which he and his co-workers were studying, someone asked him how he managed to hit on the correct structure from among so many equally plausible ones. He replied to the effect that he asked himself what the Almighty would do if he were putting together the atoms in the structure and that this turned out to be pretty accurate.

Life in the Physics Department at Manchester was close-knit and pleasant. The custom of afternoon tea was of course followed, with Bragg presiding, and no one would willingly miss it, whatever else he did, if he could possibly manage to be there. In many ways I was sorry to leave when W. A. Wood and I were offered jobs at the National Physical Laboratory in 1927, but as I had long been eager to work at the N. P. L. and as our task was, under Shearer, to set up a new X-ray analysis section, it looked too good to miss. And so it proved.

It was with some pleasure, then, that I heard of the appointment of Bragg as Director of the National Physical Laboratory in 1938. He brought with him Bradley, with whom I was very pleased to renew my friendship, and Lipson, whose friendship I have valued ever since. Bradley and Lipson joined the Metallurgy Department, where Preston was already working, while Shearer, Wood and I were in the Physics Department; but, needless to say the contact between the two groups was pretty close.

At that time I was working on the examination of the structure of dental enamel and dentine, by X-ray diffraction, micro-radiography and optical birefringence, and, once again, I was to experience the painstaking help which Bragg gave to his staff. He agreed to communicate a paper of mine to the Royal Society; and I have never forgotten his detailed criticism of the draft, which must have cost him a great amount of time and trouble, and certainly made for a much better paper.

But Bragg's time at the National Physical Laboratory was all too short; and in a year or so, he succeeded Rutherford at Cambridge, taking Bradley and Lipson with him. Then came the War which, for me, brought a complete break with my former work and thrust me into atomic energy as early as 1941, and then, *via* R. A. F. Bomber Command, back to atomic energy, at first in Canada and later at Harwell. Since then I have made contact with Sir Lawrence only at scientific meetings, on Committees, at the Royal Institution and so on. Of these I remember best the occasion of the meeting held at the Royal Institution in October 1952, to mark the fortieth anniversary of the discovery of X-ray diffraction. It was at this meeting, of which I was the organizing secretary, that von Laue made what was believed to be his first public speech in English. Many famous figures were present and Bragg was of great help in the arrangement of the historical exhibits, and in supplying me with material which I could use for the report I had been asked to write for *Nature*. On all these occasions I found in him the same keenness and enthusiasm and the same youthful approach that I remember from the first. Indeed it came as a great surprise to me to hear that his 80th birthday was imminent; and I am very grateful to be given this opportunity of paying my tribute to him.

Acta Cryst. (1970). A**26**, 184

Bragg, Protein Crystallography and the Cavendish Laboratory

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X-ray analysis of crystalline proteins and viruses was begun at the Cavendish Laboratory by J. D. Bernal in the middle thirties, some years before Bragg arrived there. Bernal headed the Crystallographic Laboratory, a sub-department housed in a few ill-lit and dirty rooms on the ground floor of a stark, dilapidated grey brick building. These dingy quarters were turned into a fairy castle by Bernal's brilliance and his boundless optimism about the powers of the X-ray method. He would occasionally tell Lord Rutherford, the Cavendish Professor of Physics, of his first crystallographic excursions into the fields of biology, but no echoes of these encounters reached us students. We were but a side show among the glittering spectacle of atomic physics that unfolded itself in other parts of the Cavendish Laboratory.

Rutherford's premature death in the autumn of 1937 started a round of musical chairs in British Physics. W. L. Bragg moved from the National Physical Laboratory to Cambridge to succeed Rutherford. P. M. S. Blackett moved from Birkbeck College, London, to Manchester, and Bernal succeeded Blackett at Birkbeck College, taking all of biological crystallography with him, except myself.

Bragg's coming was heralded by the arrival of huge X-ray powder cameras built for the study of metals; they were accompanied by A. J. Bradley, the new head of the Crystallographic Laboratory, and by his assistant, H. Lipson, who had unravelled the structure of complex alloys at Manchester. I felt forlorn among my haemoglobin crystals, doubly so because my native Austria had been overrun by the Nazis, my parents had become refugees, and the money which my father had given me for my studies was nearly exhausted.

I waited from day to day, hoping for Bragg to come round the Crystallographic Laboratory to find out what was going on there. After about six weeks of this I plucked up courage and called on him in Rutherford's Victorian office in Free School Lane. When I showed him my X-ray pictures of haemoglobin his face lit up. He realized at once the challenge of extending X-ray analysis to the giant molecules of the living cell. Within less than three months he obtained a grant from the Rockefeller Foundation and appointed me his research assistant. Bragg's action saved my scientific career and enabled me to bring my parents to Britain.

That was the first of many occasions when I was struck by Bragg's quick and imaginative response to a new discovery. The last one to happen at the Cavendish Laboratory came 15 years later, in July 1953. I had just developed an X-ray photograph from a haemoglobin crystal which had two atoms of mercury attached to each molecule of haemoglobin. The reflexions occurred at the same angles as those of the mercury-free crystal, showing that the two sets of crystals were isomorphous, but their intensities had markedly changed. At this moment we both realized that the phase problem was solved, at least in principle, and that the way was at last open to unravelling the structure of proteins by X-ray analysis. Two months later Bragg retired from the Cavendish Professorship and our close collaboration came to an end.

What happened in the intervening years is well described by Bragg himself in his article *First Stages in the X-ray Analysis of Proteins* (Bragg, 1965) or, as he sometimes prefers to call it, 'How Proteins were not Solved'. We tried a series of approaches which look hopeless in retrospect but which served at least to keep the pot boiling. There was my futile and extremely laborious attempt to solve the structure of haemoglobin from its three-dimensional Patterson map (though this did bring me Patterson's life-long friendship) and later our joint effort at tracing the molecular Fourier transform of haemoglobin in projection on the centrosymmetric plane of my monoclinic crystals. We hoped to do this by recording the $h0l$ reflexions at a series of swelling and shrinkage stages. Even had we succeeded in outlining the nodes of the transform and finding the signs of the loops, an image of the haemoglobin molecule in projection would have got us nowhere.

There are three papers which have stood the test of time. In two of them Bragg showed that all the X-ray data that I had collected from different forms of haemoglobin fitted an ellipsoid with the dimensions $65 \times 55 \times 50$ Å. He then proved that the transform of such an ellipsoid accounted for the changes in the intensities of the low-order reflexion which occurred when concentrated salt solution was substituted for water as the liquid of crystallization of my horse haemoglobin crystals (Bragg & Perutz, 1952*a*,*b*). The third paper was popularly known as the Liverpool Street Timetable. In it Bragg showed that the transform of any random function, such as the times of departure of the Cambridge trains from Liverpool Street Station, contained a set of loops which change sign only at certain minimum intervals. These intervals were determined by the width of the function: 24 hours for the trains or, in our case, 50 Å for the width of the haemoglobin molecule (Bragg & Perutz, 1952*c*). Bragg's principle of minimum wavelength enabled us to find the signs of the $00l$ reflexions from haemoglobin and later helped Caspar, Franklin and Klug to determine the signs of the equatorial reflexions from tobacco mosaic virus.

Bragg possesses a remarkable degree of physical insight into natural phenomena, especially those concerned with optics and the properties of matter. Once he has conceived a basic idea, he works out the details with lightning speed. In problems of X-ray optics, he would generally use physical arguments rather than mathematical ones. He would illustrate his conclusions in a series of neatly drawn sketches, and then write the accompanying paper in a lucid and vivid prose. Some scientists produce such prose as a result of prolonged redrafting and polishing, but Bragg would do it in one evening, all ready to be typed the next day, rather like Mozart writing the overture to The Marriage of Figaro in a single night. On only one occasion did he confess that he had to take an aspirin to go to sleep afterwards.

Bragg taught us to concentrate on problems of central importance, to approach them directly, to waste no time on trivialities, and to publish our results promptly, lucidly and concisely. He writes: 'When any major discovery in science is made, there is always a number of people who claim that the idea or the discovery was implicit in some previous work they or others have done; very often they are right. Such still-born children of the scientific brain abound, still-born because their author had not the art, the confidence, the enthusiasm and vitality to express his work in such a form that it had a living impact on the world of science. As I sometimes feel it necessary to remind young research students, we are not writing our papers for consideration only by God and a committee of archangels, but for frail fellow mortals. . . . Unless a paper has an immediate effect, it will almost certainly play no part at all in the progress of science and might as well never have been written. Papers of the last generation are only of interest to science historians. Here science is unlike the arts, where the value of original thought is often enhanced by time. Science is like a coral reef, alive only on the growing surface. The work of the past is of course the foundation on which further advance has been made, but it is dead, it has been replaced by a more complete understanding.'

Sketching and painting count among Bragg's recreations. He does charming landscapes in watercolours, and portraits of his family and friends in crayon or chalk, all drawn with sensitivity and acute perception. Anyone working with him soon discovers that his approach to science is also an artistic one. He delights in the construction of simple models to illustrate scientific concepts, such as his rafts of soap bubbles to represent the deformation of metals (Bragg, 1942); the X-ray microscope which was an optical device for forming an image of a structure from the observed X-ray diffraction pattern (Bragg, 1939); the fly's eye to simulate X-ray diffraction with light waves (Bragg, 1945); or his broomstick with a row of nails winding round it as a model of a helical polypeptide chain. It must have been his artistic, imaginative eye that made him conceive of X-ray diffraction simply as reflexion from sets of parallel lattice planes, instead of von Laue's mathematical formulation as a set of three simultaneous equations.

Bragg's foothold in the arts led him to try and bridge the gap between the Two Cultures by instituting an annual series of lectures in the humanities. These took place in the Cavendish Laboratory and made both students and teachers of physics more literate.

Bragg's period of office in Cambridge coincides with the decline of the Cavendish Laboratory as the world's leading centre of atomic physics. This was an inevitable consequence of the war and the transformation of atomic physics to 'Big Science', to which the tradition and structure of Cambridge University were ill-adapted. Rather than fight a rearguard action, Bragg decided to back two new applications of physics in which the Cavendish was again to lead the world.

One of these, the build-up of molecular biology, took place gradually after the war, as J. C. Kendrew, F. H. C. Crick, H. E. Huxley, J. D. Watson and V. M. Ingram joined us in successive years. At first finance was a nightmare. Bragg felt that we must not rely indefinitely on the generosity of the Rockefeller Foundation which had supported me from 1939 onwards. So he put me up for an Imperial Chemical Industries Research Fellowship and found some other temporary grant for Kendrew, but by 1947 these sources of support were approaching their end, and the University offered no help.

At this desperate juncture D. Keilin, the late Professor of Biology, suggested an approach to the Medical Research Council. In traditional fashion, Bragg met Sir Edward Mellanby, the Secretary of the MRC, for luncheon at the Athenaeum Club. Bragg explained that Kendrew and I were out on a treasure hunt with only the remotest chances of success but that, if we did succeed, our results would provide an insight into the workings of life on the molecular scale. Even then it might take a very long time before they would bring any direct benefit to medicine. Mellanby took the risk.

An MRC Research Unit was set up consisting initially of just Kendrew and me. By now this has developed into the MRC Laboratory of Molecular Biology which houses over a hundred scientists. Bragg's far-sighted backing was first rewarded in 1953, just before he left the Cavendish Laboratory, when Watson and Crick solved the structure of DNA; Huxley, by then at MIT, together with Jean Hanson, discovered the sliding mechanism of muscular contraction, and I solved the phase problem in haemoglobin. The first spectacular successes of protein crystallography came a few years later with the solution of the structure of myoglobin by Kendrew and his collaborators, and that of haemoglobin by my colleagues and myself.

The other new application of physics which Bragg helped to initiate at the Cavendish Laboratory was radioastronomy. During the war, Martin Ryle, a young Cambridge physicist working on radar, had become interested in radar operators' reports of signals from the sun and the galaxy. Joining J. A. Ratcliffe, a lecturer in physics who studied the ionosphere, Ryle constructed the first radio-telescope from ex-War Department components. Bragg realized the promise of Ryle's early attempts at detecting radio signals from outer space; besides, the location of these signals by interferometry intrigued him as a problem in optics. When, some years later, Ryle and Ratcliffe decided to set up a large radio-telescope near Cambridge, Bragg warmly supported their approach to industry which resulted in the endowment of the Mullard Radio Observatory. In time this led to the discovery of quasars and pulsars and the mapping of radio sources in the most distant parts of the Universe.

Bragg loves formal occasions, like College Feasts and Royal Garden Parties. Each year we used to have a Cavendish Dinner in one of the College Halls, attended by all the scientists in the Laboratory. Speeches were made and topical songs recited that had been specially composed. We knew that he was a keen bird watcher and at his last Cavendish Dinner we gave him a large pair of binoculars. Thanking us for this parting present, he said 'Whenever I look at a strange bird, I shall think of you'. With these words he left us, to become Fullerian Professor of Chemistry at The Royal Institution of Great Britain.

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