

## *Personal Reminiscences*

J. MONTEATH ROBERTSON

During the 1914–18 war my father lost his eyesight so I had to leave school and manage the farm at home. I liked this work but was always deeply interested in science, having read about J. J. Thomson's work on the atom and the electron in Arthur Mee's *Children's Encyclopedia* and other popular works, which my brother and I used to discuss very fully and often far into the night. We both felt a tremendous excitement about these great discoveries. Only afterwards, when my brother returned from the war, was it possible for me to do some private study and gain University entrance. Without a proper school training I found the science part easy but languages difficult.

At the University of Glasgow I read rather widely in Mathematics, Physics, Chemistry and Geology, but when it came to research I went into Chemistry, mainly I think because it seemed closer to the atoms and molecules in which I was deeply interested. My Professor, G. G. Henderson, persuaded me to start working on the structure of the sesquiterpenes and so I took my Ph.D. in organic chemistry. This was a difficult field and little progress was made, although L. Ruzicka about this time had been able to discover the carbon framework in some of these structures by dehydrogenation experiments. I could not help feeling that there should be some purely physical method by which one might be able to find the spatial positions of the atoms in such complex molecules. I knew about the X-ray method. W. H. Bragg had published his early measurements on naphthalene and anthracene, while Muller and Shearer had published some striking work on the long chain compounds. This seemed to be the method I was looking for, so with difficulty I obtained some good crystals of my complicated sesquiterpene derivatives and sent them to W. H. Bragg. Nothing much happened, so in 1926 when I was able to obtain a Fellowship of sufficient value (£250 per annum for two years) I followed my crystals to the Royal Institution, fondly hoping that in the two years available

I might work out these structures. This was in fact finally accomplished, but the task took over thirty years instead of two! An interesting commentary on this situation was made at a Chemical Society meeting in London a few months ago where I was reading a paper on the structure of some terpenoids. In thanking me, Professor D. H. R. Barton, who knew something of this early history, congratulated me on having at last completed my Ph.D.!

My decision to go to the Royal Institution was not an easy one to take. At this time I was also greatly attracted by work on atomic structure and I thought of seeking a place in the Cavendish Laboratory where Aston was working on isotopes. But in the end it was the Royal Institution that drew me because I wanted to finish the job I had begun in Glasgow. The transition from organic chemistry to physics was also difficult, especially as the equipment was primitive. I had to make most of my own X-ray tube with glass and sealing wax, and run it from an induction coil. The pumps were also primitive and home-made, so that one had quickly to become expert in high vacuum technique and leak detection. But everyone at the Royal Institution was helpful. Sir William Bragg asked me to start on a photographic survey of the anthracene intensities, while he himself worked in the next room with his ionization spectrometer. My more immediate tutors were W. T. Astbury, J. D. Bernal, Kathleen Lonsdale, A. Muller and G. Shearer; and I owe them all a great debt. It was an atmosphere in which one had to learn quickly, because these people did not suffer fools gladly.

By 1928 I had not achieved much, apart from publishing two incorrect structures, but had learned a good deal. I was then elected to a Harkness Commonwealth Fellowship and went off to the United States for two very happy years, spent mainly in the Physics Department at Ann Arbor, Michigan. I learned some theoretical physics with Uhlenbeck, Goudsmit, Dennison and Laporte, and tried to keep up some chemistry with Gomberg, in whose laboratory I had a bench which I seldom occupied. I read A. H. Compton's *X-rays and Electrons*, and also became acutely aware of the phase problem. This I tried to solve by a scheme that would take too long to describe, but computational facilities were totally inadequate for the approach I was trying. In 1930 the most important event in my life occurred when I married Stella Nairn in Toronto, in contravention of one of the conditions of my Fellowship, which was tenable only by unmarried persons.

Sir William Bragg then offered me a post in the Davy-Faraday

Laboratory and we returned to the Royal Institution. The next nine years were perhaps the busiest in my life, and I was able to solve many organic structures accurately and completely. The work was hard and the computational difficulties severe. We devised an elegant system of moving strips of figures on a large board, and many tedious hours were spent in summing Fourier series and making structure factor calculations after the children were in bed. This work was important and exciting, because we were measuring bond lengths and putting the chemical formulas on a true absolute scale for the first time. However, there was still an element of dissatisfaction, because the chemist could justly claim that he was already very familiar with these structures and that we were in a sense only confirming what he already knew. Our ultimate goal was to be able to solve structures that were chemically unknown.

Here the big event occurred a few years later when one day Sir Jocelyn Thrope and R. P. Linstead brought some excellent crystals of the newly discovered phthalocyanines to the Royal Institution. I was immediately summoned by Sir William Bragg and asked to examine this problem. To begin with it seemed an almost impossible task, because the unit cells were large and the reflections exceedingly numerous. But there was a striking isomorphism between some although not all the derivatives, and I felt that some help could surely be obtained from this fact. To cut a long story short, the isomorphous substitution and heavy atom methods of phase determination were evolved in this work and the structure completely determined without any reference to chemical theory. Great new vistas of possible progress in the direction of solving chemically difficult or unknown structures were thus opened up.

The organization at the Royal Institution during this time certainly helped a great deal in the progress I was able to make. There was an excellent workshop under Jenkinson. H. Smith who operated Muller and Clay's 5 kW rotating anode X-ray tube was most helpful, and B. W. Robinson and R. H. V. M. Dawton devised an integrating photometer which I used a great deal. I had no research students, although towards the end of my time at the Royal Institution I enjoyed active collaboration on certain problems with A. R. Ubbelohde, Ida Woodward, J. J. de Lange, and L. O. Brockway. (Why do young and immature members of University staffs always demand help from large numbers of graduate students from the moment they are appointed? If they would first work for ten years with their own hands they might then be better able to make a distinctive contribution.)

I left the Royal Institution early in 1939, not through choice but because with a rapidly growing family it was now urgently necessary to earn more money. Before leaving I had got as far as writing a paper to suggest that if mercury could be substituted for zinc in insulin, which seemed chemically feasible, then even a structure of this complexity might ultimately be solved by the heavy atom method. But now I went to Sheffield University, as senior lecturer in Physical Chemistry, and in a sense had to begin life again, in a laboratory that had none of the equipment I needed. However, the still greater interruption of the Second World War now began. I joined the Air Force and stopped all X-ray work for a number of years.

I returned, not to Sheffield, but to Glasgow as head of a large Chemistry Department. For my research this again meant starting from nothing, in a laboratory entirely devoid of X-ray equipment and with considerable responsibilities for teaching chemistry. However, I was now in a position to get what I wanted, and the University generously supported me. I soon began solving structures again and was pleased to find that I had not forgotten the way. I got out the notes on earlier work that had been hurriedly compiled and put away in a safe place as the first air raids began. Also, it was now not only desirable but a duty to organize the training of research students in many fields. I am glad to think that these efforts have been successful. Among those who began X-ray work with me in Glasgow after the war are A. McL. Mathieson and J. D. Morrison (now mass spectrometry) in Melbourne, Maria Przybylska in Ottawa, J. G. White in Princeton, Jack Dunitz in Zurich, Sidney Abrahams with Bell Telephones in New York, Walter Macintyre in Boulder, James Trotter in Vancouver, M. G. Rossmann in Cambridge, H. M. M. Shearer in Durham, and J. S. Broadley and D. M. Donaldson at Dounreay, Thurso (now atomic energy). Ian Dawson (now electron microscopy), J. C. Speakman, George Sim, Tom Hamor and Andrew Porte (now nuclear magnetic resonance) are all back in Glasgow again, after adventures in other places, and are actively participating in our latest and most exciting work.

We are, I think, now achieving a spectacular revolution in the X-ray analysis of organic molecular structures. The methods first developed in the 1930's are now coming to full fruition. This is largely because, with the invention of fast electronic digital computers, it is now possible to work in three dimensions with the same or even greater facility than in two dimensions ten years ago, and so for the first time we can really use the X-ray method at its full power. This is,

of course, an over-simplification. Much new and important development work has had to be undertaken during these recent years and many new problems solved. However, if we adopt the chemical approach which I have always advocated, and prepare a series of suitable phase determining derivates (and in general we know enough chemistry to be able to do this), then we can with confidence obtain complete solutions of organic molecular structures containing up to 100 atoms or more. However, this work is not yet, nor is it likely to become in the immediate future, a routine tool which anybody can apply. A good deal of judgement, and often intuition, are required in the first stages of deciphering the dimly resolved electron density distributions. It is always unsafe to predict the future, but I would say that crystal analysis is likely to remain both an art and a science for some time to come.