My Time at the Royal Institution 1923-27

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I would like to add some of my personal recollections to supplement to some extent those of Kathleen Lonsdale.

These early years at the Royal Institution were certainly the most exciting and the most formative of my scientific life. We who had the privilege to be among the first of those who worked there were quite exceptionally lucky because we were at a point when a new field, the arrangement of atoms in crystals, was just being worked out. No one will ever have precisely that kind of excitement again now that the main types of crystal structures are known. Just because we did not know what to expect, every discovery opened up new possibilities. For instance, Sir William's and Sir Lawrence's work at the outset of the analysis of crystals like rocksalt seemed to have destroyed the idea of the chemical molecule. Sir William's later work on the organic compounds restored it.

There were really two experimental schools of work in the Davy-Faraday Laboratory, those of the old hands following the Bragg method—Kathleen Lonsdale herself and Astbury, for instance—based on the use of that very exact tool, the ionization spectrometer, and those who were developing for the first time the new method of X-ray photographic analysis, incapable of giving anything like the same accuracy of intensity estimations but able to cover a far larger number of reflections.

I myself remember being given a few days of instruction on Kathleen Lonsdale's spectrometer and deciding that I was not made for it: to spend a whole day for only two accurate reflections was quite beyond my patience. So I was put onto a very different apparatus, not something that I just had to sit down to, but something which had to be made from the very beginning. In my small room I was given a few pieces of brass made up by Jenkinson, some miners' lamp glasses, a little aluminium foil for the window, plenty of sealing wax to stick everything together. I was given some glass tubing and a little mercury to make a diffusion pump, some copper and iron wires for the transformer and as an essential ingredient an aluminium hot-water-bottle and a small piece of platinum to make the interrupter.

I had, of course, the invaluable guidance and assistance of Shearer, the designer of the efficient gas tube, and of Müller, who had the general direction of the younger arrivals. He was most helpful but also extremely abusive if one did things wrong, which I was continually doing. I remember his unprintable remarks when the Winchester bottle in which we kept the back vacuum broke with a loud explosion, or, rather, implosion.

It was after three months when I had not been able to get even the trace of an X-ray out of the apparatus, after endlessly burning myself and breaking things, that I decided that experimental physics was not for me and I went up to Sir William with the plea to be allowed to go back to the *theory* of crystallography. It was the first time I had seen him since I had been taken on. I had done a paper on the 230 space groups which had given me my job. But when he said 'You don't think I *read* your paper?' this, somehow or other, cheered me up enough to go back and finish the work and in the end to get out the structure of graphite and have it printed before the end of the year.

To do that I had to make my own cylindrical camera which I did in the most amateur way out of a piece of brass tubing which I had cut with a hack-saw, bored a hole in it, stuck in with sealing wax a smaller piece of brass tubing with two bits of lead with pin holes through them for the aperture. The film was held together in place with bicycle clips and I used an old alarm-clock and a nail to mount and turn the crystal. It worked and remained as a prototype of all existing cylindrical cameras as is shown by the fact that the diameters of the cameras have remained essentially the same ever since.

The hazards of work at the Royal Institution can be illustrated by what happened at the crucial stage of my graphite investigation when I had with great trouble managed to secure a few flakes of graphite from the Museum of Natural History, very much begrudged by the Curator, Dr. Spencer. I made a selection under the microscope and put them out on a nice piece of paper by the window and then went down to eat my sandwich lunch with the pingpong players. When I came back the graphite crystals had completely disappeared! I could not make out what had happened and finally I tackled the charwoman and asked her whether she had seen them. She said 'Them smuts?' (there were quite a lot which used to come through the window) 'I swep' 'em up.' So I had to start all over again. Mrs. Dyke had an extreme and quite unnecessary interest in laboratory cleanliness and I was not entirely upset when very shortly after that she tried to clean the X-ray apparatus while it was going. The terminals always collected an enormous amount of dust and she could not resist it but, she said! 'It jumped out at me, like.' She was lucky, I suppose, to have survived!

Working a Shearer gas tube, with its hand adjustable leak, was more art than science. Some people, like Gibbs, could get their sets to work for days on end without any trouble but I had to sit over mine all the time. It had a disconcerting habit of suddenly going hard or soft. At longer intervals the interrupters would develop leaks which would lead to their filling up with explosive gas and going off with a loud bang. In the end we found the noise was intolerable, so we put them up on the roof which of course made it impossible to keep an eye on them; you just had to wait for your bottle to 'pop off' and then go for an expedition among the chimney pots to replace it.

What was especially good about the laboratory was that it worked as a group of young people who were quite unashamedly keen at discovering this new world of molecular and atomic pattern. We did not shut ourselves up but wandered in and out of each other's rooms seeing how the work was going, talking all the time, not only at tea time, and I had the pleasure of doing vicariously a great deal of research. I always felt I was saved from the worst trouble, that of actually writing out the papers.

An element of laboratory life which Mrs. Lonsdale has not mentioned, were the colloquia which were instituted, I seem to remember, about 1925, where we were permitted to go up into the sacred regions of 'the flat' and, in Sir William's study, sit down on easy chairs and discuss the work of one of our number with continual comments and interruptions, particularly from Astbury.

The characters, scientific and otherwise, of the earliest workers were themselves markedly different. Kathleen Lonsdale, despite her unobtrusive ways, had such an underlying strength of character that she became from the outset the presiding genius of the place; her opinions were sought for and her judgment was always respected. Some of the research workers, like Gibbs, were very quiet and unassertive. His admirably executed work on the structures of the various forms of tridymite, only stable in narrow ranges of temperature, has never been improved on to this day. In contrast, Astbury was slap-dash and imaginative with unflagging enthusiasm which kept us going. At a time when there was no possibility of working out the structure of compounds much more complicated than naphthalene, he willingly attacked the totally unknown field of proteins where even chemical analysis was absent. Crazy experiments were the rule. I remember very well taking an X-ray photograph of the leg of a live frog stimulated by an induction motor to see whether the contraction made any difference to the X-ray pattern. The frog was released later and seemed to be none the worse, but the patterns, of course, were quite undistinguishable, just typical protein patterns, though really we would not have recognized it then.

Research workers came and went. They were accommodated wherever an inch could be found for them in the old building. I remember Patterson settling down as best he could between the glass cases that enshrined the historic apparatus.

After graphite, I did very little more experimental work at the Royal Institution. I was more interested in developing methods of analysis and this led to preparing charts both for flat and cylindrical cameras which involved a lot of calculation. I remember asking Sir William whether I could have £20 for the hire of an adding machine but as this was apparently beyond the capacity of the Institution, I had to do the whole thing by hand and eye using seven figure logarithm tables. It took me about two months: I suppose it would be equivalent now to about a minute machine computing time.

Almost the last experiment I did at the Institution concerned the structure of alloys and here I remember well working in Faraday's own cellar in the basement, still festooned with flasks full of Dewar's rare gases, but an admirable low-temperature place where one could crystallize things slowly. I was trying, by a flotation method, to measure to several points of decimals the density of a very small single crystal of a delta bronze. It involved a great number of weighings and measurings and on the very last weighing on which all the rest depended, the little crystal slipped out of my fingers and disappeared. An eighteenth century cellar is not the best place to look for a crystal of less than half a millimetre in size and I never found it again.

In spite of all these difficulties I do not think that any of those who were then in their twenties would ever have wished to work anywhere else. As Mrs. Lonsdale has said, we had to be kicked out!