



Paul P. Ewald heads the department of physics at the Polytechnic Institute of Brooklyn and is editor of *Acta Crystallographica*, the international journal of crystallography. He came to the United States four years ago from Ireland, where he had served as professor of mathematical physics at the Queen's University in Belfast.

Some personal experiences in of CRYSTAL

By P. P. Ewald

An article based on Professor Ewald's address as Retiring President of the American Crystallographic Association at its meeting in Ann Arbor, Michigan, June 24, 1953.

X-RAY CRYSTALLOGRAPHY, like any good crystallization, grew from a few distinct nuclei. The first nucleus was the Laue-Friedrich-Knipping experiment in Munich. Hardly had the news of this new effect been given at the spring 1912 meeting of the Bavarian Academy of Science and found its way into the papers, before a second nucleation was induced in England. While Laue had explained the effect as one of diffraction of very short light waves by the regular lattice arrangement of scattering atoms, W. L. Bragg concluded from the shape of the Laue spots that they should be explained as an effect of reflection of waves on the internal atomic planes, an idea that led him at once to what is now known as Bragg's Law. Thus it was the focusing property which gave the first clue to the Bragg version of the phenomenon, as published in the *Proceedings of the Cambridge Philosophical Society* in November 1912.

Soon after this W. H. Bragg applied this principle in the construction of the x-ray spectrometer, an instrument which led to the fundamental discovery of the *K* and *L* series of characteristic line spectra as distinct from the continuous "white" spectrum of the general Bremsstrahlung. With this discovery the wide field of x-ray spectroscopy was opened up precisely in time to give convincing support to the Bohr theory of the atom in its first infancy and subsequently throughout the stages of increasing refinement and complexity. The second wide field opened up by W. H. Bragg's discovery was x-ray crystal analysis, for which the characteristic wavelengths provided the yardstick for measuring the distances between atoms or atomic net planes in a crystal. The use of this yardstick was, however, only obtainable by first determining a crystal structure without its application. This was achieved by W. L. Bragg by comparing the Laue pictures of NaCl, KCl, KBr, and KI; the changes produced in replacing a lighter

atom by a heavier one of greater reflecting power led to the confirmation of the "spatial chessboard" structure which had been postulated for these salts by Barlow and Pope. Once the relative arrangement of the atoms was known, the absolute scale of their distances followed from the density of the crystal.

What exciting years were these last pre-war years 1912 and 1913. They belong to those periods of eruptive development that occur when an entirely new impact hits and unites fields of science which for many years had not yielded to the most strenuous external pressures. This had been the case with x-rays prior to 1912, with optical spectroscopy and with the interpretation of the first quantum phenomena in the theories of radiation and of the photo-electric effect. In these same years a revival of interest in the theory of the solid state took place; in Born-Kármán's paper on specific heat (1912) the first application of quantization to the lattice model of solids was made, and shortly after that, in 1915, appeared Born's *Dynamik der Kristallgitter* which marks the nucleation of the modern theory of solids. Immediate as the impact of the new discoveries was on physics, it was a delayed one for chemistry. The fact that in simple inorganic salts the concept of a molecule should no longer hold did not please the chemists. Ephraim's book on inorganic chemistry was, as far as I am aware, the first textbook fully to accept this fact, but it did not appear until 1921. Progress in x-ray diffraction came from many European countries in those early years. Maurice de Broglie in Paris was quick in developing his own spectroscopic methods and in training co-workers like Trillat and Thibaut. Some members of this audience may remember the unique setup of his laboratory in his private *hôtel* in the rue Lord Byron where cables for the current came in by holes cut in the Gobelin's adorning the walls. In Holland Lorentz developed the Lorentz-factor in his lectures and Debye,

the international coordination

DIFFRACTOMETRY

at that time in Utrecht, ventured to tackle the theory of diffraction by a lattice in thermal vibration—a problem which appeared superhuman to anyone but a Debye. In England Moseley made the first systematic survey of the *K*- and *L*-series throughout the periodic system and Darwin discussed the absolute intensity of x-ray reflection by setting up the first dynamical theory far ahead of all others; in order to account for the difference between the theoretically expected, and the observed intensities, he developed the idea of the mosaic crystal which proved indispensable for all later work. The crystallographer G. Wulff in Russia showed the advantages of crystallographic projection techniques; Nishikawa obtained the first fibre diagrams and Terada, also in Tokyo, was the first to observe the sudden appearance and disappearance of the diffracted spectra on the fluorescent screen. Remember that all this happened at a time when the identity of the Bragg reflection and the Laue diffraction theories was not yet generally understood.

It is hard nowadays, especially for the younger among you who have been taught x-ray diffraction in a well organized university course, to imagine how crystal analysis then appeared to those engaged in it. It may be illustrated by a post card I received from W. L. Bragg on which he wrote that he had measured the spectra of pyrites and had been trying to obtain the structure. "*But it is terribly complicated*", he wrote. It was the first example of a cubic crystal in which the trigonal axes do not intersect.

THE WAR of 1914–18 brought not only the interruption of international relations, it even brought the actual x-ray diffraction studies very nearly to a standstill. The application of these studies to chemical and technical problems had not yet been discovered. Only one advance of great importance was made in 1917, and that independently in Göttingen and in Schenectady by Debye and Scherer and by A. W. Hull, respectively. While all previous measurements required fairly large well-formed single crystals, which were not always easy to obtain, the powder method was applicable to practically all solid substances. I first heard of this method from the crystallographer A. Johnsen, then professor in Kiel, and keeper of a fine collection of minerals. His words, which I remember well, are significant for the enthusiasm with which the powder method was acclaimed: Who would still want to take single crystal pictures, painstakingly adjusted and hard to index? We just powder our crystals in a mortar and

get the powder lines to fit into a quadratic form and that gives us all the information.

In the period after 1918 the retarded development flared up afresh. The two Bragg schools, at the Royal Institution and in Manchester, were the leading centers for structure analysis and for the training of the next generation of physicists in this art. They were also an international meeting ground of crystallographers. The spectrometer remained for a long time the main instrument. Apart from giving a direct indication of the strength of reflection, it offered the great convenience of showing exactly from what plane the reflection came, an advantage that was lost in the rotation diagrams of Polanyi (1921) and only regained in the Weissenberg x-ray goniometer method (1924). The deciphering of the experimental data was achieved by frontal assault in each case. A "normal" decline of intensity with increasing order of reflection was established by W. H. Bragg and the deviations from the normal sequence were attributed to the halvings or similar subdivisions of the sets of reflecting planes and later to the structure factor. The usefulness of space group theory in providing a framework for the atomic positions was stressed by Niggli in his book (1919), but given the practical test by Wyckoff, together with his numerous co-workers, in determining a great number of structures with the help of his *Analytical Expression of the Results of the Theory of Space Groups* which appeared in 1922. The first English adaptation of the theory of space groups followed in 1924 (Astbury and Yardley).

About the same time the first books on the new subject appeared. The book by the Braggs, *X-Rays and Crystal Structure*, had already been published in 1915; it gave mainly a coordinated account of their own investigations and is still a fundamental book, unique for the simplicity of its reasoning and the beauty of its style. It gave the direction to the English school of x-ray workers, but it was never meant, at the early date of its publication, to present more than the line of thought that had achieved the great first results. It was often reprinted but never expanded or revised.

My own book, *Kristalle und Röntgenstrahlen* (1923), represented the continental point of view and aimed at integration of the advances in the methods of x-ray diffraction and at discussing the implications of the results. It was sold out in two years and I never prepared a second edition, partly because the subject was growing so rapidly, but mainly because the two editions of the chapter I wrote for the *Handbuch der Physik* (1926 and 1933) gave a more succinct and modern presentation of the same matter.

Similar monographs on the subject were written in France by Ch. Mauguin (1924) and in the U.S.A. by Wyckoff (*The Structure of Crystals*, 1924). Together these three early books document in detail the advances made up to about 1923 regarding the methods of producing and indexing diffraction photographs and of using the structure factor for obtaining the atomic arrangement. Significantly neither Wyckoff's nor my book contains a main chapter on the intensity of diffraction; in spite of Lorentz', Darwin's, and Debye's work too little was known about it. Mauguin deals more fully with intensity.

MEANWHILE the x-ray crystallographers were becoming more ambitious. The first structures that had been determined, like NaCl, diamond, zinc blende and wurtzite, had been without a parameter; the atoms could not be moved away from the intersection of symmetry elements in the cell without admitting too many atoms to the cell. In pyrites and calcite, structures with one parameter had been solved by discussing the intensity sequence among the various orders of reflection of a face. It was still fairly easy to extend the methods of discussion to the case of two, and, in rare cases, of three parameters. But you could not set out to determine the structure of any given crystal, because in most cases it was likely to contain a large number of parameters. The purely optical principles of discussion then broke down. At this stage the idea of fixed atomic radii was introduced by W. L. Bragg and his school and eagerly expanded and modified by V. M. Goldschmidt and others. Nowadays it is a valid and much employed principle which is firmly based on a large body of experience. It appeared a risky principle in the mid-twenties and one would have liked first firmly to establish it on a large number of structures which had been determined without its use. This gave a special meaning to the collection of structure data which C. Hermann and I assembled as Vol. 1 of the *Strukturberichte*. In reporting the structure determinations we tried here to separate clearly the optical arguments, which seemed safe, from any doubtful additional hypotheses. Wyckoff followed the same line in his critical surveys of structures which were published in 1924, 1931, and 1935. This purist tendency has been dropped deliberately from the recent revival of the *Strukturberichte*, the Structure Reports.

In spite of auxiliary assumptions derived from atomic radii and structural chemistry the full structural analysis of crystals containing three or more parameters remained at best a hazardous undertaking. All problems seemed to end up in a sigh: if only we had a reliable means of measuring and evaluating intensities and of deriving from them the structure factors! It is true that in 1914 Darwin had given two expressions for the intensity of an x-ray reflected by the external face of a crystal, assuming either a perfect or a mosaic crystal. These expressions gave widely different values, and the measured intensities did not seem to fit either assumption too well. Besides, Darwin's papers were not well

written and were not properly understood. His restriction to the specular reflection from the net planes gave no indication as to what became of the cross-grating spectra which each of these planes would give owing to its own atomic periodicity. This was one of the reasons which prevented me from appreciating Darwin's work, and it was only after having set up my own dynamical theory of x-ray diffraction that I discovered that some of my results for a perfect crystal were identical with those of Darwin. Experimentally Bragg and James and Bosanquet showed in 1921 that the intensity of reflection depends largely on the treatment given to the reflecting surface of the crystal, such as grinding, polishing, etching.

By 1925 it had become apparent that the whole future of x-ray crystal analysis was at stake unless a solution to the intensity problem could be found. I learned that Wyckoff was coming to Europe and it occurred to me that this would offer an excellent occasion for having a joint discussion of all those who had worked on the intensity problem. After some correspondence I found a date in August, 1925, which was acceptable to nearly the whole group of experts and I arranged for five days of discussion at my mother's house in the country at Holzhausen on the Ammersee, some 25 miles from Munich. The little local inn was rented, a blackboard was borrowed from the nearest country school and a few boxes of cigars placed on the table in my mother's big studio (she was a painter). Then the exciting moments came of meeting my colleagues at the nearest railway station at which they, fortunately, all arrived on schedule. Remember that by 1925 the international relations had not yet been resumed on any large scale and that this was for most of us the first post-war meeting of an international character. Those present were, if I remember correctly, W. L. Bragg, Darwin and James from England; Wyckoff from U.S.A.; Brillouin from France; Fokker from Holland; Waller from Sweden; Laue, Mark, Ott, Herzfeld, and myself from Germany. Occasional visitors and participants were Debye and Fajans. Waller had just published his dissertation; the first part of this was a review and extension of the dynamical theory and the second an extension of Debye's work on the temperature factor. It was a very learned paper and required many years of development to be fully evaluated in its implications for the discussion of experimental results.

I think all of us enjoyed these full days of intense discussion in which Darwin finally got so entangled between his own papers of 1914 and 1922 that he promised to restate them and where Bragg declared emphatically at the end of one session: I will not be satisfied unless I can determine a structure with 19 parameters! This seemed utterly fantastic at that time, and yet, two or three years later, he was tackling the silicates and polytungstanates and was just about as far as he had wished. The Holzhausen conference was an important event in the history of x-ray crystallography. It coordinated at a critical time the various approaches, experimental and theoretical, British and Continental,

i.e. reflection versus diffraction. It further made scientists meet after the war, many of them for the first time, and laid, I am happy to say, the foundations for a lasting personal friendship and respect. In doing so, it also paved the way for two of the future international enterprises in crystallography, the *Strukturberichte* and the *International Tables*. It stimulated experimental and theoretical work in the problems discussed at the meeting as is shown by a number of papers in the subsequent years. But the credit for overcoming the formidable deadlock of the intensity problem goes to W. L. Bragg who returned to Manchester to tackle it in a most systematic and realistic way. He first established a standard of intensity in the 400 reflections of a properly prepared rock salt face; together with James and Darwin he restated the results of the latter's theory in a *Phil. Mag.* paper of 1926; James, with Miss Firth, Brindley, and others, made a thorough experimental study of the temperature effect using high and low temperatures; Waller came to Manchester to help on the theoretical side. As a result of fundamental importance for all parts of physics the first direct confirmation of the zero point energy of an oscillator, here the crystal proper vibrations, was obtained. Hartree, then also at Manchester, tackled the remaining unknown intensity factor, the atomic factor, first on the Bohr orbit atomic model, and, after the advent of wave-mechanics in 1926, by the method of self-consistent field. Bragg, in 1927, reported on the atomic factor derived from the Thomas statistical model of the atom.

This may give an idea of the concerted effort which finally overcame the deadlock.

BY THIS TIME advantage was taken, also by the English workers, of the theory of space groups. Bernal came to see me in Stuttgart in 1925 carrying along a voluminous manuscript in which he had derived the 230 space groups in his own way. The problem was how to publish this work. As happened not infrequently to Bernal, the manuscript was interspersed with folding tables densely covered with symbols in order to accommodate all information on them. He had devised his own symbols for the space groups and it was all Greek to me. I well remember the three of us, Bernal, myself, and Carl Hermann sitting alongside on a sofa and the maps being unfolded on my knees. It took Hermann a split second to understand the tables, including the strange terminology, and to suggest various points of rearrangement of the tables in order better to bring out some of the subgroup relations which Bernal's arrangement did not show. The battle between them was fought out on my knees, and it took close to an hour to go through the tables.

Several new books had been published or were being written, such as Mark's book *Die Verwendung der Röntgenstrahlen in Chemie und Technik* (1926), Mark and Rosbaud's *Space Group Tables*, my own *Handbuch* article (1926), and a book by Schleede and Schneider which was being planned. Besides, the older books needed new editions by 1928. It was a matter of some

concern to me, and I am sure to the other authors also, how to get around the dilemma either of having to include detailed tables and illustrations of the 230 space groups, or of writing a book that lacked practical value for the actual determination of crystal structures. Besides, there were proposals by Rinne and Schiebold, by Hermann, by Mauguin, and others for changing the nomenclature of the space groups so as to make it more descriptive than the Schoenflies symbols. A multiplicity of symbols for the same space group was going to create considerable confusion in an already sufficiently complex subject. The only way I saw out of this confused situation was the preparation of a set of impersonal tables containing a complete and standard description of each space group, a work to which each author could refer in his own textbook and from which he could pick examples on which to discuss the ideas of space group theory, without being obliged to bring a complete set of tables. I discussed this idea with Bernal and Mrs. Lonsdale at the occasion of a meeting of the Faraday Society in London, 1929, and together we laid it before Sir William Bragg who gave us every encouragement and promise of help and convened a meeting of a large group of crystallographers gathered for the Faraday Society, where this plan and others were discussed. Bernal and I undertook to prepare a detailed syllabus of such tables. We hit upon all kinds of difficulties, partly because decisions had to be taken concerning the symmetry axes of the second kind, the fixing of origins, the graphical representation of symmetry elements and of equivalent points, etc., and partly for reasons of a more personal nature because people in different laboratories and countries had become used to symbols and drawings which did not please those accustomed to others. A conference was the only way to thrash out these points, and, again taking advantage of a European trip of Wyckoff, Bernal and I prepared a meeting for July 1930. On the invitation of Niggli it was held at his institute. I took the chair at the four-day meeting, and we worked quite hard all the time. Those present were Wyckoff from the U.S.A., Bernal, Astbury and Mrs. Lonsdale from England, Mauguin from France, Niggli and Brandenberger from Switzerland, Kolkmeijer from Holland and myself, Hermann, Schiebold and Schneider from Germany. The questionnaire Bernal and I had circulated together with the invitation gave a lead to the discussions and some of the points were quickly settled. Hermann's notation was recognized to offer great advantages, and some simplifications which Mauguin proposed were accepted; Schiebold, somewhat reluctantly, refrained from pressing for the acceptance of his system. A rather lengthy skirmish took place over the graphical representations. The English were accustomed to the Astbury-Yardley diagrams, Niggli to those in his book to which most others were not partial. Mauguin circulated a neat set of cards which he used in his course showing the cell and a group of equivalent atoms for each space group but leaving out the indication of symmetry ele-

ments. It was finally agreed to give two figures for each space group, one showing the equivalent points in Mauguin's way, the other the symmetry elements in a modified form of Niggli's representation. Preference for taking the origin at a center of symmetry whenever possible, and of using rotation-inversion rather than rotation-reflection axes was soon agreed upon. Wyckoff's symbols for special positions were adopted and so was the product form for the structure factors, as given in Mrs. Lonsdale's Tables. It was further agreed to list the sub-groups for every group.

The discussion on the third day was on the second volume which deals with mathematical and physical tables. The details of the tables of trigonometrical functions were laid out in a form convenient for the calculation of structure factors; other trigonometric tables, useful for the calculation of absorption and other corrections were planned. The listing of absorption coefficients and of atomic factors, and many other details, were discussed along with the distribution of the work between the laboratories. It was also agreed to offer the Tables for publication to the publisher of Niggli's book.

In the preface of the *Internationale Tabellen* you will find details of the work supplied by the various groups, and a list of the Learned Societies whose generous subsidies made possible the publication of the work at a very reasonable price. The Tables appeared in 1935 and it has always made me happy that they were universally accepted and fulfilled the hopes in which they were conceived.

THE NEXT international enterprise for which I worked was the foundation of an international organization of crystallographers. It began in 1944 when I was asked by the X-Ray Analysis Group to give an evening lecture at the March meeting in Oxford. The second part of this talk was the plea for the formation of an International Union of Crystallography. This plea was published in *Nature* (154, 628, 1944). It sets out, as the main task of the Union:

- (1) the publication of an international journal of crystallography;
- (2) the establishment of archives for the storing of scientific results which would be too costly to publish in full;
- (3) the abstracting, summarizing, and recording of crystallographic work, in particular in connection with the planned general scientific abstracting scheme;
- (4) the preparation of a second edition of the *International Tables*, and their public ownership;
- (5) the preparation or coordination of analytical tables for identifying crystals (Barker index, card index of powder lines);
- (6) the representation of crystallography in the system of other international scientific unions.

The ball set rolling in Oxford was played in great style by W. L. Bragg who arranged an international congress of crystallographers in London in 1946. This was actually the second international congress, the first having been held in 1934 under the auspices of the

Union of Physics when Sir William Bragg was its president. It is unnecessary to recall the events in London which ended with the resolutions to found a Union, preferably an independent Union of Crystallography and, if this were not accepted by the International Council of Scientific Unions (ICSU), to form a group within the Union of Physics; further to start at once with the preparations for an international crystallographic journal, for the resumption of *Strukturberichte* in a new form, and for a new edition of *Internationale Tabellen*. The discussions of the committees nominated for these tasks began without delay while the foreign visitors were still about. In fact, the Russian delegation which arrived a day after the congress had closed, was just in time to take part in the discussions on the journal which took place a few days later in Cambridge. It is thanks to them that *Acta Crystallographica* carries its name.

The actual birthday of the International Union of Crystallography was the hot 3rd of August, 1948, at the Union's first Assembly at Harvard. It was the culmination of a long sustained effort of preparations, including ocean crossings and oceans of correspondence on the part of a great number of crystallographers. Everything was set for the Union to crystallize out at this meeting. Then an unforeseen inhibition occurred. The provisional executive committee had proposed to change the first draft of the Statutes of the Union in some points regarding the voting power of the delegates. The new formulation had to be accepted before the Statutes could be passed en bloc. So the changes had to be voted on, especially since there were some objections. Somebody raised the question: on what voting power is this going to be decided? Neither the first draft nor its amendment were binding, since neither had been accepted. Arguments for voting on the old or the new scheme went on in a freakish way. Finally the heat, I guess, must have concentrated the solute sufficiently, so that the inhibition was overcome and the Union at last crystallized out by the adoption of the revised statutes en bloc.

LOOKING BACK to 1946 and 1948 we may ask ourselves whether the foundation of the International Union of Crystallography was worth while. To answer this question we have not only to study what the Union has achieved, but also where we would be without it. Its main achievements are the journal *Acta Crystallographica*, the two, and soon four, volumes of *Structure Reports*, and the first volume of the new version of *International Tables*; besides, there are the two international Congresses and Assemblies—Harvard 1948 and Stockholm 1951—to which the third Assembly in Paris 1954 will be added next year. Furthermore, there is active work going on by correspondence in the commissions of the Union, as on Powder Data, on Apparatus and Standardization, and on Nomenclature. Within the system of International Scientific Unions the Union of Crystallography belongs to the small Unions, small by the number of adhering countries, by its

financial demands, and by the limited importance of its international program which is not as vital in crystallography as it is in astronomy, geodesy and geophysics, or radio science, and not as extended as in chemistry and biological sciences. But as a small Union it has earned respect and acknowledgment by the determined effort to achieve internationally important results in the field of publication and standardization. Had the Union failed to materialize it is most likely that by now we would have three full-fledged crystallographic journals, in the States, in England, and in Germany. Each of these journals would be indispensable because each would contain important papers. There would be three editorial, and, worse, three publishers' policies regarding the scope and length of the papers, the yearly published volume, and the price. It is unlikely that private publishers would have received the generous subsidies on which *Acta Crystallographica* was started. In the first five years *Acta* has received altogether \$17 400 from Unesco and from industry. These subsidies have helped over the first few years which are a critical time for a journal. Thanks to this help we have been able to accommodate an ever increasing influx of papers. The number of pages published has risen in the last three years by 78 percent, the production cost per page by 7.8 percent. The number of subscribers has been increasing steadily, at a rate of about 50 more subscriptions every year, and this is a healthy sign. Unfortunately, however, this rate is far too slow to offset the increase in cost of production. It means that at present *Acta* is adding to the "red" in the Union's books a deficit of about \$10 000 per year. We are thus still in the midst of the teething troubles of our five-year-old baby.

It is not unnatural that this should happen. When the journal was started, it was done on the understanding that the price per volume be \$10 and that the balance between production cost and sales be met by the subsidies. The \$10 price was maintained for the first three volumes, then at the Stockholm Assembly the price was raised by 50 percent to \$15, but meanwhile the volume had increased by 200 percent against the first volume. Now you might ask: is it necessary to publish that many papers? A moment's consideration will show you that it is a natural development. An increasing number of people trained in crystal analysis produces more and better papers every year; the advances in x-ray diffraction technique alone, and again in computational technique, allow an increased output of structure determinations, and the ever closer connection with chemical, biochemical, metallurgical, and physical investigation presents the diffractionist with problems of considerable interest in nearly overwhelming numbers. If the journal of the International Union of Crystallography is to tie together all this diverse diffraction work and be the forum for its adequate discussion, then we cannot afford to turn down good manuscripts because we are getting too many of them for a strictly limited volume. For the last few weeks I have felt very unhappy in following

this course after having received strict orders from the Executive Committee at our Paris meeting not to exceed last year's volume.

What then can be done with *Acta*? We have now some 1100 subscribers, that is double the highest figure ever obtained for the *Zeitschrift für Kristallographie*. This number is considerably below the saturation value, which I estimate at 2000. There are many university and industrial laboratories without *Acta*, in spite of their interest in the solid state. Many big establishments take only a single copy in spite of demand in different localities. There are also many among you who do not yet take advantage of the reduced price at which you can get your own copy to study at your leisure at home. *The Physical Review* is received by nearly every one of the 10 000 members of the APS and he pays for it in his membership fee. If we were similarly to arrange a general distribution of *Acta* to the 700 ACA members, of whom about 100 already take *Acta* at the reduced rate of \$9, this would bring in one-half of the yearly deficit. But so far the ACA Council has not taken to this proposal.

There are two ways out of this rather desperate situation: One is to collect further subsidies, preferably guaranteed over a number of years, and to continue running the journal at a loss. The private subscriber may like this proposal because he is getting the benefit of the subsidy. But it puts the journal on an unsafe basis and endangers its financial independence. The other way is to increase the price of subscription so that the journal is self-sustaining. With the present volume and number of subscribers this point would be reached by raising the subscription rate from \$15 (\$9) to \$24 (\$15). Some income could also be gained by carrying advertisements, but this is not considerable. A page charge, while acceptable in the U.S.A., appears unacceptable to the European scientists. Further income will be necessary later for following the natural development, i.e. increasing the volume beyond the present 870 pages; this will have to be met by a further substantial increase in the number of subscribers.

I think it is important to explain this situation to a group such as is assembled here. We should not take the existence of scientific journals for granted. Each one of us should fight for their existence and make sacrifices, not only by saving space in his own publications by the utmost condensation, but also by subscribing and getting others to subscribe. Only by a deliberate and concerted effort can *Acta*, and also the two other publications of the International Union of Crystallography be brought over the inevitable difficulties of the first ten years. The gain these publications bring to the large fellowship of crystallographers all over the world seems to me to justify the existence of the Union and to make it worth while not to relax in sustaining its activities. We may, I think, be proud of what has been achieved so far and it seems unthinkable that the International Union of Crystallography should not be able to keep pace with the development of crystallography itself.