

The Early History of the International Union of Crystallography*

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Madam President, Fellow Crystallographers:

It is a great pleasure for me to speak on this tenth anniversary on the gradual development of modern crystallography and, in particular, of the Union of Crystallography. As I am speaking from memory, maybe not every detail is quite correct, but I believe that essentially my presentation is true. What I would like to stress is that crystallography has had to fight a good deal to find a recognized place in the world of science. Probably every new development which expands as rapidly as the new crystallography did has to fight for its own niche. You all know that crystallography started in the cabinets of princes, in Italy and elsewhere, where rarities and freaks of nature, such as crystals, minerals, fossil bones and plants and antique objects like mummies were indiscriminately collected until the special nature of crystals was gradually recognized. Then the great step came with the invention of the goniometer and the quantitative description which it made possible. That is all an old story and well known.

With a big leap I come to the activities of Paul von Groth in Munich. He was Professor of Mineralogy and Crystallography. His interests were wide-spread, including mineral sites and even the evaluation of crystals for jewellers and in court cases. He had a great many pupils from all over the world who came to him first in Strassburg and later in Munich. This gave him extensive international connections. He founded the *Zeitschrift für Mineralogie und Kristallographie* which he edited for some 50 or 55 volumes until his retirement at the age of 78, when his eyes did no longer allow him to continue. He then handed the *Zeitschrift* over to Paul Niggli in Zürich, and two years later, on his eightieth birthday, the editorship of the *Zeitschrift* was enlarged by appointing co-editors: Laue and myself for physically and Fajans for chemically oriented papers. Groth's ideal had been to create a journal for crystallography only, including the physical and chemical aspects, irrespective of whether the crystals occurred in nature or were grown in a laboratory. But for financial reasons he thought that he could not omit mineralogy in the title because crystallography was not a subject represented by a chair and an institute of its own. I am afraid that this is still the case in many universities, and even that it might become worse with the re-organization of the universities.

The *Zeitschrift* was run by Groth with the help of

his two assistants and occasional graduate coworkers. It contained a section of abstracts of papers published anywhere and the aim was to make these so informative that for the subscriber to the *Zeitschrift* there was no need to look up the originals in order to find out whether he would be interested in their details. Niggli continued these abstracts while he was sole editor, but it soon turned out that his and his assistant's time was fully consumed by this work. Groth used the abstracts in compiling his five volumes of *Chemische Kristallographie*, a fundamental book which even now is still being consulted. His assistants Gossner and Steinmetz were driven hard in repeating measurements which Groth doubted to be correct and this added a critical side to the mere abstracting. Crystallography's 'big bang' came in 1912 with the experiment of Laue, Friedrich and Knipping and the first structure determinations by the Braggs. Since then the number of crystallographic papers has risen exponentially. When I became co-editor of the *Zeitschrift*, I felt that the abstracts should be restricted to papers containing structure determinations. This became the origin of the *Strukturbericht*. We also decided to 'internationalize' the *Zeitschrift* by publishing papers in English and French, instead of translating them, as Groth had done.

As the subject developed, books began to appear or be in preparation. My own book *Kristalle und Röntgenstrahlen*, which was mentioned in Professor Casimir's welcoming talk the other day, appeared in 1923, and in the same year Charles Mauguin's book came out. There was an inherent difficulty for the authors in writing such a book: if it was to be useful for the determination of crystal structures, it had to contain tabulations of space groups, of equivalent coordinates (like in R. W. G. Wyckoff's *Analytical Results of the Theory of Space Groups*), of wavelengths, absorption coefficients, etc. As each author compiled his own tables, using his own arrangement, nomenclature, symbols and values of physical quantities, great confusion was inevitable – quite apart from the many factual mistakes that crept into the tables. It became necessary to produce a standard set of tables to which any future author could refer. J. D. Bernal and I, with the encouragement of Sir William Bragg and M. von Laue, began to prepare a meeting for the planning of such standard tables. This meeting took place in Zürich in 1930 and we brought together Wyckoff and Pauling, Astbury, Mauguin, Schiebold and others from Germany and Niggli and Brandenberger from Zürich. It was decided to prepare tables of space groups, of symmetry relations, and of mathe-

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mathematical and physical data useful in actual crystal structure analysis. The nomenclature of space groups which was there adopted was a combination of Carl Hermann's and Mauguin's systems and the illustrations were those that Mauguin had used in his classes. These changes from the Schoenflies notation brought the nomenclature much closer to the requirements of crystal analysis by X-ray diffraction. The preparation of the Tables was distributed among those volunteering, and Carl Hermann, with his unsurpassed knowledge of space groups, was appointed editor. The tables appeared in 1935; they are the precursor of the modern *International Tables for Crystallography*.

After the first volume of *Strukturbericht* had been completed by Hermann and me, the work was continued by others until it was interrupted by the outbreak of World War II.

The scientific standing of the *Zeitschrift* was excellent, but not so its financial success. Originally it had belonged to a publisher Engelmann. He broke down during the German inflation and the *Zeitschrift* was acquired by an enterprising young firm, the Akademische Verlagsgesellschaft. The head of that firm agreed to most of the changes the Editor proposed, and we could not complain. On the other hand, the journal was the property of the firm and the editors had no influence on the financial side. Prices were rising steadily during the inflation, and that was a handicap for German publishers. But worse than that, the *Zeitschrift*, like other German journals, was billed per volume without limiting it to a definite number of volumes per year. The rapid growth of physics and crystallography in the thirties led to an increase in the number of volumes per year, and the result was that the librarians in countries with stable finances could not plan their budgets and declined to subscribe to German journals. The circulation of the *Zeitschrift* therefore remained very limited.

This unsatisfactory situation was one main reason for my stressing the desirability of creating a corporation so that the crystallographers themselves could own an international journal in which to publish their work. This idea took shape when I was invited to give a talk to the X-ray Analysis Group at a meeting in Oxford in March 1944. I there laid out a program that we should establish an International Union of Crystallography – in analogy with the Unions of Physics, Chemistry, Astronomy and Biology. The next year, Bragg arranged a small preparatory meeting in London. Among other points of discussion, we tried to find a new name for the subject of the Union. Bragg had something in mind like Atomic Architecture; I mentioned the name the German crystallographer Rinne had coined, namely Leptology. Luckily, we did not fall for that, for if you look up that word in a Greek dictionary, you find that it means 'talking windy stuff'. Bragg and the British group became very active corresponding with their American counterparts and in 1946 Bragg called the conference in London which was

mentioned by Dorothy Hodgkin in her introduction to this session. This conference became a fabulous meeting of crystallographers, the first after the separation during the World War and friendships were renewed and many newly formed. The results of the conference were the following: Firstly it was decided to create a separate Union of Crystallography. The existing International Union of Pure and Applied Physics (IUPAP) would have liked to keep crystallography as one of its major committees, and since I happened to be the General Secretary of that Union, I found myself with a divided loyalty. But since the relations of the structurally interested workers were at least as close to chemistry as to physics, the vote was carried for an independent Union.

The second decision was that a new Journal should be created and run for and by the crystallographers. In the discussion on that point the American delegates firmly stressed that in this journal American spelling would be applied to American papers. This condition gave rise to some need for diplomacy, especially when, some time later, negotiations took place with the Cambridge University Press for the production of the journal. The manager said: 'this is quite impossible; we have hardly trained our compositors to correct English spelling, and now you want us to order them to mis-spell!' We insisted, however, and told them this was not their responsibility and that their compositors should just stick to the manuscript. So they finally gave in.

The third resolution was to continue *Strukturbericht* as *Structure Reports*. Arthur Wilson undertook to organize and edit these for many years before he became Editor of *Acta*.

A Provisional Committee was formed from among the delegates to which Evans and I could report on the progress we made in further organizing the union. The International Council of Scientific Unions (ICSU) is a kind of head organization for coordinating the interests and work of the various unions and we wished to be accepted as one of that group. So Robert Evans in Cambridge and I, at the time in Belfast, had to draw up statutes for submitting to ICSU. We had a common friend, F. J. M. Stratton, the astronomer, who was the Secretary General of ICSU. He was most encouraging and of great help, being convinced that more useful work was being done by 'small' unions than by the 'large' unions which covered too wide a field.

Evans and I also prepared a crystallographic text which we sent to various printing establishments in France, England, Sweden, Denmark, the Netherlands and Austria, asking them to print it as they would see fit and to inform us on their prices. We obtained many answers; some very attractive, some cheaper than others. It was a time of paper rationing, labour unrests and non-stabilized valuta. The decision required some political forecasting. We finally decided to ask the Cambridge University Press to take over production. Evans decided on the type-font for legibility and

conciseness and specified the design and colour of the cover. The former is essentially still in use today. Evans also assumed the task of Technical Editor and kept close touch with the Press, since his office was across the street from the premises of the Press. He was a very accurate proof reader, and you will find hardly a misprint in the first volumes of *Acta*. On the other hand, the first volume is not without fault. I had saved a paper by Garrido for the first issue as its first paper. Two years later W. A. Wooster showed that it was all wrong, namely conflicting with the symmetry of the crystal. A good editor should have noticed that!

I shall not say much about *Structure Reports*, except that it was, and is, a formidable task and that great advances have been made in recent years to get it up to date. This has become clear from the report given in the first meeting of this year's Assembly.

Coming back to the beginnings of our Union, we had to have its constituent assembly. The Provisional Committee had received the invitation to have this meeting at Harvard. So from 28th July to 3rd August 1948, we had another beautiful meeting with a great many old and new friends. We were all so keen to see and hear colleagues from whom we had been separated for years. For many it was the first visit across the Atlantic. I was the chairman of the inaugural meeting, and Robert Evans the secretary. I well remember my difficulty of getting the meeting started because, naive as I was, and not trained in the British formalities and rules of meetings, I just began: 'Well, we are going to form a Union of Crystallography and the first point seems to require that we confirm by a vote this intention'. In the front row there sat some American hecklers who asked 'Who is going to vote'. I looked at about two hundred crystallographers and said: 'You, the Assembly'. Then from the front row: 'Who belongs to the Assembly? By what authority can they vote?' and so it went on. I was really in a pinch. Evans somehow rescued me, and from then on the proceedings went smoothly. It was decided to approve the Statutes and By-Laws, to hold Assemblies every three years, to confirm the editorial and other arrangements made for *Acta* and *Structure Reports*, to form several Commissions, etc. The invitation to hold the second Assembly in Stockholm in 1951 was transmitted by Gunnar Hägg and gladly accepted. In short, we were in business.

After this, the Union grew by leaps and bounds, and so did the number of papers submitted to *Acta*. At the Stockholm Assembly a resolution was carried, to my amazement, to fix the price of *Acta* for the next three years to the prevailing price – I believe it was \$10 – for the yearly volume. I was aware that in view of the unsettled prices of paper and labour, and the increasing pressure of war-time research papers being made ready for publication, this advance fixing of the subscription price would lead to financial unbalance. But I acquiesced in the thought that we had not made

full use of the funds collected for the starting of the Union. At the third meeting, in Paris 1954 Evans (the Treasurer) and I got heavily scolded for transgressing the financial capability of the Union. The way it was done very nearly broke the back of the Union. I was, however, convinced that the number of subscribers would eventually increase sufficiently to balance the finances because chemical laboratories became more and more interested in *Acta*. No second attempt was made, at any of the following Assemblies, of fixing the subscription price for three years in advance. What you pay for is the amount of information you receive in the volume, and the information density in *Acta* has been as high as in the best-edited journals.

I sometimes wonder what crystallography and its development would have been without the Union. At the time of the Bragg conference in London, there existed two societies in the USA, a Crystallographic Society and the Society for X-ray and Electron Diffraction. Each was planning a journal for accepting crystal-structure analyses. In Europe, each country had its mineralogical society and journal which accepted structural work, though only reluctantly on non-minerals. This might easily have led to a multiplicity of places where the crystallographer would have to search for papers, even fundamental ones. The Union never attempted a monopoly on crystallographic publication; its aim was a consolidation where theoretical and experimental, mathematical, physical and chemical work could be critically presented and discussed, and references to work appearing elsewhere would be found. This consolidation has been achieved. With the revision and enlargement of the *International Tables* by a committee of the Union consistency has been preserved in the data used in the many laboratories throughout the world. Research like that on the accuracy of intensity measurements and its bearing on the determination of atomic scattering factors requires an international organization – and there are a number of other committees of the Union which do very valuable work on an international scale. I conclude that it is good to have the Union.

When in 1946 I went to the Paris Assembly of the Union of Physics I travelled together with my friend Charles Darwin. This Union had developed only a very restricted activity in the 24 years since its formation in 1922, largely due to political difficulties. Darwin discussed whether it would not be just as good to let the Union expire. There are, he said, anyway too many national and international meetings which keep you away from your work and reduce your efficiency. He was rather defeatist in that respect – and luckily he was wrong. I do not feel that the Union of Crystallography is a distraction, nor would I say that of the Union of Physics after the development it has taken. So may I conclude this survey by wishing the Union, that is each one of you, a *Vivat, Crescat, Floreat!*