played by form birefringence in these aggregates of aggregates was of great help in understanding spherulite optics. Wiener's formulae also apply to crystal structures made up of superposed slabs of known structures, truly *mixed bodies* in Wiener's sense, such as we found in the bastnaesite-vaterite series, whose indices of refraction can thus be predicted from the relative thicknesses of the slabs. Today I have the satisfaction of seeing how important spherulitic crystallization is in metals, in polymers, in ceramics, even in crab shells! The study of regular textures in polycrystalline aggregates bids fair to become one of the important fields in crystallography.

My studies on the law of Bravais continued those of E. Mallard, G. Friedel, and H. Ungemach. They led to a first generalization, with D. Harker, in 1937 and to further extension, with Gabrielle Donnay, in 1961. The results were not readily accepted. The distinction I drew in 1933 between the "Haüy-Bravais lattice" (or morphological lattice) and the "X-ray lattice" (or structural lattice) was considered sheer nonsense by a good friend of mine, an outstanding mineralogist, whose judgment on other matters crystallographic I highly respected. He maintained there could be only one lattice—the lattice. Our friendly disagreement became compounded when I had to recognize not only two lattices, but two space groups! No one today would deny that different properties have different symmetries. Take halite,



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for instance. Optically its point symmetry is that of the sphere. Structurally a face-centered cubic lattice, with cell edge $a = 5.64 \text{ \AA}$, expresses the periodicity of chemically equivalent ions. Morphologically the cubic habit indicates a primitive lattice, with $a' = a/2$, which expresses the periodicity of the ions regardless of their chemical nature, Na^+ and Cl^- being equivalent from the viewpoint of chemical bonding. And magnetically MnO , which crystallizes in the NaCl structure type, has a cubic or pseudo-cubic lattice whose cell edge $a'' = 2a$ is equal to twice that of its chemical crystal structure. In short, morphology reveals the bond assemblage, X-ray diffraction the chemical structure, and neutron diffraction the magnetic structure.

In some quarters people contested the propriety of the term *law* as applied to the Bravais law and its generalization. They wanted to speak of them as *rules* or *principles* instead. The same critics, however, did not object to the "law" of constancy of angles or to Haüy's "law" of simple indices. They apparently could not see that the law of Bravais is nothing else than a more precise form of the law of Haüy, and that its generalization includes all three preceding laws as particular cases. By definition *laws of observation* or, synonymously, *empirical laws* are based on many facts; they enable other facts to be predicted, lead to the formulation of hypotheses, which can be tested against additional facts and (we hope) yield more laws of observation.

Since 1955 the *periodic bond chains* of Hartman and Perdok have illuminated the relations between morphology and structure. Hartman has given a theoretical reason why the generalized law of Bravais, without being perfect, holds true as well as it does. The geometrical approach of our generalizations and the physical approach of the P.B.C. are finally converging, to my utter delight. McLachlan's concept of laminarity and Schneer's calculation of the electron density, as Fourier series in which the coefficients are crystal-form frequencies, testify that the field is still alive, not only in Europe but also on this continent.

The compilation of *Crystal Data* began in 1943. The idea was to determine crystalline species from cell dimensions. The *Delanunay cell*, adopted in 1954, was replaced with the *reduced cell* in 1963. The conditions for a cell to be the reduced cell go back to P. Niggli (1928); they have now been programmed by A. D. Mighell and A. Santoro, with whom I signed the replacement section "Reduced Cells" in the 1969 reprinting of Vol. 1 of *International Tables for X-ray Crystallography*. In the throes of the publication explosion, the third edition of *Crystal Data* is soon to appear under the sponsorship of the National Bureau of Standards. It will be due to the labors of many dedicated

people. The mineralogical literature has been covered by our fellow-mineralogist Mary Mrose, of the U. S. Geological Survey. But there is one person who, as general co-editor as well as editor for inorganic compounds, has devoted more efforts to this project than all the rest of us put together. Without her selfless determination, her quiet courage and her resilience when things went bad, there would be no third edition of *Crystal Data*. When the books come out, you will know whom to thank—Dr. Helen Ondik.

Twins and other crystalline intergrowths (epitactic, syntactic, and topotactic) have given us lots of fun, particularly in cryolite (with Dorothy Wrinch) and staurolite (with V. J. Hurst and Gai Donnay). The curve of twin obliquity *vs.* composition, for the albite law in plagioclases, is unfortunately distorted by a computation error. The twinning we had to postulate in digenite to account for the peculiar X-ray omissions has been proposed in other sulfides by Nobuo Morimoto for the same reason; direct evidence for this twinning is still lacking, however.

Thanks to Curien and co-workers it is now realized that the *symmetry of the complete twin* is either black-white symmetry or color symmetry, according as the twin comprises two or more than two crystals. Holser has applied the symmetry of the two-sided plane to the interpretation of twinning. The current problem is to express the twin operation as a space-group operation and seek the structural significance of *twin glide planes* and *twin screw axes*. More and more, as the supply of good single crystals is giving out, X-ray diffractionists must rely on twins to solve crystal structures. In X-ray laboratories twins no longer "belong in the waste-paper basket". And this is progress.

The saga of generalized symmetry, started independently by Bethe (1929) and Heesch (1930), but most diligently developed by Russian crystallographers, recalls the happy times we spent working with the chemists of the Brookhaven National Laboratory during the summers of 1957 and 1958, and with the mineralogists of the Sorbonne, where I was a Fulbright Lecturer, in 1958–59. My wife and I remember this period as one of the most exciting in our careers. The magnetic structure of chalcopyrite was the first one to be solved by straightforward application of magnetic space-group symmetry. The paper had just come out when the discovery of the magnetic spiral (Yoshimori, 1959) destroyed any illusion we might have entertained on the general applicability of our method. But what compensation in the strange new vistas that suddenly appeared: the magnetic spiral was an example of A. Niggli's concept of degenerate symmetry.

The work on trioctahedral one-layer micas, begun with Nobuo

Morimoto and completed with Hiroshi Takeda, led to prediction of the atomic co-ordinates from cell dimensions and composition. Such a result was made possible by ruthless schematization of the geometrical constraints imposed on the mica structure. To a first approximation the important feature was the near-coplanarity of the basal oxygen atoms. In a second approximation Takeda, who is pursuing the problem and is reporting on it at this meeting, has improved the model by analyzing the effect of the tetrahedral distortion.

The concept of *lattice complex*, due to P. Niggli, was elaborated by Carl Hermann. It was my privilege to join the group of the latter's followers, headed by Professor E. Hellner at Marburg, and with them to complete the *Tables of Lattice Complexes* for all 230 space groups, to be published by the National Bureau of Standards. The lattice complexes should help you master the intricacies of space-group symmetry and facilitate the description and classification of crystal structures, especially those with high symmetries, which are found among minerals.

Throughout my life, somehow, I nearly always found myself working on problems that were not the fashion of the day. Few members of my profession, especially in U.S.A., seemed to pay attention to what I was doing. I recall beginning a lecture, some ten years ago, by quoting a sentence I had picked up in one of the letters of Seneca¹ to Lucilius, "Topics that I know and can discuss, nobody cares to hear; subjects that would interest people, I know nothing about." This statement pretty well summed up my predicament. It perhaps also explains why Seneca developed his stoic brand of philosophy. Be that as it may, I still think a scientist should investigate the questions that haunt him, not those that might be forced upon him by official guidelines. And so I say to you, young researchers: Take an interest in as many fields as possible, help as many people as you can, but select your own problems. Enjoy your work. Never get discouraged. Money is getting scarce? What of it? "The passion for truth dulls acquisitiveness," says my former colleague S. S. Kuznets, 1971 Nobel laureate in Economics. Besides, austerity weeds out the operators! Your greatest reward will always be the bliss of solving your problem and seeing the pieces of the puzzle fall harmoniously into place. This reward, no-one can ever take away from you. Next to the joy of discovery comes that of recognition by your peers. This is the one Seneca had to do without. For giving it to me today, my fellow-mineralogists, please accept my deepest thanks.

¹ Numquam volui populo placere; nam quae ego scio non probat populus, quae probat populus ego nescio. (Ep. XXIX, 10)