

INTERNATIONAL UNION OF CRYSTALLOGRAPHY

IUCr Newsletter

Volume 18 · Issue 2

Collected archival edition

Original article text and available illustrations

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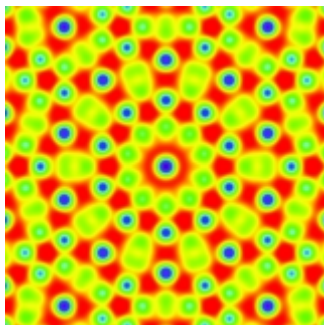
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IUCR JOURNALS NEWS

Short-range order diffuse scattering in quasicrystals

***Acta Cryst.* (2010). A66, 372–383 (doi.org/10.1107/S0108767310004885)**



The diffuse scattering intensity of the phason flip in the Penrose pattern, which is calculated by the application of a derived diffuse scattering intensity formula.

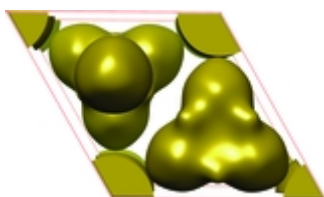
The short-range order diffuse scattering often observed in alloys is also observed in quasicrystals. A newly proposed theory enables us to analyze short-range order atom or atom cluster fluctuations in quasicrystals. The theory uses a higher-dimensional description of quasicrystals. The intensity formula is, however, quite similar to that for conventional crystals and provides a method of determining the short-range order parameters or so-called α parameters for quasicrystals.

A. Yamamoto

IUCR JOURNALS NEWS

Maximum entropy method and charge flipping, a powerful combination to visualize the true nature of structural disorder from in situ X-ray powder diffraction data

***Acta Cryst.* (2010). B66, 184-195 (doi.org/10.1107/S0108768109052616)**

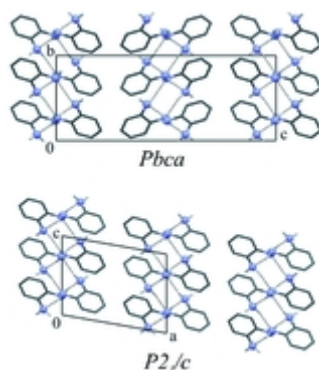


Completely *ab initio* electron-density distributions have been obtained from X-ray powder diffraction data by the maximum entropy method (MEM) applied to the combination of structure-factor amplitudes from Le Bail fits with phases derived from charge flipping with histogram matching. This approach bears the potential of a fast method of electron-density determination without the need for a structure model, and will be especially useful for highly disordered materials. The figure shows such an *ab initio* density of the disordered high-temperature phase of Rb₂C₂O₄.

A. Samy, R. E. Dinnebier, S. van Smaalen and M. Jansen

A new polymorph of benzene-1,2-diamine: isomorphism with 2-aminophenol and two-dimensional isostructurality of polymorphs

***Acta Cryst.* (2010). C66, o198-o201 (doi.org/10.1107/S0108270110008474)**



Two-dimensional isostructurality of the two benzene-1,2-diamine polymorphs.

A new orthorhombic polymorph of benzene-1,2-diamine shows two-dimensional isostructurality with the well-known monoclinic form of this compound. Both polymorphs are constructed from bilayers of the hydrogen-bonded molecules that have the polar amino groups directed to the inside and the aromatic groups, showing a herringbone arrangement, directed to the outside. Moreover, the new form of benzene-1,2-diamine is isomorphous with the only known form of 2-aminophenol.

A. Czapik and M. Gdaniec

IUCR JOURNALS NEWS

Detection and correction of underassigned rotational symmetry prior to structure deposition

***Acta Cryst.* (2010). D66, 503–513 (doi.org/10.1107/S0907444910001502)**



About 2% of X-ray structures in the Protein Data Bank fit into higher symmetry space groups. The extra symmetry can be imposed on these structures with very minimal atomic movements, bringing redundant subunits thought to be related by non-crystallographic symmetry into perfect alignment. Experimental success may rely on using the highest possible symmetry constraints for marginal cases where the interesting structural features are barely detectable. Atomic models can now be readily interconverted between groups and subgroups with the new validation program *labelit.check_pdb_symmetry*.

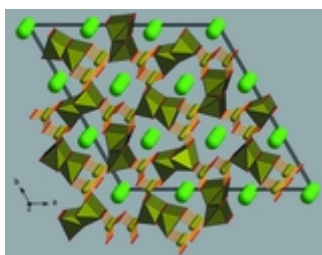
B. K. Poon, R. W. Grosse-Kunstleve, P. H. Zwart and N. K. Sauter

IUCR JOURNALS NEWS

The trigonal polymorph of strontium tetraborate, β -SrB₄O₇

The trigonal polymorph of strontium tetraborate, β -SrB₄O₇

***Acta Cryst.* (2010). E66, i48 (doi.org/10.1107/S1600536810019069)**



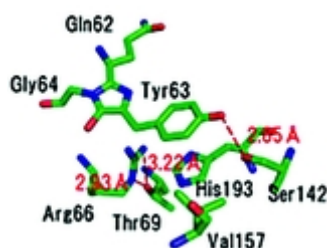
The framework of linked BO₃ triangles and BO₄ tetrahedra with channels filled with the Sr²⁺ ions. The trigonal phase of strontium tetraborate, β -SrB₄O₇, arises as the metastable crystal and possesses a greater structural correspondence with a glass in the process of its crystallization. The proportion of the four-coordinated boron ions is 50% in this phase while this proportion in a glass is 30–50% and in the stable rhombohedral phase, α -SrB₄O₇, is 100%. Crystals of the β -phase have noncentrosymmetric structure and show interesting nonlinear optical properties.

A.D. Vasiliev, A.V. Cherepakhin and A.I. Zaitsev

IUCR JOURNALS NEWS

The structure of mAG, a monomeric mutant of the green fluorescent protein Azami-Green, reveals the structural basis of its stable green emission

***Acta Cryst.* (2010). F66, 485-489 (doi.org/10.1107/S1744309110011127)**



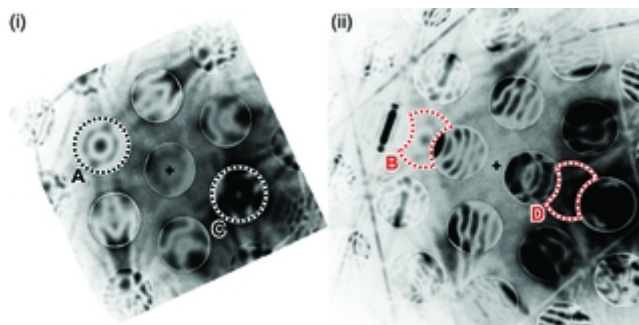
The unique QYG chromophore and the surrounding residues of mAG.

Azami-Green (AG) from the stony coral *Galaxea fascicularis* is a monomeric green-emitting fluorescent protein that is not a variant of *Aequorea victoria* green fluorescent protein (avGFP). The crystal structure of mAG has been determined at 2.2 Å resolution, which reveals the structural basis of the stable green emission of mAG. The conformation of the QYG chromophore, formed by Gln62-Tyr63-Gly64, is fixed by hydrogen bonds and van der Waals contacts with the surrounding residues. Particularly, His193, Arg66, and Thr69 contribute to the fixed conformation of the chromophore.

T. Ebisawa, A. Yamamura, Y. Kameda, K. Hayakawa, K. Nagata and M. Tanokura

Differential quantitative analysis of background structure in energy-filtered convergent-beam electron diffraction patterns

J. Appl. Cryst. (2010). 43, 280-284 (doi.org/10.1107/S0021889810000749)



Two 0 ± 3 eV energy-loss-filtered CBED patterns, (i) and (ii), from the same thickness of single crystal corundum near $\langle 001 \rangle$. A and B are at equivalent locations relative to the zone (crosses), as are C and D. The background oscillations (B and D) mimic the oscillations within the reflections (A and C). Energy filters have significantly advanced the quantitative interpretation of electron diffraction patterns by elastic scattering theory. They are, however, incapable of removing electrons involved in phonon excitation and those scattered elastically by specimen surface imperfections. Thus, a background in zero loss energy-filtered convergent beam electron diffraction (CBED) patterns remains. It is observed that this background can be separated into diffuse and oscillatory components, the latter mimicking the elastic distribution. A differential treatment of CBED patterns leads to a more complete analysis by elastic scattering theory.

P. N. H. Nakashima and B. C. Muddle

Application of magnetic Compton scattering for spin-specific magnetic hysteresis measurement

J. Synchrotron Rad. (2010). 17, 321-324 (doi.org/10.1107/S090904951000292X)

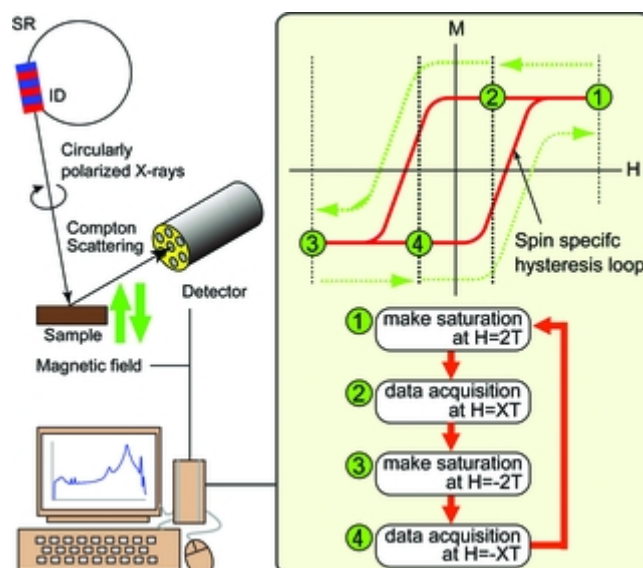


Illustration of the experimental set-up and procedure for measurement.

Magnetic Compton scattering is sensitive to the spin magnetic moment of a magnetically active electron. Applying the induced magnetic field dependence of magnetic Compton intensity, a method to obtain the spin-specific magnetization hysteresis loop has been developed as a new tool to investigate microscopic origin of macroscopic magnetization. Its validity was demonstrated on TbCo amorphous film and it was shown that this method has sufficient sensitivity to investigate spin magnetization, even of a thin film. We expect this method will be used for studying magnetic materials.

A. Agui, H. Sakurai, T. Tamura, T. Kurachi, M. Tanaka, H. Adachi and H. Kawata

SPECIAL REPORT

Crystallography in Canada

continued from Volume 17, Number 4

(This is an extended version of the article that appeared in the printed newsletter)



Lachlan Cranswick was the driving force responsible for generating the Canadian content of these issues. He was a strong believer in the preservation of the history of the development of crystallography and crystallographic computing. He now is a prominent part of the history of Crystallography in Canada.

Early days of protein crystallography in Canada

Crystallographic research in Canada dates back to the 1960s when a number of centres began investigations into various protein structures. At the NRC in Ottawa there was a strong contingent of small molecule crystallographers led by W. F. Barnes. This group included Farid Ahmed, a mathematician trained in D. W. J. Cruickshank's laboratory, Al Hanson and Maria Przybylska, amongst others. Indeed, David Phillips of lysozyme fame worked at the NRC from 1958 to 1960. David developed the spot shape correction equations for upper-level Weissenberg photographs while he was at the NRC. Maria Przybylska embarked on a major protein crystallography project, that of the complex between hemoglobin and haptoglobin in the early 1960s; unfortunately, the structure was not solved for many years.

The University of Toronto was also a hub of small molecule crystallography, with Stan Nyberg in the Chemistry Department and Norman Camerman in the Biochemistry Department. Theo Hofmann, in the Biochemistry Department at the University of Toronto, isolated an aspartic proteinase from the fungus *Penicillium janthinellum*. He named it penicillopepsin. Theo characterized penicillopepsin biochemically and kinetically and he recognized the importance of structural data in order to bring these facts together and to understand the catalytic mechanism. Theo grew the first crystals of penicillopepsin and began a collaboration with Stan Nyberg on the structure. Data collection was performed in Nyberg's laboratory on precession photographs and a postdoctoral fellow, I-Nan Hsu, carried out that initial work. The work was subsequently transferred to the University of Alberta in the mid 1970's.

The other major crystallographic centre in Canada of the time was in the Chemistry Department at UBC. Jim Trotter, an organic chemist and a small molecule crystallographer, headed up the lab; he was trained in Scotland with J. M. Robertson. Jim's main interests remained in the field of small molecule studies, as opposed to protein crystallographic studies.

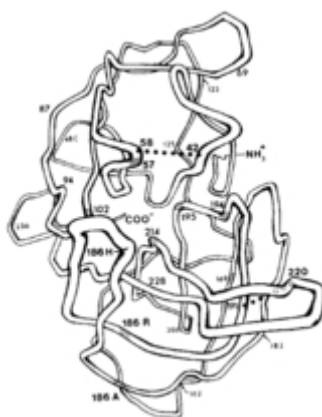
In addition to Cambridge's Laboratory of Molecular Biology, Dorothy Hodgkin's laboratory in the University of Oxford was also a Mecca for those wanting to learn the methods of protein crystallography. Two Manitobans were privileged to study with Dorothy in the 1960's, Carol Saunderson and myself. I arrived in Oxford in 1963 just as Carol was graduating from Oxford with her DPhil. Carol returned to Canada and went to the NRC labs in Ottawa, where she eventually became involved in protein crystallography and specialized in the cathepsins, a group of lysosomal proteinases that resembled the plant enzyme papain.

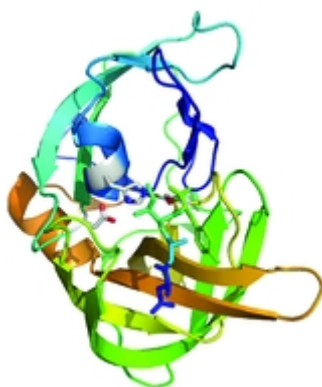
Although I did not work on the insulin project while I was at Oxford, I did catch the bug to work on the structures of proteins. This occurred in 1965, when I traveled with two fellow researchers from Dorothy's group, Ken Watson and Tony Cooper, to London to attend the unveiling of the lysozyme structure at the prestigious Royal Institution. David Phillips headed the team of researchers that solved this structure at 2.0 Å resolution. Included in the Phillips' team were Tony North, Colin Blake and Louise Johnson. This really was an inspirational experience, to see the structure of hen egg white lysozyme, the first enzyme to have its structure determined. In addition, this research inspired many others to work on the catalytic mechanism of an enzyme that had a three-dimensional structure available. I was determined to work on the structures of enzymes from that day forward.

In December of 1966, I returned to Canada and joined Dave Hall in the Chemistry Department at the University of Alberta. Dave was a small molecule transition metal crystallographer who was starting one of the first crystallographic laboratories in Alberta. Also present in the Chemistry Department was Ray Lemieux, one of the world's leading carbohydrate chemists. I had the good fortune to work with Ray as a postdoctoral fellow in Dave's laboratory on structures of small monosaccharides with a variety of halo-substituents on the C1 atom of the pyranose ring.

In late 1967, I received a phone call from Larry Smillie and Cyril Kay, both in the Biochemistry Department at the University of Alberta. Cyril and Larry wanted to start a protein crystallography group to assist in structural studies of molecules in which they were interested. Having obtained my name from Manpower Canada under the job category of 'crystallographer', Larry and Cyril invited me to join the Department of Biochemistry, provided I received a scholarship from the Medical Research Council of Canada (MRC) to support my research and salary. After being successful with this MRC application, I started my forty-year long career in the Department of Biochemistry in July 1968.

Larry Smillie provided me with a highly purified sample of α -lytic protease, an enzyme from the soil bacterium *Lysobacter enzymogenes*. We managed to crystallize it in a trigonal space group and with my first postdoctoral fellow, Michael Joynson, who had done his DPhil at Oxford with Louise Johnson, we set about to determining its three-dimensional structure. This was an interesting and exciting project because Larry had just finished determining the amino-acid sequence of the protein and had shown that it was closely related to the mammalian enzyme α -chymotrypsin. In order to show the evolutionary relationship between these enzymes, it was important to determine that it had a similar tertiary structure. Eventually we showed that it did!





SGPB in 2007

The first protein structure determined in Canada was done in my laboratory in Alberta. SGPB is the B protease, a proteolytic enzyme from the soil bacterium *Streptomyces griseus*, and closely related in sequence to α -lytic protease. The paper describing the molecular details of the 2.8 Å resolution structure was published in *Nature* in 1975. SGPB had the same disposition of aspartate, histidine and serine residues as did α -chymotrypsin. It also had the two beta-barrels that constituted the major fold of the molecule. SGPB and α -chymotrypsin were clearly related evolutionarily, even though one was from a cow and the other from a bacterium. Other proteolytic enzymes from *Streptomyces griseus* that our group solved were SGPA and the trypsin-like enzyme, SGT. SGPA constituted the major part of the work that Gary Brayer did for his PhD thesis and Randy Read did the work on SGT for part of his PhD thesis research. Many other people played major roles in the early stages of our protein structural studies: Penny Coddington, Wendy Hutcheon, Masao Fujinaga, Louis Delbaere, Bill Thiessen and the biochemists who isolated and purified the original enzyme preps, Lubo Jurasek, Larry Smillie and Peter Johnson.

The structure of penicillopepsin, determined by I-Nan Hsu in collaboration with Theo Hofmann's group at the University of Toronto in 1976, produced a great deal of excitement because it brought with it the structural data that would aid in interpreting the catalytic mechanism of this family of enzymes. The aspartic proteinases are so-named because they have two aspartic acid residues in an amino-acid triplet of Asp-Thr(or Ser)-Gly at the active site. This family is medically important because two of the members are kidney renin, an aspartic proteinase that is critical in the regulation of blood pressure, and HIV proteinase, an enzyme that is crucial for the replication and 'life' cycle of the HIV virus. Because of the medical importance of these two enzymes, they rapidly became key targets for the development of inhibitors, in the regulation of blood pressure and as antiviral drugs. These were certainly very 'heady' days in the field of structure-based drug design and many of those suffering today from AIDS have benefited tremendously from this research.

Other leading protein crystallographers in the Department of Biochemistry at that time include my friends and colleagues Bob Fletterick and Wayne Anderson. When the MRC Group in Protein Structure and Function was formed in 1975 we hired a second crystallographer, Bob Fletterick. Bob had done a postdoctoral fellowship with Tom Steitz at Yale, where he worked on the structure of hexokinase. When Bob moved to Edmonton he worked closely with Neil Madsen on the structure of rabbit muscle phosphorylase *a*. Bob worked diligently on the solution of this structure and collected a data set every day (remember this was before the days of flash-cooled crystals) in order to get suitable heavy-atom substituted crystals to solve the MIR phase problem. Two very capable postdoctoral fellows helped Bob in this task, Jurgen Sygush and Steve Sprang. Together this team solved the structure of phosphorylase *a* and used the structure to interpret many of the details of the catalytic mechanism. The sugar binding sites for the glycogen molecule were visualized and the pyridoxal phosphate bound to lysine was identified. This was a momentous achievement in these early days of protein crystallography.

In 1979 Bob left for San Francisco, where he set up a crystallographic laboratory that is still highly productive. Jurgen Sygush went to the Université de Sherbrooke and set up his laboratory there in 1978.

In 1979, Steve Sprang joined Bob in San Francisco. Eventually, Steve established a Howard Hughes structural biology unit at the University of Texas Southwest Medical School.

Wayne Anderson, who also graduated from Tom Steitz' laboratory in Yale, came to the MRC group after a postdoctoral stint in Brian Matthews' lab at the University of Oregon in Eugene. Wayne brought a different interest to the Department. He was part of the team that solved the structure of the DNA binding protein CRO. This structure revealed a paradigm for protein-DNA interactions, that of an α -helix binding in the major groove of the DNA double helix. Interestingly, this binding mode had been predicted by Gordon Dixon, a biochemist at the University of Calgary. Wayne's interests were many and varied. He worked on Fab fragments of monoclonal antibodies that had been raised against DNA. This was an excellent model for the autoimmune disease Lupus erythematosus. With Wayne's help, we were successful in purchasing one of the first multiwire detectors from Ron Hamlin in San Diego. This was a huge advance in the data collection speed in Alberta and we used it to tremendous advantage. Wayne was at Alberta for nine years before moving on to Vanderbilt University in Tennessee. Wayne and Bob both left tremendous legacies at Alberta and we owe them a great debt for their teaching and their research.

The 40 years that I have been a member of the Department of Biochemistry have been truly rewarding. I owe a great debt to all of those who have supported me in my various projects of structural biology. I want to thank Michelle James for typing this manuscript. I have been fortunate to have received funding from a variety of agencies over the years, the MRC of Canada, CIHR, AHFMR, the Canada Research Chairs program and CFI among others.

Michael James

Eric Gabe



Eric Gabe (1957)

I graduated from Cardiff University with a BSc in 1957 and went on to postgraduate work in crystal structure analysis under the direction of D. F. Grant, obtaining a PhD in 1960.

During that time computers were becoming available and it was possible to attempt three-dimensional analysis of medium-sized organic compounds for the first time, though it was a very tedious process. There were no integrated systems for structure analysis and it involved the use of several different computers. Thus, the output from a structure-factor calculation on one computer was incompatible with calculating a Fourier map on another computer, and would require manual re-typing of the required paper-tape input. Time on the computers, which were in different cities, always seemed to be allotted to crystallographers during the night. At five in the morning, realising that the paper-tape going into one side of the reader was not emerging on the other, was not the happiest experience. Typically, the whole process might take a year.

I then moved to the Pure Physics Division of NRC in Ottawa as a postdoctoral fellow under W. H. Barnes, though I worked mainly with Farid Ahmed using programs which he had written for an IBM 650. After a brief stint in London using a Ferranti Mercury machine - from midnight to 6 am - I moved to A. L. Patterson's lab in Philadelphia where I wrote many programs for the in-lab IBM 1620, a rather strange machine which used a decimal structure rather than binary.

In 1966 I moved back to Ottawa and became involved in the automation of a Picker diffractometer in the Mines Branch labs of the Department of Energy, Mines and Resources, the first such machine in Canada.

This work used a DEC PDP 8 minicomputer and, because of its speed, I was able to use the otherwise idle computer time while the instrument was scanning a reflection to calculate absorption corrections by Gaussian quadrature. This led me to realise that such machines could be used for all aspects of structure analysis. In 1972 I moved back to the Chemistry Division at NRC and began work on what ultimately was to become the NRCVAX package. The first programs were written in machine code for the PDP 8e computer, the basic version of which had 4 K 12-bit words and a 1 microsecond cycle time. It was possible to add more memory, up to 32 K, at great cost, but the only auxiliary storage available was on magnetic DEC tape, which was very slow. Fortunately, in about 1969, DEC introduced the OS 8 operating system using a 1.6 mega-word disk and a Fortran compiler with an excellent overlay structure. In my opinion, this single-user operating system was far better than many later systems. Working in collaboration with D. F. Grant, A. C. Larson, Yu Wang and P. S. White, we were able to develop a complete Fortran package for data collection and analysis using the PDP-8e. Initially, floating-point operations used software emulators and calculations were rather slow. However, the introduction of floating-point hardware speeded things up enormously and it became possible to solve and refine a 20- to 40-atom structure in less than a working day. The only real restriction was caused by the size of the memory in full-matrix calculations and block-diagonal methods were used.

The diffractometer program *DIFRAC* migrated to control CAD-4 machines using PDP 11 computers and the analysis package moved to the PDP 1160. Memory size still imposed a limit on full-matrix least-squares and, in fact, was more severe on the PDP 11 than on the earlier and seemingly smaller machine, because of the less efficient overlay scheme. In the early 70s most structure analysis was done on large mainframes and many people had difficulty in believing that it could be done on in-lab minicomputers. I remember being told at a meeting that such things could not be done on a PDP 8!

When the system moved to the PDP 11 we encountered portability problems, with compiler inconsistencies and different input/output handling. Programs were modified accordingly, but we realised that the code needed to be made more portable. When the system migrated to the DEC VAX in the mid-70s (and to the increasingly available PCs using Windows), size restrictions were removed and the code itself was revised to use a safe subset of Fortran 77, sometimes referred to as 'pidgin' Fortran. Areas where compilers in different machines still caused problems were isolated to a small set of routines which could be controlled by system-specific parameters. These include graphics, input/output, Boolean and time/date routines. In this way potential problem code can be isolated and easily dealt with if the need arises. The infamous, and grossly overstated, Y2K problem was dealt with by changing one line of code. The NRCVAX system is not, and was never meant to be, a 'black box', though it can be used quite successfully with the built-in defaults. Users with crystallographic knowledge however, can use the many options available to solve and refine difficult structures.

Eric Gabe

Osvald Knop



Osvald Knop

One of the first to do crystallography in the Maritime Provinces was Osvald Knop (Ossie). Born in 1922, he arrived in Halifax in 1949 via Mazarek University in Czechoslovakia, Sweden, a spell at Cal Tech with Linus Pauling and Laval (DSc 1957). At Cal Tech, Ossie worked on intermetallic phases attempting to solve the three-dimensional structure of Co_2Na using a Hollerith Punch Card adaptation of Beevers-Lipson strips. A similar machine was installed in the basement of the Mathematics Inst. in Oxford. It was

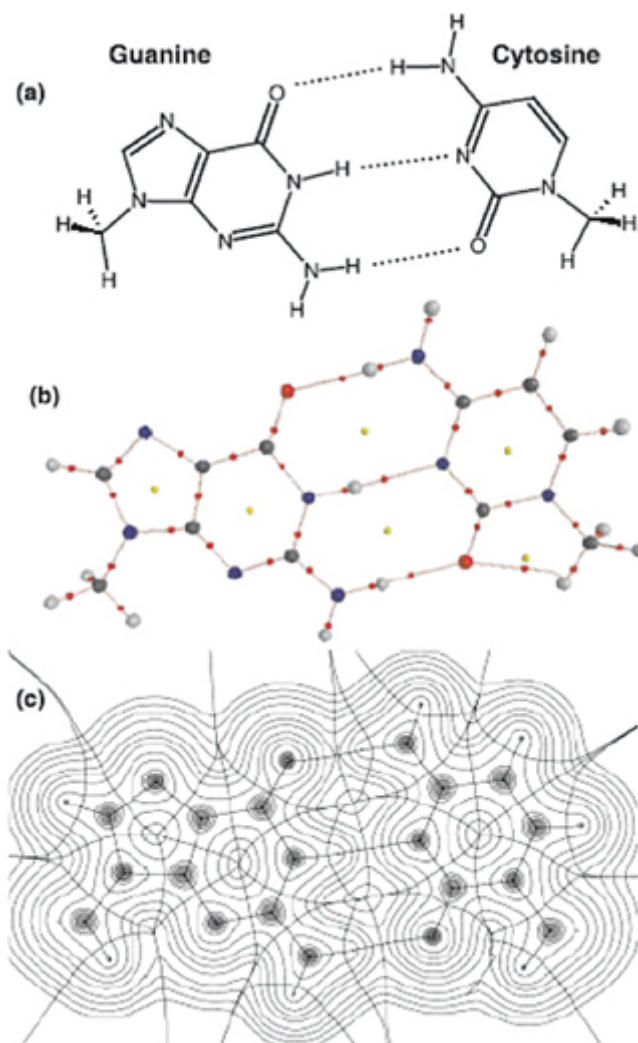
powered by a multi-horsepower diesel motor, which was started by a crank handle, and when it ran (night only) the whole building shook. In Halifax with Ossie there was also Karoline (Lottie) Castelliz, who received her PhD in Vienna in 1932.

In Halifax Ossie first worked on the three-dimensional structures of the lycopodium alkaloids, although once his work showed progress other groups with greater resources and deeper pockets took over. His other major interests were metal oxides, sulfides and intermetallic phases. The early equipment consisted of a Solus Schall generator - with a pre-war model plate - and a band-driven Weissenberg camera. The generator, still faintly working, departed to the dump only 10 years ago - the transformer oil containing no trace of PCBs. The camera still exists; those who remember this model will not be surprised to learn that it is now exhibited as a memorial to the misery of working with a badly designed instrument.

Ossie and his students produced many elegantly accurate phase equilibrium diagrams, some with neutron diffraction data (he was the first to use Chalk River for diffraction experiments). The three main furnaces for this work - constructed by Ken Reid - were called Faith, Hope and Charity and survived in working order for many years. Ossie's time in Halifax included 7 years at Nova Scotia Technical College and many years at Dalhousie. He retired in 1990, became an Emeritus Professor and still comes into the lab most days of the week.

T. Stanley Cameron

QTAIM: quantum theory of atoms in molecules



Three representations of the structure of the guanine-cytosine base pair: (a) classical structure; (b) the network of bond paths linking neighbouring atoms that defines the molecular graph; (c) contour map of the electron density, showing the bond paths and the intersections of the zero-flux interatomic surfaces with the plane of the diagram. The atomic symbols in (b) denote the positions of the nuclei: H (white), C (black), O (red), N (blue). The small yellow dots denote ring critical points, the red dots the positions of the bond critical points: the origin of the unique pair of trajectories of $\text{grad } \rho$, each of which terminates at a neighbouring nucleus, and the terminus for the set of trajectories that define the interatomic surface. There is a wealth of information in the molecular graph. Note that it provides a faithful mapping of the classical structure onto the topology of the density, showing in addition to the three hydrogen bonds a weak interaction between a methyl H and the keto O in cytosine that is not indicated in the classical structure. The presence of the bond path is but a useful way of depicting and summarizing which pairs of atoms share an interatomic surface, as demonstrated in (c). That this shorthand notation mimics the way in which the same information is conveyed by the structures that evolved from experimental chemistry is surely one of the most powerful of all the physical vindications of the zero-flux boundary condition for the definition of a quantum open system.

Science is based on observation. Among the most important of the quantities accessible to measurement is the distribution of charge - nuclear and electronic - that constitutes matter and determines its properties. We are indeed fortunate to live in an age wherein the accurate measurement of charge distribution became a reality, with each year witnessing an increase in our technical ability to determine its form and interpret its physical consequences.

The topology of the charge distribution is dominated by the electron-nuclear force, causing the electron density ρ to exhibit maxima at the nuclear positions, thereby imposing the atomic form on the structure of matter. The resulting topology of the electron density provides the physical basis for the partitioning of space into atomic regions. The connectivity of the atomic regions defined by the lines of maximum density linking neighbouring atoms - the bond paths - yield all of the structural concepts of chemistry: open, cyclic and cage structures. Changes in the topology of ρ occasioned by motions of the nuclei cause changes in structure, yielding a theory of both structure and structural stability.

The primary purpose in postulating the existence of atoms in molecules or crystals is a consequence of the observation that atoms or functional groupings of atoms exhibit characteristic sets of static, reactive and spectroscopic properties which in general vary between relatively narrow limits. Thus, the knowledge of chemistry is ordered, classified and understood by assigning properties to functional groupings of atoms, properties which are then used to identify the presence of a given group or to understand the behaviour of some total system. It follows that the topological definition of an atom in a molecule is of no physical substance unless it enables the extension of quantum mechanics to an atom in a molecule, enabling the definition of its properties. The necessary boundary condition for the extension of quantum mechanics to an open system - to an atom in a molecule - is a natural consequence of the topological definition of an atom as determined by the dominance of the nuclear-electron force. An atom is a region of space bounded by a surface not crossed by any gradient vectors of the density, a 'surface of zero-flux in $\text{grad } \rho$ '. The resulting theory is called the quantum theory of atoms in molecules, QTAIM. As is well documented, the atomic and group properties predicted by QTAIM agree with the additive group contributions measured experimentally, agreement with experiment being the only test of theory.

QTAIM, by providing the quantum basis for an atom in a molecule, necessarily recovers all of the related concepts of experimental chemistry. Thus, in addition to the definition of atoms and molecular structure, QTAIM provides the physical basis for the Lewis model and its associated chemical concepts. The pairing of electrons and the associated concepts of electron localization/delocalization, are determined by the atomic expectation value of the exchange density and given physical expression in the topology of the Laplacian of the electron density, the quantity $\nabla^2 \rho$.

Thus, the measurable electron density provides the link between the primary concepts of experimental chemistry and quantum mechanics, providing the prediction and understanding of the properties of matter at the atomic level. It is to be understood that every question capable of expression in terms of quantum observables - energy, force, pressure, current etc. - can be both asked and answered. QTAIM provides an entirely new way of asking and answering questions concerning structure and its relation to measured properties that previously were matters of intense debate, the very definition of structure - of bonds being present or absent - being a source of controversy. Now it only remains for the investigator of the density to ask proper and meaningful questions.

The acceptance by the experimental community of the topological theory of structure was not immediate. One could not find in the displays of the total density the accumulations of density corresponding to the bonded and lone pairs anticipated on the basis of the Lewis or orbital models. The field of accurate determination of ρ was instead dominated by the construction of density difference maps, $\Delta\rho$, obtained by subtracting the promolecule density ρ^0 from the 'true' density ρ determined by a modelling of the measured structure factors. An interatomic surface defining the common boundary of two bonded atoms cuts a 'bond' (as depicted in the $\Delta\rho$ maps) in two, making it difficult for some to accept that the primary structural unit is the atom, not the bond, and thus to turn the equation around to study $\rho = \Delta\rho + \rho^0$ rather than $\Delta\rho$.

The turning point in the acceptance of the topological theory of structure by X-ray crystallographers came with the demonstration that the topology of the Laplacian of the density provides a remarkable pictorial mapping of the Lewis electron pair concept onto real space, the bonded and lone pairs being defined in terms of concentrations of charge appearing as local maxima in $\nabla^2\rho$. This work was presented at a GRC on electron density and bonding in 1983. The topological properties of the Laplacian have been shown to be a direct consequence of the properties imposed by the Pauli principle on the pair density and on real space through the spatial localization/delocalization of the density of the Fermi hole. With the Laplacian providing a bridge between the old and the new, the topological theory of molecular structure rapidly gained acceptance to reach the dominant position it enjoys today amongst an ever growing audience of chemists and, in particular, X-ray crystallographers. An interested reader may view a video of a talk reviewing the development of QTAIM that I presented last January in the 'Frontiers in Chemistry Series' at Case Western Reserve University, to be found at: <http://www.chemistry.mcmaster.ca/faculty/bader/>

Richard F. W. Bader

Comments on QTAIM

The QTAIM approach to the examination of electron density has provided crystallographers with a remarkable tool to examine, experimentally, the fine details of the electron density throughout a crystal. Our group here at Dalhousie has been specializing in the collection of high-angle low-temperature multi-redundant X-ray diffraction data to examine the weak interactions present in crystals. In a series of papers, we are beginning to see that, although many of the C-H...X and even H...H interactions are weak, they are, with the insights from QTAIM, clearly detectable. Moreover, for many of these weak interactions it begins to appear that they are far more logically organized than was ever suspected. The result is that the species in crystals are often held in place not just by the familiar conventional forces but also by a multitude of Lilliputian tethers.

T. Stanley Cameron

A life in science



Andrew Booth working on a digital multiplier

A few words of introduction. I am the last surviving member of the group of X-ray crystallographers who worked in the Chemistry department at Birmingham University in the early 1940s. The Chairman was Sir Norman Haworth, who won the Nobel Prize for his determination of the molecular structure of ascorbic acid (vitamin C). X-ray crystallographic work was under the direction of E. Gordon Cox (later Sir Gordon), lecturer in Physical Chemistry.

I had studied medicine and theoretical physics at London and done an engineering 'graduate training' course at my father's aero engine shadow factory in Coventry. This was followed by several short-term wartime jobs. These included one to set up an X-ray inspection laboratory at the Armstrong Siddley Aero engine works in Coventry. I did not find industry very interesting and applied for an advertised Graduate Scholarship awarded by the British Rubber Producers Research Association (BRPRA) to work at Birmingham University on rubber structure. My father was of course livid, because accepting the Scholarship involved a reduction in income by a factor of six. The Wartime Scientific Assignment Board had to be consulted, and agreed with the additional constraints that I had to be OC Air Navigation at the Birmingham ATC training centre several evenings per week.

By the time I arrived at the lab the government had re-assigned the X-ray group to work on explosives, in my case pentrythritol tetranitrate (PETN), $C-(CH_2ONO_2)_4$, and a substance named RDX. I met with Cox for about an hour and was given a stack of paper. This included some of Cox's own work and Bernal's classic 'Chart' paper. I was told 'Find 26 errors in this!' Next, Cox led me to the actual X-ray lab. He located a large and filthy wooden box and said 'This contains an X-ray tube, get it going'. I did not see him again for 18 months, as he had moved to an administrative post at the Ministry of Supply.



Left to right back row: Williamson, Booth, Llewelyn; front row: Boyle, Claringbull, Small. From roof of Birmingham Chemistry Building, 1942.

The staff in charge of the unit were now Frank Llewellyn and George Claringbull. My immediate contact was George Jeffrey, whom most crystallographers will still remember. I soon assembled the contents of the wooden box. It was indeed a home-made X-ray system, but a previous student had contaminated it with mercury. The whole thing was assembled using APESO wax. I was told later that the unfortunate man responsible was kicked out. I was now introduced to the Phillips sealed-tube unit, which was the one actually used.

Next, there were training sessions on space-group determination using such simple crystals as oxalic acid, easy structures such as NaCl, and eventually a gathering of data for PETN. Once the diffraction films were made and analysed, the next operation was the determination of absolute intensities using the Lindeman electrometer, a most revolting and temperamental device.

Finally, the data were taken to the calculating room where, with the aid of models, one attempted to see how best the proposed structure could be fitted into the unit cell. This was a frustrating process for which one used Patterson syntheses and Beevers and Lipson strips, along with Brunsviga and Facit hand calculators. Although we had an assistant to do some of the routine work, I soon became frustrated and devised some mechanical aids. Fortunately I was able to get these made at the BRPRA workshop. The work now went more smoothly and the structure was soon finished. I was then diverted to other problems such as isotope separation and other wartime needs.

Before leaving the subject of our group, two remarks may be of interest. First, youthful desires. Each morning, Llewellyn and I used to take the same electric tram from central Birmingham to the University. There was a particularly beautiful lake on the approach drive, with a fine display of daffodils in springtime. As we passed the lake we would exchange philosophical thoughts. Llewellyn expressed an eternal love for Science and a determination to make it his life work. It is interesting to find that his first move was to a Chair in Chemistry in New Zealand and then to head the NZ Broadcasting Commission. This was followed by a move back to the UK, Directorship of the British Council, and a Knighthood. Claringbull, too, soon left crystallography to become Sir George, and Director of the Geological Museum in London.

The second observation is the behaviour of Professor Haworth. Each evening at 5pm he would wander round the labs and ask each research student 'What have you achieved today?' Next morning, soon after 9am, he repeated the process with 'What have you achieved since I last saw you?' I have found this a most useful technique.

With my PhD in hand my scholarship ended and I moved to the BRPRA laboratories near London. The Director, J. W. Wilson, proved to be one of the best people for whom I have worked. My interests were in improving the understanding of the X-ray analytical process and in improving the computational techniques. Wilson gave me a completely free hand and I worked 12 hour days, and also acted as Air Raid Warden and Fire Watcher. Meals were taken at the nearby 'British Restaurant', which provided good and inexpensive food.

I designed and put into construction a large mechanical structure-factor calculator and also a relay Fourier synthesiser. On the theoretical side, I started on the development of error and accuracy analyses for the results of current X-ray analyses.

The resulting papers from this work soon attracted some attention and I was offered a Nuffield Fellowship and Lectureship in Physics at Birkbeck College, London, under the leadership of Desmond Bernal. At that time, Bernal had no College space for his new 'Biomolecular' laboratory, so his initial team was housed in the Davy-Faraday laboratories at the Royal Institution (RI). As my practical program was proceeding at BRPRA I spent most of my daytime in the splendid library of the RI, writing my book 'Fourier Technique in X-ray Organic Structure Analysis' (Cambridge University Press, 1948), where references and details of most things mentioned above can be found. I also taught Optics and Theoretical Physics at the old Birkbeck building in Bream Street.



The Bernal team, 1948. Left to right, back: Aaron Klug, Jeffrey, Hirsh, Pit, Helena Scouludi; front: Anita Rimel, Ehrenberg, Desmond Bernal, Helen Megaw, Carlisle. Missing: Booth away in USA.

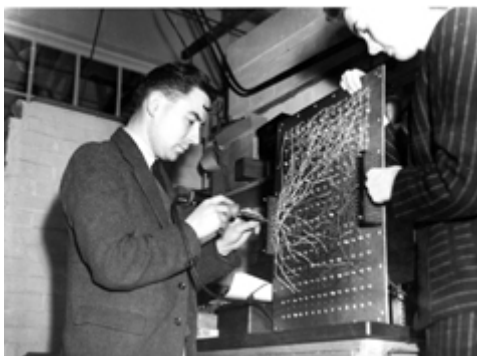
Following a visit to the USA in 1946 to attend the first ASXRED conference at Lake George, I visited most of the computing facilities and, on return to England, terminated the work on the mechanical Fourier machine at BRPRA and started on an all-electronic version. Meanwhile, I had been awarded a Rockefeller Fellowship, which I used to work with von Neumann at the Institute for Advanced Study in Princeton. While there I designed two general-purpose computers, the ARC and APEXC. The former was an all-parallel binary machine on the von Neumann plan, to form a test-bed for memory development, the second my own architecture and serial operation for X-ray calculations. Meanwhile, I was testing various forms of magnetic memory.



Kathleen Britten (left) and Xenia Sweeting (right).

On return to England my two assistants, Kathleen Britten and Xenia Sweeting, soon constructed the ARC and I had an operational magnetic drum memory (now in the Science Museum, South Kensington). Bernal now had his new laboratory building and his new Biomolecular lab was opened with myself as assistant Director for Computing.

Over the next few years, the APEXC design was given to International Computers Ltd. in exchange for generous financial support for my laboratory, which became the foundation of the world's first Department of Computer Science in 1953. It is still in existence.



Andrew Booth and Kathleen Britten working on the 1st decoder for their X-ray calculator.

My 17 years of work had resulted in the generation of some decent methods for calculating atomic coordinates from Fourier syntheses, together with methods to assess their accuracy. The computer work had generated a series of methods that it was hoped would, at least partially, automate the whole process for small molecules. Quite apart from this, I had initiated the whole field of computer-aided language translation, as well as the use of computers for the chronological dating of ancient and other texts. On the experimental side, research was in progress on ferro-electric materials and on thin-film magnetics.

I had made several attempts to get the University to create a permanent Chair in Computer Science, and even obtained a guarantee of funds from industrial friends to support it. It was rejected on the grounds that 'It is too soon to see if computer science will have a long term existence'! It must be said, however, that this may have been due to the malice of another academic at another university. I learned of this much later, along with evidence of his complete dishonesty.

In 1962, I decided to leave the Socialist mediocracy that Britain had become. When I let this be known I had some immediate offers, one from Western Reserve University, Cleveland, and another from the University of Saskatchewan. Western Reserve offered an 'Interdisciplinary Professorship in Autonetics' a title which the then Chancellor, Jack Millis, constructed from Greek roots as 'Doing what you like'. Saskatchewan, however, wanted someone who would modernize the College of Engineering and get it involved in research. I thought this very interesting and eventually accepted the Chair of Electrical Engineering, with the expectation of succeeding the then Dean of the Faculty when he retired in 1963. I kept the Cleveland offer on hold and accepted it in 1963 as a 'professor at large'.

The University of Saskatchewan was founded in 1909. The College of Engineering consisted of Departments of Agricultural, Civil, Mechanical, Chemical and Geological Engineering. The first year enrolment was about 1000, mostly male. None of the departmental Chairmen had doctoral degrees and there was practically no research.

In my first year I started a number of projects, including the design and construction of a fully transistorized version of the APEXC. With excellent financial assistance from the Canadian National Research Council and the Defence Board, this machine was completed within the year. By now I had discovered that no university in the Prairie Provinces had any interest in X-ray structure analysis so that, apart from some small investigations which I used as tests, the machine was used for biomedical and other data acquisition. I am happy to say that the machine operated without maintenance or error for over a decade.

I introduced student participation in college government long before the riots of the 1960s. Thus, when the troubles started, the engineers were a steadying influence on campus revolt. I also introduced structural changes: a Division of Hydrology and a Division of Biomedical Engineering, the latter arising from my early medical training. One result was that MD's could obtain an engineering degree in three years and engineers an MD in the same period. It is interesting to note that, in my time, several engineers obtained the MD, while no MD completed the course. My own contributions were the first

demonstration of cell damage due to microwaves, hyperbaric oxygen in wound treatment, laser eye surgery and open-heart surgery. By the time I left Saskatoon in 1972 I had a reasonably qualified staff and a graduate school enrolment of about 300. This was the fourth largest in Canada. On the side, our work on MT had continued under the supervision of Kathleen Booth and our system had been demonstrated to the Queen's Printer.

I should have been happy to stay at Saskatchewan until my retirement, but out of the blue came the offer of the Presidency of Lakehead University in Ontario. This was a new University with splendid buildings. Unfortunately, the first President had resigned in the face of student protest. My mandate here was to stabilize the faculty and students, to introduce research and, as I had not been told before I accepted the post, to balance the budget. I managed to do most of these things before I retired in 1978. The exception was research: at no time were there funds to make the necessary new appointments.

In the very limited spare time I had available I had kept in touch with the area of small computers, so, when a curious little device called the KIM became available, I acquired one. Visitors were amused to see the machine set up on a side table in my office. I also got one of the first TI60 programmable pocket calculators with associated printer unit. I spent a happy Sunday afternoon programming it to calculate and print all of the $F(h,k,l)$ values for PETN from the coordinates in our original paper. It was about ten times faster than the months of work in the 1940s.

On retirement in 1978, we moved to BC. Here, we set up a small Company and, with a grant from the National Research Council, designed and produced an energy-control device and software to economise on electrical domestic power use. We showed that our interface, used on the PET computer and tested over a 12-month period, could reduce the power bill by 30%. We had hoped to market this but had no sales acumen and failed, although we disposed of a number of units to various university and government departments as an interface from computers to the real world.

I also used the PET to automate a Fourier refinement of the original PETN coordinates. The program was written in BASIC and the results cleared up several anomalies that we had detected in the original work. It also agreed with previous work at UBC.

I became an honorary scientist at the Institute of Ocean Sciences at Patricia Bay, Sidney, BC, and designed a correlation sonar to be used to determine sub-surface ocean currents. This was successfully constructed at IOS and has produced useful results. Other projects include computer recognition of marine animal sounds and the computer translation of ancient maps to modern format. Our main activity, however, has been a study of waves in the ocean, and particularly the transport of atmospheric CO_2 to the water for absorption. This has led to a number of interesting sidelines, although the work has now ceased because of a lack of government support for the institute.

Other contributions are in the fields of photometric evaluation of coal deposits, and the design and insertion of corneal implants. Finally, I designed and implemented a course for engineering students entitled 'Economics and Business Management for Engineers'. This was given for a number of years at the University of Victoria and resulted in a small book of the same name. The emphasis was on computer modelling and, among other things, one of the exercises for students was to design and use an extended version of the J. W. Forrester Club of Rome model. The interesting outcome is that disaster looms by 2025 even without global warming.

I am still very interested in mathematical crystallography and often return to several problems from my early days: uniqueness of the solution in the absence of phase information and the general problem of phase determination.

Andrew D. Booth

19th century basics spearhead 21st century progress: some autobiographical details



The team of ICPET (Institute for Chemical Process and Environmental Technology) and IRC (Institute for Research in Construction) crystallographers receives an NRC award for Research Excellence on April 28, 2008. Front: Isobel Davidson, Pierre Coulombe (NRC President), Pamela Whitfield. Back: Yvon Le Page, Patrick Mercier, Lyndon Mitchell. (Credit for photograph: National Research Council of Canada).

This is the '*MISSYM* story', as suggested by the Editor of this Newsletter issue. This very simple and small part of my work at NRC is now used to delegate so much quantum-modeling brainwork to computers that it may be worth consigning the sequence of concepts and events onto paper.

In the fall of 1976, I was a postdoc with Gabrielle (Gai) Donnay (Martin, 1989), a diffractionist with great reputation and a crystallography professor at the Geology Department of McGill University. Her husband, Joseph (José or JDH) Désiré Hubert Donnay (1902-1994, see Donnay, 1962; Le Page, 1994) was Emeritus. JDH was a legend of mineralogy, crystal morphology and twinning. They occupied facing desks in the same office. Together, they were a formidable team, essentially an encyclopedia of mineralogy, crystallography and diffraction. I had been assigned the task of teaching the yearly undergraduate crystallography course to students from physics, chemistry and geology departments, while the Donnays were away on and off, a couple of weeks at a time, during a sabbatical year. Under any circumstances, teaching such a spectrum of nearly 40 undergraduate and graduate McGill students would have been a daunting task for any recent PhD in Physics. Knowing that the lecture notes would be under the microscope of the Donnays each time they came back unannounced made things really daunting.

Maybe the very first theorem stated after definitions in any decent crystallography course is the Primitivity Theorem: a cell based on three shortest (meaning that none is shorter) non-coplanar lattice vectors is primitive. I tried to work out a simple proof for the lecture notes and failed. The theorem was printed in textbooks by Buerger, Friedel etc., but without proof. I knocked on JDH's door and asked what classroom proof he used for this theorem. Tall, straight, alert and friendly at 74, he was known to be the most knowledgeable living person for anything about geometrical crystallography. 'Aaah, celui-là ...', he said, hinting that something was ajar: even Bravais (1850), who had proved everything else, had not printed a proof for it, but was instead referring to a very complicated algebraic proof in a book printed twenty years earlier and that could not be found anymore. JDH knew of other proofs as well, but all complicated and not suitable for teaching. I was extra careful to prove everything else, including the existence of seven crystal systems and no more, a fact that is usually taken for granted. The students were great and interested. Nobody dropped from the course or flunked the final exam on both geometrical crystallography and X-ray diffraction, to the amazement of the Donnays, who actually checked that the exam books at the bottom of the graded stack were up to their specs for graduation.

My second postdoc was at the NRC's X-ray diffraction laboratory at the Chemistry Division in Ottawa, starting in 1977, working with Larry Calvert, Eric Gabe, Allen Larson and Yu Wang on the nuts and bolts of what would become the *NRCCAD* Fortran diffractometer program, the *NRCVAX* structure package and the *CRYSTMET* crystal structure database. The IUCr XIth Meeting in Warsaw in 1978 allowed two contributions, provided that one would be in the session on 'Crystallographic Teaching'. In addition to a poster on least-squares weights in accurate refinements, I contributed the short 1976 proof for the existence of no more than seven crystal systems as a second poster (Le Page, 1978). Little did I know that, combined with the primitivity theorem (see full story in Le Page, 1992a), this superficially pointless classroom proof was the first quatrain of a lifelong saga that is still developing today at the rate of about one paper each three to five years. Applications are still bearing the most stunning fruits nearly every day.

The most pressing problems we had to tackle with diffractometer automation were recognition of the metric symmetry of the cell, and derivation of crystal symmetry and its subsequent use in efficient data collection. The popular cell-reduction algorithm at that time was based on distinguishing 44 mutually exclusive cases for the matrix of dot products between edges of the Niggli cell (Niggli, 1928). It is summarized in *International Tables for X-ray Crystallography* (1969), Vol. I, pp. 530-535. In addition to having generated an abundant literature of errata about conditions and of typos, the very basis of that approach was questionable to my eyes. As Niggli cell reduction requires knowledge of the lattice symmetry because it involves tests on equality, my feeling was that it is then logically circular to use the Niggli cell to determine lattice symmetry. Mathematically mutually exclusive cases may no longer be mutually exclusive when experimental error steps in, creating all sorts of problems having to do with pseudo-symmetrical lattices or the ordering of logical tests. Something more robust was needed.

The Chemistry Division was a couple thousand feet from CISTI, the then recent scientific library for Canada. Although collections were created in 1974, they included excellent original editions of Bravais, Mallard, Voigt, Friedel, Wyckoff, Niggli etc. that are quite difficult to find elsewhere. A pristine copy of Seeber (1831) even existed in a locked room reserved for rare books. I could then see that JDH had not exaggerated: the algebraic proof of the primitivity theorem sprawled there over about fifty pages.

After studying Bravais (1850) and dusting off the 19th century vocabulary, I came up with the conclusion that, with **u** and **h**, respectively, designating a direct and a reciprocal vector from a primitive lattice, the pair of conditions {**u**

- $\mathbf{h} = 1 \text{ or } 2; \mathbf{u} \times \mathbf{h} = 0$ } was necessary and sufficient for lattice row [**u**] to be an even-order axis of metric symmetry. The only thing missing to transform this into a foolproof computer algorithm was a limit on the components of **u** and **h**. It can be proved that the magnitudes of the Miller indices of **u** and **h** cannot exceed 2 when referred to three shortest non-coplanar translations. Difficulties associated with experimental error were avoided: any set of three short translations could be used with no angular condition. Extension to detection of lattice pseudosymmetry was straightforward. Calling δ the angle between **u** and **h**, the case $\delta = 0$ corresponds to symmetry, while the case $\delta < \epsilon$, where ϵ is a small limiting value, corresponds to pseudosymmetry because a small affine transformation can then create lattice symmetry from pseudosymmetry. The number of detected even-order axes is sufficient for crystal-system assignment, except for orthorhombic, which has three mutually perpendicular twofold symmetry axes, while rhombohedral has three coplanar ones, keeping complexity of branching to a very minimum. The lattice type is read off the values 1 or 2 of **h**
- **u** for the metric symmetry axes. This became the *CREDUC* cell-reduction algorithm at the heart of *NRCCAD*, *NRCVAX* and the MDF checking software, and more recently *CRYSTMET* and *Materials Toolkit*. It has worked perfectly and never required any tweaking since the first day it implemented the necessary and sufficient pair of conditions above and identified one case in each crystal system. The work was presented at IUCr XII in Ottawa (Le Page, 1981) and published in Le Page (1982).

The next step was *MISSYM*, which is meant to identify the symmetry of a structure model within a given distance tolerance. It is difficult to realize the significance of *MISSYM* for crystallography, now that the dust around R. E. (Dick) Marsh's campaign in the 1980s has mostly settled. Dick has the incredible talent to 'see' symmetry upon mere inspection of atom coordinates. He was very thorough in his re-analyses, which involved re-typing deposited structure factors, as they were in printout form at the time. Some of his re-analyses are crystallographic gems, demonstrating an extraordinary crystal-chemical sense. Dick's intentions were to raise the level of awareness and the crystallographic culture of *Acta C* contributors, or at least its editorial team. Results were not entirely up to expectations. Years after initially beating the drum about the matter, awareness had risen to 100%, but Dick's batting average remained unchanged all along at about one 'marshed' structure per *Acta C* issue. In spite of scientifically impeccable work and laudable intentions, Dick Marsh's campaign had the unintended adverse effect of creating waves across the whole body of science that were starting to bring the science of crystallography into disrepute. As crystal symmetry must re-establish the lattice within a fraction of a lattice translation, the rotation part **R** of a crystal-symmetry generator is therefore a lattice-symmetry operation. The *CREDUC* algorithm spells out Miller indices for the directions of the elements of metric symmetry in the reference system of the three shortest translations. All numerical expressions for **R** are then known in Cartesian axes. Extension of *CREDUC* to the determination of the symmetry of a model then only involves solving the equation $\mathbf{x}' = \mathbf{R}$

- $\mathbf{x} + \mathbf{T}$ for **T** on one pair of atoms with same nature and known Cartesian coordinates **x** and **x'**.
Checking that those values of **R** and **T** transform each atom in the list into an atom with the same nature and also in the list within the distance tolerance then establishes the existence of symmetry in the structure model. This is the essence of the *MISSYM* algorithm, presented at IUCr XIV in Perth (Le Page, 1987a) and published as Le Page (1987b), and of its *MISSYM 1.1* version (Le Page, 1988), where defaults were tailored in the light of the large number of cases exposed by Marsh up to that date. Applied widely to past *Acta C* issues, *MISSYM 1.1* flagged from defaults all structures that had been previously marshed, as well as a few genuinely pseudosymmetrical structures, but nothing else, as presented at IUCr XV in Bordeaux (Le Page, 1990). NRC gave the IUCr a licence for the use of *MISSYM* for its own not-for-profit purposes. Brian McMahon inserted *MISSYM* in the IUCr checking package in the fall of 1989 (Dacombe & McMahon, 1990). Overnight, marshable structures became an essentially extinct species in IUCr journals. Dick Marsh refocused his campaign on chemistry journals with about the same success, while things went back to business as usual at *Acta C* and *MISSYM* went on with its own career as a filter in crystal structure databases (Calvert, 1992; White, Rodgers & Le Page, 2002).

The *CREDUC* algorithm was again key to the interpretation of convergent-beam electron-diffraction (CBED) patterns for cell volumes (Le Page & Downham, 1991) or least-squares cell data (Le Page, 1992b). These concepts and developments were applied to rapid phase analysis in electron microscopy, and presented at IUCr XVI in Beijing (Le Page, Chenite & Rodgers, 1993).

It has been well known since Mallard (1885) that twinning is due to metric symmetry or pseudosymmetry of a multiple cell, and characterized by its twin law with small obliquity and small twin index. Although Cesaro (1886) dabbled with the problem, more than a century later no software existed that would be capable of spelling out all possible twin laws within maximum index and maximum obliquity for a given material. The main change required to adapt the *CREDUC* algorithm to the prediction of binary twins was then to relax the limit on the dot product condition and the limit on Miller indices to a common integer $2n$ value. If the dot product for a solution is p , the twin index is either p or $p/2$, depending on twin-lattice centering. The value of δ is the twin obliquity, while that of ϵ is the maximum obliquity. The corresponding program called *OBLIQUE* was presented at IUCr XVIII in Glasgow (Le Page, 1999) and published as Le Page (2002). It has since been spelling out possible binary twin laws within maximum obliquity and maximum index without missing one or proposing a wrong one, again because it is based

on theorems. This is a big help for the identification of twin laws from single-crystal diffraction patterns for all organic, inorganic and, recently, macromolecular materials.

Le Page, Klug & Tse (1996a), presented at IUCr XVII in Seattle and published as Le Page, Klug & Tse (1996b), describes a step-by-step manual procedure to use *MISSYM* to derive conventional crystallographic descriptions from the *P1* models in primitive axes used by *ab initio* DFT quantum modeling. This was again a timely contribution, as quantum modeling was becoming feasible with the expensive supercomputers that had recently sprouted around the planet. By that time, the performance of inexpensive off-the-shelf PCs was already within one to two orders of magnitude of what was needed to perform quantum computations on real systems. As an order of magnitude corresponds to five years in Moore's law terms, it became clear that a spectacular rampup in the use of quantum software was to be anticipated for shortly after Y2K. Anybody who dabbled with quantum software knows how tedious and time-consuming the creation of the model and its input files can be, as well as the interpretation of the output files. It could then be foreseen that by around 2005, the power of even a modest computing setup would exceed the data creation and interpretation capability of an individual, thus creating a bottleneck. Exactly like Eric Gabe, Allen Larson, Yu Wang, Peter White and myself had created in the 1970s the *NRCCAD* and *NRCVAX* software packages that automate to a great degree the work of the inorganic structural crystallographer, I similarly set up in 1998 with Paul Saxe from Materials Design Inc. to automate to a very significant extent the job of the quantum modeler in a first package called *Medea*. We first created and automated the least-squares symmetry-general calculation of the elastic tensor from total energy (Le Page & Saxe, 2001) and from stress calculations (Le Page & Saxe, 2002). At the heart of this automation is the programming of the step-by-step procedure described in Le Page, Klug & Tse (1996b). The work was submitted to the XIXth IUCr Congress in Geneva (Le Page, Saxe & Rodgers, 2002a) and printed as Le Page, Saxe & Rodgers (2002b). The next implementation, called *Materials Toolkit*, with Innovative Materials Technologies Inc. (Le Page & Rodgers, 2005) is much more versatile and includes powerful extensions beyond the capabilities described in Le Page, Saxe & Rodgers (2002b). Its achievements include exploratory computation of the thermomechanical properties of existing or prospective materials by the thousand, contributions to seismology, permanent storage of toxic or radioactive elements, correction of experimental errors in published elastic tensors for reference materials, design of a coating for turbine blades in jet engines that increases their resistance to erosion by nearly an order of magnitude, now certified for commercial flights and commercialized, etc.

What am I doing now? Mostly three things. It is quite common nowadays that experimental data alone are too scant to allow deductive scientific conclusions. I actively seek such bogged-down studies within ICPET. Complementing the study with quantum modeling often allows definite conclusions to be drawn by giving unambiguous support to a model, even in the absence of a competing one. This is the first thing. The second thing that I do is introduce young crystallographers around me to the automated application of quantum methods as in Mercier & Le Page (2008). The combined result of the first two items is enthralling joint publications with my ICPET and IRC crystallographer colleagues that get noticed internationally (see *Acta B* highlights in IUCr Newsletter, fall 2005 and spring 2007). They also get noticed at NRC, as shown in Fig. 1, where the team of ICPET and IRC crystallographers receives a much coveted NRC award for Research Excellence. This also establishes factually that *Materials Toolkit* is an extremely powerful and versatile package, at the cutting edge of both structural science and materials science. The third thing I do is expand the range of automated applications in *Materials Toolkit* (see Le Page, 2006) while getting methods and results published. The latest such study has been the precise quantum calculation of surface tension on crystal faces for metals, currently being refereed.

When looking backward and extrapolating forward, I am happy to see that crystallography has constantly remained at the cutting edge of knowledge and technology since 1850. Its activities are surfing from one wave to another, gaining momentum each time through innovation and greater automation of complex repetitive tasks. From what I can see, this is not going to slow down soon, as quantum modeling of materials, a branch of modern crystallography, is currently undergoing a phase of

explosive expansion in its capabilities and volume of output. This comes from greater automation harnessing the exponential increase of available computing power. This expansion will ultimately be a success only if the software that automates it rests on theorems and not on *ad hoc* considerations suffering from particular cases and exceptions.

Yvon Le Page

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Stanley C. Nyburg



Stanley C. Nyburg

I was born in London, UK, in 1924. In 1939 at the outbreak of World War 2, my school was evacuated to Weymouth on the south coast. Whilst there in May, we experienced the dreadful evacuation of troops (mainly French) from Dunkirk, whom we helped to get fed and accommodated. In 1939 I took my matriculation examinations. We were examined on *The Merchant of Venice* during a heavy air-raid and (almost certainly illegally) were not sent to an air-raid shelter. We enjoyed the bombardment greatly. Oil tanks in the harbour were struck and the sky was so darkened we had to finish with all the lights on. I passed all the exams.

By 1940 bombing was so severe we all had to return briefly to London. From there we were re-evacuated to Winchester. Here I studied for university entrance exams. I was granted a university scholarship to read for a BSc in engineering but did not accept. J. Lyons & Co. asked me to work in their labs for them. I enrolled as a part-time university student at Birkbeck College (then in Fetter Lane) to read for an honours degree in chemistry with physics subsidiary. Air raids over London were frequent and perilous and classes at Birkbeck were moved from evenings to weekends. After one particularly intense air raid we had to sweep our lecture seats clean. The lecturer in Physics, having filled the blackboard, slid it to one side, revealing a gaping hole (much of the external wall had been blown away), and there in all its glory was a view of St Paul's Cathedral.

In October 1944, I was accepted by King's College, London, for one year to read Hons BSc Chemistry. Prof. G. M. Bennett was Head of Department and lectured on Physical Methods. He dealt briefly with X-ray crystallography, already a powerful structural tool. He warned us not to get too heavily involved, a subject 'clearly beyond most of you'. Prof. Bennett found me an opening as crystallographic trainee at the British Rubber Producers' Research Association (BRPRA) in Welwyn Garden City. (I did mention his earlier remarks about crystallography being 'difficult' but he laughed it off.) BRPRA was a recognised Institution of London University.

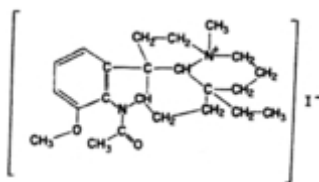
I here met George Jeffrey ('Jeff') who was to be my supervisor, and also Andrew Booth (q.v.). I started X-ray crystallography with primitive high-tension equipment on which I nearly killed myself. In November, E. G. Cox (later Sir) was appointed to a Chair in Chemistry at Leeds University and asked Jeff to join him as Lecturer. Accordingly I went in November 1945 to live in Leeds and read for a PhD there. (My brief sojourn at BRPRA allowed me to be a humble co-author on my first paper, not published until 1948) (1).

Under Prof. Cox's leadership there grew up a very effective X-ray crystallographic group. It was one of the first to semi-automate the summation of Fourier series. Hollerith cards were punched to correspond with Beevers-Lipson strips and these were summed on a card-reading machine used for accounting at the local electricity company (2). After a considerable struggle, I managed, by photographic methods, to solve the crystal structure of an organic compound. The results were submitted for a Leeds PhD granted in 1949 (3).



From the 1954 published structure of rubber.

In 1949, I married and moved to St Albans. I obtained an Assistant Lectureship at the new University College of N. Staffordshire at Keele, to where we moved in 1952. Here I solved the natural rubber structure in 1954 (4). I had started this work before at the BRPRA, where I stretched a wide sheet of natural rubber on a frame and put it in a domestic freezer. Released from the frame the frozen rubber retained its shape. With suitably cold scissors, a small fragment of frozen rubber was cut out. A skilful glass blower made a Dewar container, the inner chamber being connected to the outside by a glass spiral tube. A hypodermic was affixed to the outside and when filled with liquid air the Dewar provided a continuous fine stream of very cold air. The rubber sample was mounted in this stream in a collimated beam of X-rays. Excellent diagrams were obtained at 30 deg rotational intervals and all intensities estimated by eye. The Bragg intensities can be well explained if the crystal structure is taken to be disordered. The isoprene units are planar, as required, but can occupy one of two possible positions. This structure has been widely accepted to be true.

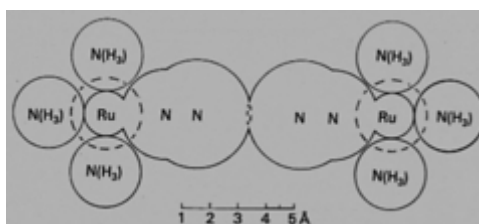


The correct molecular connectivity of aspidospermine, as determined by crystallographic methods in the late 1950s.

At Keele, a member of staff, Dr G. F. Smith, asked if we could solve the structure of aspidospermine, first studied in 1914 and the subject of chemical study since 1947. It was then a member of that increasingly large body of natural products whose molecular structures had eluded unambiguous determination by degradative methods. The methobromide proved to have the bromide ion unfavourably placed, but the methiodide also made by Dr Smith proved to be soluble by the heavy-atom method. Our analysis showed that none of the then proposed structures was correct (5,6).

We had no X-ray diffractometric equipment at Keele. We built our own X-ray generator, determined Bragg intensities photographically and determined a number of crystal structures. Here I wrote and published my monograph on structure analysis (7).

In 1958, G. A. Jeffrey had moved to the University of Pittsburgh to set up a Department of Crystallography. In 1962 he invited me and my family to go to Pittsburgh on a Fulbright Fellowship. Here I started my investigations into how crystalline Cl₂ came to have its extraordinary herring-bone structure.



Overbutting of two nitrogen molecules in a ruthenium complex.

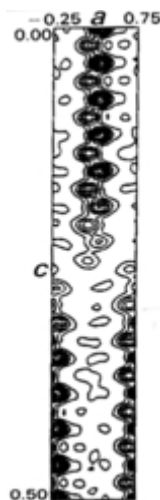
In 1963 we visited the University of Toronto and the Head of Chemistry offered me the post of Full Professorship. I accepted and we all had to return to England for one year to satisfy Fulbright conditions. We all went permanently to Toronto in 1964.

In the new Department of Chemistry at Toronto I was given liberal office and lab space and substantial funds with which to get started. Automatic diffractometers were becoming greatly in demand and the manufacturers spared no efforts to get their products bought. In the end I decided on the four-circle Picker machine. This proved a good choice, the machine being operative for more than forty years. During the 23 or so years in Toronto the Picker had a variety of computers and devices for its full automation. We used the *NRCVAX* system of structure determination written by Eric Gabe and collaborators at the National Research Council Laboratories in Ottawa. We provided a structure-solving service for the Department and were engaged in other research activities. Our most exciting determination came early on, namely the discovery by A. D. Allen in 1965 of a ruthenium complex containing molecular nitrogen as ligand. Traditionally it had always been considered that molecular nitrogen was too inert to form chemical compounds. This work clearly damaged that tradition (8).

From a study of the Cambridge Organic Crystal Database, we made scatter plots of non-bonded distances between identical atomic elements. We published the results later, showing that for the halogens especially the effective van der Waals shapes are not truly spherical but flattened. (9)

In 1974, the program BMFIT was written to give the closest match between molecules having some structural features in common. (10)

We also collaborated on crystallographic matters with the Department of Pathology at Mt Sinai Hospital in Toronto (11).



$y = 0$ electron-density map of β -C₂₄,C₂₆, showing decreasing C-atom site occupancies towards the end of the C-atom chains.

During the 70s we were also approached by Esso to pursue crystal studies of normal alkanes. This interest has lasted ever since. Our first entry into these new studies was to discover that the published crystal structure of *n*-octadecane was wrong. This we corrected. We obtained some exciting results from our alkane studies. Mineral oils contain largely normal alkanes of many molecular lengths, and when cooled this oil deposits waxes which greatly handicap the extraction of the oil. Despite enormous research effort by the oil companies, the crystalline nature of these waxes was not known. We decided to examine a simple binary mixture of alkanes as a simple model of a wax. The C₂₀:C₂₂ phase diagram was examined by our Australian colleagues. It is quite complex and shows there are certainly new phases in the 50%:50% region. We obtained excellent crystals from such a 50%:50% mixture dissolved in dodecane. Weissenberg photographs were taken with the crystal cooled to 10°C. From these we inferred that the space group was orthorhombic, $Bb2_1m$, with $a = 4.99$, $b = 7.47$ and $c = 56.26$ Å, the

two pure components being triclinic. We managed to find the crystal structure. Either of the two alkane molecules can occupy the same position, their combined symmetry being a mirror normal to the *z* axis, a symmetry element which neither alkane possesses. This fascinating structure forms the basis of the alkane wax structures. At room temperature a disordered *F* phase is present. Later, a diffractometric study was made at King's College, after we had returned to UK, of the binary pair *n*-docosane:*n*-tetracosane. The phase diagram is similar but is everywhere higher in temperature. A diffractometric study was made of the 50%:50% crystal. This confirmed our earlier results. A truly remarkable crystal structure containing molecules with twenty-four and twenty-six carbon atoms which shows twenty-seven Fourier peaks! (12).

In 1987 it was decided that, since so much crystal structure service work was being carried out, the Department of Chemistry needed a more efficient diffractometer than the Picker. Also at that time my wife and I felt it would be a good time to return to the UK and live closer to our elder daughter and grandsons. The Picker was deemed surplus to requirements. I negotiated for an Honorary Senior Fellowship at King's College London and the Picker was sold to them for a few thousand dollars. Its delicate goniometric parts were covered in a slow-setting sheath of polystyrene by someone who had done similar work for Ford automobile engines. It was shipped (all 0.75 tons) over the Atlantic in a Russian freighter to London, UK.

Setting up the Picker at King's raised a number of problems, particularly the change from 115 V a.c. to 240 V a.c. However, in all this I was greatly helped by a staff member, Dr Adrian Parkins. The Picker was newly fitted to a PC, an IBM 286, and Gabe's Fortran program for data collection was rewritten in Gbasic. The Picker behaved excellently and Adrian and I completed a large number of crystal structures for staff members.

In the early 90s, King's acquired a Nonius CAD diffractometer. The Picker was subsequently devoted to further work with normal alkanes. Unfortunately, the Department of Chemistry at King's College London was closed down permanently by the College authorities in 2006.

I am now Honorary Senior Research Fellow at University College London, continuing normal alkane studies.

Stanley C. Nyburg

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I. David Brown



I. David Brown

I studied for my bachelor's degrees in General Science and Honours Physics at King's College, London, UK, at the time when Rosalind Franklin and Maurice Wilkins were determining the structure of DNA. For graduate studies I moved to the Royal Institution (RI), where the group working with the director, Sir Lawrence Bragg, was unraveling the first crystal structure of a protein. For my PhD under the supervision of Jack Dunitz, I solved the crystal structures of two organic complexes of Cu(I), doing most of the calculations by hand, but experimenting with writing machine-language code for a computer with around a hundred and twenty memory locations which had to store both program and input. When Jack accepted an appointment at the ETH, I followed and spent my final PhD year in Zurich. In 1959, as was customary at the time, I crossed the Atlantic to take up a postdoctoral position at McMaster University with Howard Petch. My job was to finish building a single-crystal neutron diffractometer at the newly opened McMaster swimming-pool reactor. During this time I developed an interest in the structure of inorganic compounds, a term which I use to include any non-metallic crystal that contains no C-H bonds. This was an area much neglected except by mineralogists who were, however, focused on natural minerals. Solid-state physicists were not prepared to work with anything more complex than a cubic crystal, preferably containing only one kind of atom, while chemists thought of a crystal as a kind of freezer in which a dormant compound could be indefinitely stored until it was needed to carry out some real chemistry in solution. Those who called themselves 'inorganic chemists' were more interested in preparing organic molecules that could bind to a transition metal atom. The neglect of inorganic solid-state chemistry arose in part from the lack of an effective model for the systematic description of infinite structures. Most descriptions were based on an eclectic mixture of words such as 'packing of spheres', 'ionic interactions', 'covalent bonds', 'space group symmetry', 'polyhedral packing' and 'structure type'. Unlike the chemical bond descriptions used in organic chemistry, descriptions of inorganic structure tended to be vague and unconvincing.

In 1960, I was joined at McMaster by Chris Calvo, who in his short life published a series of high-quality structures of phosphates that still compare favourably with structures published today. We set up a small X-ray laboratory with a generator and a precession camera, where we were later joined by Colin Lock in Chemistry and Doug Grundy in Geology. Subsequently, this laboratory was expanded and provided structure determinations for other faculty members, while students had the opportunity to learn how to determine crystal structures. Today this laboratory is managed by Jim Britten, and is equipped with the latest X-ray diffractometers and software.

After my postdoctoral years I joined the McMaster Physics Department, where I spent my first ten years wandering in my own scientific wilderness. Although I gained experience solving structures for my chemistry colleagues I could not see where my research was going. As many can corroborate, it is difficult in the wilderness to know what one is looking for, or even if there is an answer to the question that has yet to be discovered. When Gai Donnay came to McMaster to show how Pauling's electrostatic valence model could be improved by taking account of the correlation between bond valence and bond length, I failed to pick up on the clues. My epiphany came a couple of years later in 1971, when Bob Shannon arrived at McMaster on a year's leave from duPont. Within an hour of our first meeting he had convinced me of the importance of Donnay and Allmann's work. The rest of that year we spent extending and simplifying their approach. In our paper we showed how bond valences could be calculated from a wide range of experimentally observed bond lengths, how they are normalized by arranging that they sum to the valence (oxidation state) of each atom, how they can be used to check the validity of newly

determined structures, and how they provide a quantitative measure of the strength of a bond that, unlike the bond length, is independent of the nature of the atoms involved.

At the end of his stay, Bob Shannon left me six boxes of computer cards on which he had laboriously punched the unit cells, symmetry operators and atomic coordinates of over four hundred inorganic crystals, but this database was useless because there was no way to retrieve the information it contained. The experience made me realize the importance of creating a well-structured machine-readable database if I wanted to carry out systematic studies. A large part of my subsequent career has been devoted to simplifying crystallographic data retrieval, first by creating an annual bibliography of inorganic crystal structures (BIDICS 1969-1981), and later proposing a common file structure to allow the interchange of crystal structure databases between programs and laboratories (this has now evolved into CIF). At the same time, Guenter Bergerhoff and I started the Inorganic Crystal Structure Database (ICSD). By 1985 the ICSD had reached the point where Daniel Altermatt and I were able to use it to prepare a systematic set of bond-valence parameters.

Subsequently, I expanded the bond-valence model to show how it can be used in structure modeling of both solids and liquids, how it predicts chemical stability and reactivity including aqueous solubility, and how the bonding geometry can be analyzed into separate contributions from chemical bonding, steric strain and electronic anisotropies. An invitation from Henk Schenk to present a series of lectures in Amsterdam in 1994 led to the publication in 2002 of my book 'The Chemical Bond in Inorganic Chemistry' (OUP), which brings all these threads together.

In 1986 I acted as local chair for the ACA meeting held at McMaster, the last ACA meeting held on a university campus. Subsequently, I persuaded the ACA to establish a Canadian Division and I served as its first representative on the ACA Executive, an experience which convinced me of the importance of the ACA in bringing together North American crystallographers.

My students and academic colleagues will probably remember most affectionately the daily morning coffee breaks when my group would meet together informally. We discussed many topics on these occasions, including the problems of structural chemistry, sometimes focusing on the particular problem of an individual, sometimes raising more general questions. On these occasions we would let our imaginations run wild, but always challenged each other to test and justify each new idea. These gatherings were the forge in which the bond-valence model was hammered out.

I. David Brown

A. C. Larson in Canada



Allen C. Larson.

I had joined the Los Alamos Scientific Laboratory (LASL) in 1956 after graduating from Washington University with a PhD. I worked with Don Cromer and Brad Roof studying the structure of alloys of plutonium. One of my principle activities was developing software for determining and refining the crystal structures of these phases. I quickly established myself as an excellent software developer using the LASL-developed Madcap language on the MANIAC (a LASL-developed computer), and very soon after

that Fortran on the IBM computers at LASL. In 1966, I was elected to the Computing Committee of the American Crystallographic Association and made a member of the IUCr Computing Committee, and I chaired the Committee from 1975 to 1978. Then I was a consultant to the committee again until 1981, a total of twelve years with the IUCr Computing Committee.

We noted that many of the alloy structures we were finding were similar to already known structures. In an effort to enable us to solve the structure of the new phases which our metallurgists were creating, we decided that developing a computer-based data set of the known structures was necessary. In the summer of 1960 we had a summer Graduate Assistant, Joe Finny, search the literature and enter the details of known metal and alloy structures into our data file.

In the summer of 1973, Los Alamos Scientific Laboratory was having a funding crisis and needed to reduce its staff. They assembled a list of people to place on the RIF (Reduction In Force) list. Unfortunately for me, I was placed on that list. I did not want to abandon crystallography, as they insisted I would have to if I were to take an alternate position within the laboratory, so I opted to accept the RIF pay and leave the laboratory and moved with my family to Austin, Texas, where my wife had enrolled in the Law School at the University of Texas.



Larry Calvert (left) and Eric Gabe (right).

I contacted several people around the US and Canada in the weeks following receipt of the RIF notice. One important person on that list was Dr W. B. (Bill) Pearson, who was at NRC in Ottawa. One of my concerns was the metals data file that Dr Don Cromer and I had started to construct at Los Alamos. We felt that that project would be lost after I left Los Alamos. I asked Dr Pearson if there was anything I could do to find a new home for it. He invited me to bring it to NRC, where Dr Larry Calvert, who was doing a lot of work on the IUCr Structure Reports metals data, accepted responsibility for it. I flew to Ottawa with the metals data file and with the software that we had developed to access the file on a magnetic tape. Dr Calvert and Dr Gabe had a DEC PDP-8, which we used to further develop the data file and its software. This machine had been greatly enhanced from the 4096 12-bit word PDP-8 that I had been using in Los Alamos for diffractometer control.

After completing the transfer of the file, Calvert and Gabe asked me to install *MULTAN* on the PDP-8. I had worked on doing that on a small computer for Syntex in Cupertino late in 1974 and agreed to this new task. And after we finished that, it was agreed that I should install other portions of the Los Alamos Crystal Structure package on the PDP-8. This included the space group symbol-interpreting software that I had developed on sabbatical year in Cambridge and Los Alamos. However, since I had installed that package on the Syntex hardware, we decided that perhaps it would be best if I wrote a new program. Thoughts about licensing drove us to this decision. The accuracy of the new program was checked by comparing its output with that of the original program, which had very carefully been checked manually against the International Tables for X-ray Crystallography, 1952. The new code uses matrices and vectors to develop the groups, where the older code had developed its operations by scanning a list of binary numbers that I had developed. The new code was carefully documented internally, but the older code was not nearly as well documented internally. This new code is the one that is used in the *GSAS* package today.

In the summer of 1978, Eric Gabe and I presented the NRCC software package at the IUCr Computing School in Enschede, The Netherlands. I was partially funded by the IUCr on this trip, which included the IUCr Meeting in Warsaw, Poland.



Allen Larson in a style of clothing more typical of Canada (*circa* 1981).

My last major stay in Ottawa was from 15 November 1980 to 15 February 1981. On this trip, Eric Gabe and I started to develop a powder data refinement capability into the software package. I was back in June 1981 for the IUCr meeting that year, now funded by Los Alamos National Laboratory that had rehired me on 1 June 1981.

In the seven years that I was self-employed I also spent two months at the University of Calgary working with Dr K. Ann Kerr, again installing and developing crystallographic software. This was in two separate month-long stays, the first in June 1975, the second in March 1976.

My duties on returning the Los Alamos were to support the diffraction instruments at LANCE, the Los Alamos Neutron Scattering Center. Initially I was to support a single-crystal instrument, but since they wanted to develop a powder instrument as well was asked to help develop software to analyze its data as well. Prof. Robert Von Dreele at ASU was one of the people involved with developing the powder diffraction capability and he assisted me in expanding the capability of the Los Alamos crystal structure software to be able to refine structures against powder data.

One of the criteria that I always use in developing software is to keep it open to the addition of new data sources and new output products. With this in mind, Bob and I continued the process of inserting powder data refinement into the Los Alamos Crystal Structure package. I had been thinking of a name for our software system and came up with 'Generalized Structure Analysis System'. After a bit of thought we decided that we should drop the 'ized' in that name and thus the 'General Structure Analysis System', *GSAS*, came into being.

Allen C. Larson

An interesting interlude with pyrolytic graphite

In 1972, after a brief visit to New Zealand, Larry Calvert and his colleague, Eric Gabe, called in at the (then) Chemical Physics Division of CSIRO in Melbourne. Discussion with the Chief of the Division, Dr A. L. G. Rees, led to Larry coming to spend an extended spell with us in 1973-1974.

At that time, I was interested in certain aspects of extinction of an experimental nature, rather than the more usual theoretical concerns. Originally, the subject had dealt with the shape of single-crystal diffraction peaks (ref. 1). Since most single-crystal X-ray peaks are too narrow to handle experimentally, my attention had been directed to pyrolytic graphite, of which the 0001 peaks were of a more feasible magnitude for experimental observations.

Joining us in these investigations was Reg Killean (Dr R. C. G. Killean, School of Physical Sciences, University of St Andrews, Scotland). One aim was to investigate whether the level of polarization changes across a diffraction peak. A source of polarized X-rays of adequate intensity was needed. It turned out that graphite 0006 could be quite successfully used with $\text{Cu K}\alpha_1$ (ref. 2). The basic arrangement for this experiment involved a Picker diffractometer with a pyrolytic graphite crystal set to provide the polarized beam. This beam of polarized monochromatic radiation passed through a hollow shaft constituting the collimator of a second small diffractometer (ref. 3) with the second pg crystal (the analyser). Mounted on the shaft was the detector arm, which could rotate about the shaft. One could therefore take readings of intensity at 0, 90, 180 and 270° and deduce the polarization ratio at each

angular setting of the specimen crystal at specific 2θ settings. The four readings allowed for internal checks. Thus, due to physical symmetry, readings at 90° and 270° should be equal.

This allowed a complete study of the change in polarization of the 0002 reflection of pyrolytic graphite (a ZYA specimen). This procedure allows for accurate average values of polarization ratios for any crystal monochromator system. A further experimental study was possible using the polarized monochromatic beam and the small diffractometer. This was to investigate the interaction with a turbostratic structure. For this purpose, the polarized beam is passed along a hollow shaft which constitutes the inlet collimator to the small diffractometer. Also attached to the shaft is the detector unit with the second (specimen) crystal adjustment. The intensity for each setting of the specimen crystal is determined and can be converted to an estimate of absorption. A full account is given in ref. 5.

From the material studied in this last work, it was possible to obtain an estimate of the X-ray attenuation coefficient of carbon for $\text{Cu K}\alpha_1$ radiation given in ref. 6. Larry was an enthusiastic and stimulating colleague and the three of us worked well together. The collaboration was both enjoyable and fruitful. The work carried out then has developed over the years and has led to a modified viewpoint on extinction.

Sandy Mathieson

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MEETING REPORT

Crystallization Schools in Spain

Granada, Spain, May 2009 www.iscgranada.org/,
lafactoria.lec.csic.es/iscgranada

The 2nd Int'l School on Biological Crystallisation (ISBC) and the 1st Int'l School of Crystallisation: Foods, Drugs and Agrochemicals (ISC) were held under the auspices of the IUCr Commission on Crystal Growth. The Schools were aimed at students, and scientists from industry and academia, seeking fundamental knowledge of the crystallization phenomenon and the behaviour of crystallising solutions. Whereas the ISBC focused on the crystallisation of biological materials, the ISC's centre of attention was the application of crystallisation techniques to the fields of food and drug technology and the agrochemical industry. Both schools offered hands-on demonstrations. Attendees participated in round-table discussions and daily poster sessions. There were 111 participants from 18 different countries on five continents. All students from outside Spain received some funding from the Schools.



Students at a practical during the Demonstration Fair at ISBC.

The ISBC focused on the fundamentals of crystallisation from solutions with applications to biology. The five days of lectures and practical demonstrations covered the crystallisation of biological macromolecules, biominerals and biomimetic materials, with a full day devoted to crystallisation of membrane proteins and large macromolecular complexes. Introductory lectures covered purification of proteins, the physicochemical properties of solutions, nucleation and light scattering theories, crystal growth techniques, handling of protein crystals, bio-mineralisation, and publication of crystallisation results. A list of the main topics and lecturers can be found at isbcgranada.org.

The ISC School encompassed lectures on both classical and novel crystallisation techniques such as gel growth, hydrothermal growth or crystallisation from melts, polymorphism, chiral crystallisation, crystallisation in space and the interface between drug substance and drug product. The list of teachers and the full program is available at lafactoria.lec.csic.es/iscgranada.



Winners of the Poster Prizes in ISC 2009

Poster sessions (35 posters in ISBC and 27 in ISC) were held each day and five poster prizes were awarded in ISBC and four in ISC. ISBC 2009 poster prize winners: IUCr Prize, Anita Hoffmann (Germany)

and Bilal Qureshi (Germany); La Factoria Prize, Rafael Fernández Leiro (Spain) and Rossana García Fernández (Cuba). ISC 2009 poster prize winners: IUCr Prize, Jonathan A. Foster (UK) and Yuki Araki (Japan); La Factoria Prize, Alenka Lenart (Slovenia), Marianna Lioliou (Norway) and Tomoya Yamazaki (Japan).

The social program included a welcome cocktail on Sunday night, a tour of the Moorish palaces of The Alhambra, and a Flamenco Party in the gypsy district of Sacromonte, renowned worldwide as the epicentre of flamenco culture.

Participants received a CD containing all the lectures and demonstrations performed, including posters and pictures. We are grateful to all who worked to make the Schools successful, especially the Sponsors and Exhibitors.

Juan-Ma Garcia-Ruiz

OBITUARY

Maria Przybylska (1923-2008)



Maria Przybylska

Maria Przybylska (maiden name Kolasa) passed away peacefully on October 8, 2008 in Ottawa, Canada. She was born on March 2, 1923 in Warsaw, Poland. Having narrowly escaped Poland with her father and brother just ahead of the German and Russian invasion of 1939, she made her way to Scotland. She studied chemistry at the University of Glasgow and her passion was to study biologically relevant molecules. After she attended a lecture by John Monteith Robertson on the application of X-ray crystallographic methods to determine structures of organic molecules she was inspired to pursue graduate work with Robertson. She determined the structure of quinol dimethyl ether with only a slide rule to perform the inverse Fourier transforms.

She obtained a fellowship from the National Research Council of Canada (NRCC) to work in the X-ray Diffraction and Electron Microscopy Section headed by W. H. Barnes where, in collaboration with Leo Marion and F. R. Ahmed, she started working on the structures of alkaloids. This work led to her appointment in 1951 to the NRCC's Division of Applied Chemistry. Structure determination of organic molecules at that time required patience. Data processing for a molecule of ~25 atoms took months! However, there was great excitement in solving every new structure as each one provided substantial new insights. Maria was able to assemble a group of excellent scientists. Later, her group was merged with the group of F. R. Ahmed and A. W. Hanson and was incorporated into the Division of Biological Sciences. During the latter part of her career she focused on immunochemistry and the challenges of crystallizing and solving structures of large biologically significant proteins. Maria spent many years studying the haptoglobin-hemoglobin complex. Her desire to solve the structure of a protein was fulfilled with her participation in the oncomodulin project in collaboration with F. R. Ahmed and others. Structures of several other proteins were later determined in the laboratory, including the antibody Fab fragments specific for *Salmonella* and *Brucella* polysaccharide antigens.

Maria had little patience for bureaucracy and administration, preferring to work in the wet lab, solving and analyzing structures. Maria was 'all business' when it came to her laboratory, insisting on thoroughness, cleanliness and accuracy. She was a rather private person, but always ready to discuss her or others' research projects. Behind this business-like front was a kind, caring and supportive person who welcomed newcomers to the group and provided advice and guidance when needed.

Maria worked at NRCC until her retirement in 1988 and continued her research as her health and facilities would permit into the late 1990s. Her distinguished scientific career spanned 50 productive years, yielding 70 publications. She was pre-deceased by her husband Wacław Przybylski (1918-2005), and is survived by two sons, Martin and Steven (Michelle), and grandchildren.

We are thankful to Farid Ahmed, Eric Gabe, Carol Huber, George Birnbaum, Margaret Pippy and Helen Sheppard for useful input. Photograph is courtesy of Steven Przybylski.

Mirek Cygler and David R. Rose

OBITUARY

John S. Rutherford (1938-2009)



John S. Rutherford

John Stewart Rutherford passed away in September 2009 at the age of 70 years in the aftermath of a stroke from which he had suffered in May.

Remembering John for his achievements in teaching and research falls short of characterizing the entirety of his contributions to the scientific community, which is equally indebted to him for the energy and effort he spent in helping institutions of higher learning in emerging countries to uplift their teaching and research programs.

After graduation from Glasgow University in 1959 and an interlude at an ordnance establishment in Scotland, John joined the group of the late Crispin Calvo at McMaster University in Hamilton, Ontario, where in 1967 he received his PhD with a thesis on the crystal structure of selenourea. John then went on to the Department of Crystallography at the University of Pittsburgh to study the structure of anti-cancer drugs. Back in Great Britain he investigated the crystal structure of phosphorous-nitrogen ring compounds at the University of Essex, after which he returned to McMaster for low-temperature diffraction studies. This was followed by a two-year engagement at the Northern Electric Company in Bramalea, Ontario, after which John joined the University of Regina, Saskatchewan, where he investigated the crystal structures of disulfonates. Back in his beloved Scotland, John applied his teaching skills at a high school in Linwood until 1979. This year marks a break in John's career, because from now on we see him in Africa and the Arabian Peninsula until the end of his active engagement in 2003. The first station is the University of Swaziland (1980-1982), followed by the University of the Transkei (1983-1984), the Sultan Qabos University in Oman (1995-1996), and finally the National University of Science and Technology in Bulawayo, Zimbabwe (1998-2003), where he stayed until retirement. All these institutions are indebted to John for his assistance in their quest towards academic excellence. John created or revised curricula, taught chemistry courses and participated actively in the administration, acting as head of the Department of Chemistry and assuming other functions which are too numerous to be listed here.

After retirement in 2003 he moved back to his native Scotland, where the newly gained freedom from administrative duties enabled him to resume and further develop his research on theoretical crystallography, from 2005-2008 as member of the IUCr Commission on Mathematical and Theoretical Crystallography which he had joined since its foundation as an informal international work group in 2002. He continued his investigations on the application of number theory - one of the most fundamental areas of mathematics - to problems involving crystal lattices. The cover of *Acta Cryst.* **A62** (2), a special issue on mathematical and theoretical crystallography, is taken from his article 'Some algebraic properties of crystallographic sublattices'. In later years he became increasingly interested in the applications of graph theory to problems of crystal chemistry. Here John made substantial contributions to a monograph on 'Graph Theory in Crystallography and Crystal Chemistry'. His passing prevented him from finishing this important task, which will nevertheless be brought to completion and will represent a most fitting homage to his memory.

John is survived by his wife Sasha, and two daughters from a previous marriage.

Wilfrid E. Klee

IUCR ACTIVITIES

XXII Congress and General Assembly

Madrid, Spain, August 23-30, 2011, www.iucr2011madrid.es/

IUCr2011 in the language of Cervantes?



No, don't worry; the official language of the *XXII Congress and General Assembly of the IUCr* will not be Spanish, although this international event will be held for the first time in Spain, in Cervantes' country. We kindly invite you to join us and to experience Spanish hospitality and especially that of Madrid in August, 2011.

Spain and Spanish crystallographers joined the IUCr in 1949, just two years after the birth of the Union. When *Acta Crystallographica* appeared in 1948, the first article published in its first volume was signed by the young Spanish crystallographer Julio Garrido on 'Observations sur la diffusion des rayons X par les cristaux de ClO_3Na ' (*Acta Cryst.* 1948, **1**, 3-4). Today crystallography is well established in Spain and crystallographic teams are distributed all around our motherland. Including the crystallographers that officially joined the *Spanish Group of Crystallography and Crystal Growth* (Grupo Especializado de Cristalografía y Crecimiento Cristalino, GE3C) and those belonging to other associations like Neutron Users, Solid State, Proteomics, Surfaces etc., over 400 Spanish scientists are engaged in crystallographic research, sharing diffraction facilities located not only in Spain (like the recently inaugurated Spanish synchrotron ALBA) but also at other international facilities (ILL and ESRF) with financial support from the Spanish Government.

At the Executive Committee meeting arranged during the Third General Assembly in Paris in July 1954, it was proposed that the IUCr should organize specialized symposia between the assemblies and the first of these meetings (Structures on a scale between the atomic and microscopic dimensions) was held in Madrid in 1956. In April 1974 an Inter-Congress Conference also took place in Madrid on the subject of Anomalous Scattering and ECM-6 was held in Barcelona in 1980. Now we are hosting the XXII Congress and General Assembly of the International Union of Crystallography in Spain, nearly one hundred years since the Laue and Bragg experiments, a memorable event from both scientific and social perspectives.



The IPC members met in May, 2010, at the 'Patio del Pozo' of the University of Alcalá, one of the oldest universities in the world.

The Local Organizing Committee and the International Program Committee (IPC) have been working hard to prepare a scientific program of the highest quality. The IPC has requested the approval of the IUCr Executive Committee for a program of 4 Plenary Lectures, 36 Keynote Lectures and 98 Microsymposia. A number of Satellite Meetings and Workshops will take place throughout Spain. The Congress will cover the most relevant areas of interest, from Crystal Growth to Crystallography and Art, and include Biological Macromolecules, Material Science, Organic and Coordination structures, Topology, Diffraction Physics, Surfaces etc.



Palacio Municipal de Congresos, the Congress venue.

The Congress Venue (Palacio Municipal de Congresos) is located a few meters from one of the newest Metro lines (connecting the city centre and the Airport in about 15 minutes). It provides the necessary technology and services as well as two auditoriums, large halls and rooms with different capacities and large exhibition areas. Moreover, Madrid, the capital and largest city of Spain, located in the centre of the country, represents the perfect symbiosis and harmony between efficiency and quality of life, at a price that is hard to beat anywhere else in the world. It is a city where you will find everything..., including splendid and ancient World Heritage sites (Alcalá de Henares, Toledo, Ávila, Segovia and Salamanca). We are preparing a very open meeting for all crystallographers from all over the world, a place where everybody can feel satisfied in all aspects, and we invite you to attend, support and contribute to this Congress.

Local Organizing Committee and International Program Committee