Reminiscences

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I took my first degree (B.Sc., Physics) from a womens' College when I was 19. W. H. Bragg, Quain Professor of Physics at University College, London, had been one of the examiners, for the papers were common to all Colleges of London University. Nowadays I would have been booked, either for an industrial or for a University post, six months at least before sitting the final examination. Then, there was a real shortage of scientific jobs (there was practically no industrial research in Physics and one of my colleagues applied for 150 posts before he got one, even though he had a good higher degree). Although I was told that I had headed the University list with the highest marks in ten years, I had no work in prospect except schoolteaching, which I did not fancy at all, when Sir William Bragg, then 60 years of age, sent for me to come and see him at University College. He first asked me how I had managed to do so well in the B.Sc. examination? I can't remember my answer to this unanswerable question, but he went on to offer me a place in his research team with a D.S.I.R. (that is, Government-provided) grant of £180 a year, and the opportunity of working for my Master's degree. It was luxury and I jumped at it. To give some idea of money values: at that time a two-course lunch at University College cost 1/6, tea extra. No-one drank coffee then, and morning coffee was unheard-of.

Twenty years later, when W.H.B. died, I was still with him at the Royal Institution as a Research Fellow on a year-to-year grant of £400 a year. Those who left his group for University teaching posts added substantially to their salaries, but they were always most reluctant to go, and often W.H.B. had practically to push them out. He could pick his men, and did; and the inspiration of working with such a team and under such a leader was wonderful.

Yet in fact he did not appear to lead at all, and we were not a team in the modern sense of the word. He did not suggest a subject for my research. He told me that it would take perhaps 3 months to collect together the apparatus that I would need (ionization spectrometer to be constructed in his workshop by the formidable Mr. Jenkinson, a huge lead-covered box to hold and shield the Coolidge tube, gold-leaf electroscope, induction coil with Hg interrupter, condenser, high-tension battery, spark-gap and milliammeter) and then I would put it all together myself. In the mean time he advised me to study Hilton's *Mathematical Crystallography* and think about some problem I might tackle. Professor H. Hilton had been my mathematics teacher at Bedford College, but I had had no idea that geometrical crystallography had been his special subject. I was fascinated by his book, which gave all the elements of symmetry but never mentioned general equivalent positions. I drew half-arrows in all the diagrams to represent asymmetric units; and still have the book, 40 years later.

It was understood that my problem would be the structural analysis of some organic compound (the inorganic compounds belonged to W. L. Bragg and his school in Manchester). But I knew *no* organic chemistry and very little of any other kind. So I asked W. T. (Bill) Astbury and he suggested succinic acid. He was investigating tartaric acid himself and succinic acid seemed likely to be a bit easier! The crystals had to be about 2 mm linear dimensions and I had beginner's luck and got a wonderful crop at the first attempt, by dissolving the whole bottle in hot water in an immense flask and cooling it overnight. I can still remember Astbury's astonishment when I showed him about one hundred perfect specimens, all of the right size, the very next day. He soon convinced me that this *was* pure luck.

A little while later W.H.B. asked me to measure the density of anthracene for him, by the flotation method. I gave him the result in some trepidation, for it was 10 per cent higher than that given in the literature. But he was pleased, for it corresponded with his calculated value on the basis of two molecules in the unit cell, whereas the published value would have given n = 1.8.

By the beginning of 1923 I had the bits and pieces of my ionization spectrometer equipment and a large table. A fellow research student (now my husband) offered to do the soldering for me. I agreed that he should show me how to do it, but said that then I would like to finish it myself. Cutting the gold leaf for the electroscope and attaching it (with spit) was a tricky job, but if you got a good leaf it was very sensitive and reliable. There was no 'beam-trap' on the spectrometer. How we failed to collect substantial doses of radiation (if we did fail) I simply cannot imagine, for in spite of the lead box the beam emerging through the slits that formed a collimator was partially divergent and of formidable intensity and dimensions. We certainly did get first-layer-line reflections into our ionization chambers, although these were only intended to collect the zero layer line; and our determinations of space group were sometimes in error through this unsuspected fact. We sat for hours with one eye glued to a microscope, taking readings of the movements of the gold leaf; occasionally we argued about crystal symmetry and less scientific subjects.

Our structure analysis was largely guesswork aimed at satisfying spatial and symmetry requirements and the more intense reflections. When I asked my chemistry friends whether succinic acid was likely to be in the trans- or cis-configuration they naturally replied that the question was meaningless, since there could be free rotation about the central single bond. However, I pointed out that in the solid state free rotation seemed not to be possible, and it was then suggested that since maleic and succinic anhydrides were isomorphous, succinic acid might well be in the cis-form when crystallized. I eventually produced a structure which gained me my M.Sc. although it was subsequently shown to be wholly wrong. In a sense I feel that my whole career has its foundations on sand. Can one give back an undeserved degree?

Meanwhile my reading of Hilton's *Mathematical Crystallography* and our many laboratory discussions had given us the idea that it should be possible, by systematic investigation of zero-intensity reflections, to determine the crystal space group if not uniquely at least within narrow limits. Without realizing that this had been already done by P. Niggli and R. W. G. Wyckoff (for alas, our library work was no better than that of many of today's young 'up-and-comings'), Astbury and I proceeded, independently but in parallel, to work out the 230 space-group tables, and we persuaded W.H.B., not without difficulty, to send them for publication. The usefulness of the Astbury-Yardley Tables, which the Royal Society had to reprint—a very rare event for a *Phil. Trans.* paper—perhaps lay in the fact that they were intended for immediate practical use. W.H.B., although himself a mathematician of distinction, had little use for any abstract mathematical theory, unrelated to experiment.

In 1923 most of us had moved with Sir William Bragg, lead boxes and ionization spectrometers and all, to the Royal Institution; and I was installed in a ground-floor laboratory with two other ionization spectrometers and some early gas-tube equipment for the photographic technique. My most joyous recollection of this period is of J. D.

Bernal on his knees looking for his one and only crystal of (I think) γ-brass. My chief regret is that I have no photograph of that probably unique occasion. I also remember the one occasion on which I saw Sir William even mildly angry, although I cannot remember the date. We always had tea together in the Royal Institution library (free: so nearly everyone came regularly) and there were often visitors. One visitor, a very supercilious young man, was amused at the fact that an elderly scientist at University College, London, had spent his whole life measuring the boiling point of sulphur with greater and greater accuracy. W.H.B. commented sharply that such research workers were the salt of the earth and that it would do no harm if their critics were equally conscientious. Yet he was kindness itself when I had to admit that I had made (and published) a mistake.

One of the more colourful characters at the R.I. in those early days was the cleaner, Nellie Collins, who was invariably cheerful and talkative. She and I exchanged letters once or twice a year until her sudden death on 6 February 1961. She was a staunch member of the Salvation Army and lived at the incredible address of 1, Frugal Cottages.

I left the R.I. in 1927 on my marriage to Thomas Lonsdale, who had a job with the Silk Research Association, then housed in the Textile Department of the University of Leeds. It had been my intention to give up scientific research work and settle down to become a good wife and mother, but my husband would have none of it. He had not married to get a free housekeeper; and if he were a good husband and father I could quite well keep on my 'Arbeit', as we called it to distinguish it from the domestic chores. It was great fun, although we were hard up, too much so to be able to afford University lunches. I kept very exact accounts and I have them still. We shopped together weekly at the Leeds Market, prepared vegetables together in the morning before leaving home and I allowed myself 1½ hours for lunch (he only got 1 hour) so that I learned to specialize in meals that took only 30 minutes to prepare. With gooseberries at 10d. a stone (14 lbs!) we made jam and on Christmas Eve we hung around the Market until it closed at midnight and got 2 geese for 5/– in the Dutch auction, of which we promptly sold one for 2/6 and went home rejoicing, with the other. I was not a vegetarian in those days, but I was very happy! My husband set up some apparatus of his own design at home with which to measure the torsional strengths of metal wires, for his Ph.D. degree; and while he experimented I did crystallographic calculations.

Professor Whiddington had welcomed me in the Physics Department at Leeds University, where W. H. Bragg had formerly been the Professor and where he and W. L. Bragg had carried out their early and intensive studies of crystal structures. The advent of World War I had prevented the establishment of any research school there, and Whiddington was interested in a different field, so I had once more to build up the X-ray diffraction equipment with the bits and pieces I could find and with the help of an apparatus grant of £150 from the Royal Society. A radiatory-type Coolidge tube cost £46, a Bragg ionization spectrometer with gold-leaf electroscope, microscope and two goniometer heads was £67.4s.6d., patterns and castings for photographic apparatus came to 12s.6d., and I returned the balance of £46.3s. to the Royal Society in 1929. C. K. Ingold, then Professor of Chemistry in the University of Leeds, had offered me some large crystals of hexamethylbenzene and although these turned out to be triclinic, they formed the subject of what was, perhaps, my most fundamental and satisfying piece of research. There was only one molecule in the unit cell and it was possible, without any assumptions except that there were 12 carbon atoms present in the unit cell, to show that the molecule was effectively hexagonal and plane. I have a treasured letter from Sir William dated 30 October 1928, in which he says: 'I think your new result is perfectly delightful: many compliments upon it! I like to see the benzene ring 'emerging'. I thank you too for sending me a special account of your work: it is very welcome. I have been showing what you have written to Muller and Cox and Sir Robert Robertson and there was great interest. I will write again about it before long: this is just a note of acknowledgment.'

While I was away Sir William wrote me a number of letters, often by hand, although he had a devoted secretary, Miss Winifred Deighton who stayed with him throughout his time at U.C.L. and the R.I., over 20 years, and of whom I was rather afraid. She was tall and impressive-looking—and I was not!

In 1929 my first baby came and I found it rather difficult to do everything in the home and also find time for 'Arbeit'; so I wrote to W.H.B. and he persuaded the Managers of the Royal Institution to give me a grant of £50 for one year with which to hire a daily domestic helper. Her name was Mrs. Snowball (it really was!) and, with her to wash and clean, I managed to care for the baby, cook and continue the structure analysis of C_6Cl_6.

In 1930 my husband got a new job at the Road Research Laboratories near to London and we moved back. I was now expecting another baby and I spent most of my time finishing off (by hand, of course, using log tables) the Fourier analysis of hexachlorobenzene. In view of the oft-repeated claims of organic chemists that the crystallographers told them nothing that they did not already known, it may be interesting to reprint a more generous tribute from C. K. Ingold (dated 2 November 1931).

'Ever so many thanks for the reprint of your wonderful paper on Hexachlorobenzene. I have just been studying it in the Royal but am very glad to have it in a handy form. The calculations must have been dreadful but one paper like this brings more certainty into organic chemistry than generations of activity by us professionals.'

I have some correspondence to show that I was a little worried about the triangular contours of the carbon atoms (*Proc. Roy. Soc.* A *133*, 536 (1931)) and wondered whether these were an artifact due to termination-of-series errors, as they almost certainly were. I was also engaged in correspondence with Astbury, W.H.B. and others about a comprehensive book on Crystal Structure Analysis that we proposed to write (this was later abandoned in favour of the first *International Tables*), and on calculations of the 'structure factor formulae' of the 230 space groups, later published as *Structure Factor Tables, 1936* in photo-litho-printed firm from the handwritten MS: 181 quarto pages for 10s.6d.

In November 1931 Sir William wrote to me again 'A piece of good news: Sir Robert Mond is giving me £200 with which you are to get assistance at home to enable you to come and work here. Can you come and see me soon?' I did some hasty sums and produced an 'Estimated Annual Budget' which convinced me (and him) that £200 was not enough.

Estimated Annual Budget

	£.	s.	d.
Wages of Nurse-housekeeper	60.	0.	0.
Board and lodging, uniform and insurance for nurse-housekeeper	52.	0.	0.
Extra charring and laundry	26.	0.	0.
Extra cost of housekeeping (at least)	26.	0.	0.
(This includes more extravagant use of fuel, light, power, food, and the cessation of my own activities, such as gardening—fruit and vegetables—, bread and cake-making, jam-making, and dressmaking for the children)			

Personal expenses:	
Fares (48 weeks at 10/– weekly)	24. 0. 0.
Lunches (less cost of my lunch at home—48 weeks at 8/9 weekly)	21. 0. 0.
Extra cost of clothes (5/– weekly)	13. 0. 0.
Books, notebooks, equipment, subscription and all extras	26. 0. 0.
	248. 0. 0.
Plus income tax	29. 0. 0.
	277. 0. 0.

I think he got me £300, but by now my careful keeping of weekly accounts was going to the dogs and I have no record of my exact salary; I only know that I rejoined him in the summer of 1932 and left again (but only temporarily) in 1934 for the birth of my third and last child. On my return there was no X-ray apparatus to spare and it was suggested that I might be interested in a huge old electromagnet instead. I was. K. S. Krishnan had just published some fascinating papers on the diamagnetic anisotropy of aromatic crystals and I at once threw myself into this new field. Krishnan had shown that the method provided evidence of molecular orientation; I confirmed and extended this to aliphatic compounds and showed also that by using the simple Larmor-Langevin equation one could calculate the effective radii of the σ and π electronic orbits in aromatic compounds and show that the latter were of molecular dimensions (*Proc. Roy. Soc.* A *159*, 149, (1937)).

Early in 1940 Dr. I. E. Knaggs showed me some Laue photographs that she had taken of benzil $(C_6H_5 \cdot CO)_3$, using unfiltered radiation from a Cu target. On these there was a beautiful and intense trigonal pattern of diffuse scattering; and on looking at our collections of Laue photographs several of us found similar though less intense patterns from other substances. We wrote a letter to *Nature* about it (*Nature*, *145*, 820, (1940)) in which we questioned whether the extra reflections found on Laue photographs of diamond by Sir C. V. Raman and P. Nilakantan could really be given the explanation he had offered. This pulled me into two fields which still interest me greatly today: on the one hand that of thermal vibrations in crystals and on the other the behaviour of diamond under different conditions.

It happened that about this time Dr. A. Muller was absent from the R.I. for some months through illness and I was able to have the almost exclusive use of the 5 kW and 50 kW X-ray tubes that he had developed, as well as the invaluable assistance of H. Smith (Smithy), who was one of the few outstanding laboratory technicians who subsequently achieved an obituary in *Nature*. Another X-ray tube for which I was indebted to Dr. Muller was a very small tube, originally intended for X-ray microscopy, giving a wide-angle divergent beam. With this I was able, without the use of precision apparatus of any kind, to measure the C—C distance in individual diamonds from their divergent-beam patterns with an accuracy of 1 in 70 000 and to show that they could vary between themselves by as much as 1 in 7000, from a very pure gem specimen to a highly impure industrial diamond.

Even after 40 years as an X-ray crystallographer I am not yet as old as Sir William Bragg was when I joined his team in 1922, and I hope therefore that I still have many years of usefulness ahead of me, if I have managed to catch anything of his enthusiasm and ability for clear thought and concise exposition.