remains an enigma.' But the knowledge of nature's objects will never be final and unchangeable.

During my two years at Manchester many other silicate structures became known through the work of W. L. Bragg and his colleagues. The foundations of the system of silicates came to light through the efforts of the Manchester school, and Bragg's fundamental summarizing paper *The Structure of Silicates* appeared in 1930. By this work silicate science, which up to that time relied on transient hypotheses only, obtained a sound experimental basis. Of course, a great deal of further research has since been carried out elsewhere and great a number of new silicate types have been determined. None of these are in contradiction to the basic principles propounded by Bragg. The exceptional theoretical and practical importance of the silicates makes his achievements exceedingly valuable.

I determined at Manchester also the structures of apatite and apophyllite (the latter with W. H. Taylor), but I did not succeed with epidote. I also witnessed many important silicate and other structure determinations: danburite by Machatschki & Dunbar, KH_2PO_4 by West, the mica muscovite by West & Jackson, andalusite, sillimanite and analcite by W. H. Taylor, titanite, benitoite, thortveitite and hambergite, further $KClO_3$ by Zachariasen, and Na_2SO_3 by Zachariasen & Buckley.

The determination of many silicate structures and the silicate system of Bragg showed in a great number of instances, how greatly the application of X-rays may promote chemical research. Later on the structures of many other important groups of inorganic compounds were elucidated by X-rays, such as the structures of sulphates, selenates, tellurates, phosphates, arsenates, antimonates, germanates, borates *etc.* By these researches unknown domains of inorganic chemistry were opened. This process has been continous and is still going on; X-rays are an indispensable tool of modern inorganic chemical research.

On my return to Hungary I resumed my X-ray diffraction work at Szeged and later in Budapest and determined with my co-workers a number of new structure types.

Being one of the oldest crystal structure workers I often recall, and always with pleasure, the happy years at Manchester under the so friendly and inspiring guidance of W. L. Bragg and I hope he will be able to continue his splendid work for many years to come.

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W. L. Bragg – An Appreciation

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I was fortunate in being associated with Sir Lawrence Bragg for about fifteen years. In the early 30's Beevers and I used to come over from Liverpool to Manchester to seek his advice, which was always freely given. Later, he obtained a grant for me to work in his department and when he became Director of the National Physical Laboratory he invited me to accompany him to continue my work there; this was my first proper post. When he succeeded Rutherford as Cavendish Professor at Cambridge in 1938, he had me appointed to an assistantship there and I stayed in this position until the end of World War II. I was, therefore, exposed to his methods and ideas over a considerable range of activity.

His outstanding quality was – and still is – his ability to see the essential point of a problem and to strip away the inessentials. It was this quality that enabled him to dispense with the mathematical formality of Laue's theory of X-ray diffraction by a crystal and to substitute for it the simple effective law that goes by his name and which is still the central theme of the subject. It was this quality that enabled him to work out the first crystal structures, and then to tackle the much more complicated structures of the silicates – work that was coming to its end when I joined his department in 1936.

I was awarded a grant to work under A. J. Bradley on metal structures. At the time, flushed with the success that Beevers and I had had with the use of Fourier methods in the determination of the structure of copper sulphate and the alums, I must admit that I was disappointed at having to switch my mind to what appeared to be the relatively simple problems of metal structures. But Bragg had decided that metals and alloys were the next main field in physics to which the new techniques should be applied, and I can now see that he was right. I am sure that I learned much more physics from this new field than I would have done by continuing to work with structures of inorganic hydrates.

In this new work, Bragg's influence was indirect. His intuition lay in choosing the right man to lead the work – A. J. Bradley. Clearly, the powder method was the most important one for dealing with alloys and Bradley had a genius – I do not use the word lightly – for getting the utmost out of powder photographs. He devised cameras that are now too simple to be fashionable but which, even thirty years ago, gave good pictures with shorter exposure times than at present; he raised the accuracy of measurement to heights that have hardly been bettered today (to judge by the lattice-spacing project published in Vol. 13 of *Acta Crystallographica*) but, above all, he worked out crystal structures from powder photographs that then seemed incredibly complicated. His work with Thewlis on γ-brass and α-manganese must still be regarded as classics. His use of all the radiations from Cr to Zn ought to be an object lesson to research students who are brought up in the present-day worship of Cu$K\alpha$ radiation!

Bragg realized the importance of Bradley's work in relation to physics as a whole. It is worth while recalling the state of physics at the time: de Broglie had put forward his theory of the wave nature of the electron in 1924; it had been verified by Davisson & Germer and by Thomson in 1928; in the early 1930's its deeper significance was being explored. I can recall the frequent visits of Mott, Peierls, Jones and Hume-Rothery, ostensibly to give colloquia, but – as I can now see – to discuss the implications of the Manchester work. Modern textbooks make the subject of the metallic state seem so inevitable; if you know enough mathematics, the results must follow. They give no idea of the intuition that went into laying the first foundations.

Take γ-brass for example. Bradley had found that, by piling twenty-seven small cubes to form a larger unit cell and extracting the atoms at the origin and the centre, he could get an approximation to the structure. He then moved the other atoms to fill in the spaces so created. But there then appeared the remarkable fact that the movements created a new strong reflexion – 411 – which had the same spacing as 330. None of the other extra reflexions came anywhere near it in intensity. Jones seized upon this fact to show that an almost spherical Brillouin zone was being created. This was an important point in establishing the new theory of metals and in explaining Hume-Rothery's rules about electron–atom ratios in alloys.

Another of Bragg's interests was in order–disorder phenomena. He saw the importance of the work of Borelius on $AuCu_3$ and, with Williams, produced two theoretical papers on the subject; these are still quoted as the foundation papers in order–disorder phenomena. He inspired the work of Sykes & Jones on $AuCu_3$, which was the most thorough investigation of its time. $AuCu_3$ must be the most X-ray photogenic material ever examined; people are still finding more about it even now.

With all this academic work in progress, Bragg was keenly aware of the needs of industry. The financial support necessary for physics research is possible only if the country has a thriving industry; physicists, Bragg believed, should devote an appreciable part of their efforts to tackling practical problems. He was responsible for organizing a series of symposia on Physics in Industry, and one of these directed Bradley's attention to the problems of alloys for permanent magnets.

The 1930's were what might be called the 'heroic' period in the development of magnetic alloys. An accidental discovery had been made that the alloy Fe_2NiAl could have exceptionally high coercivity, but the reason was not known. X-ray examination of the alloy was not very informative, and so Bragg and Bradley decided that, to understand the behaviour of one alloy, the complete Fe–Ni–Al system should be explored. A. Taylor – the author of the well-known text book – undertook the work, and discovered that the alloy Fe_2NiAl lay in a two-phase region with two phases of similar structure.

Bradley cooperated with Sucksmith of Sheffield, who measured the magnetic properties of the alloys. A small box, containing six aluminum canisters, went backwards and forwards between Manchester and Sheffield, with the same packing. Only the label was changed! The work was extremely successful in its aim and considerable understanding of the behaviour of the alloys, and the effects of additions such as cobalt and copper, resulted.

When I came to Manchester, I worked on the Cu–Ni–Al system, and other research students worked on the systems Fe–Cu–Ni and Fe–Cu–Al. I remember the occasion when we made a model of the quaternary system in the form of a large tetrahedron, with the phase regions painted in different colours; this was for an exhibition at a Royal Society Conversazione. Inevitably, we had to race against time to finish it and when the time came to go to London, we were uncertain about one of the boundaries. So one of the research students was given the task of making an alloy, taking the powder photograph and finding out whether it was single phase or two phase; he was to telephone the result to us. This he did: the alloy was two-phase; but they were not the two phases that we expected!

When Bragg was appointed to the Directorship of the National Physical Laboratory, Bradley and I accompanied him. Since it took some time to bring our apparatus down and get it working, we could do no experimental work, and so Bradley devoted his time to writing a great deal of his research for publication; I am sure that, if it had not been for this enforced 'idleness', much of Bradley's work could never have been published.

Bragg also realized the importance of understanding the more fundamental properties of metals. Why are they ductile and what limits their mechanical properties? At Cambridge, he had discussions with Mott and with Orowan, and the beginning of dislocation theory was hammered out. It was then that he had another of the great ideas that show his inventiveness and his concern for simple explanation of complicated subjects – the bubble model.

The idea occurred to him when he noticed the rafts of bubbles that formed when he mixed the oil for his motor mower. What a good illustration of a crystal of an element they would make if they were all the same size! He realized that it is easy to make bubbles of constant size; they must be blown from a narrow orifice under soap solution by a constant pressure. Crowe – Bragg's research assistant – and I fitted up the arrangement and showed that it worked quite well. It has helped innumerable students to get a first foothold on this extremely important subject.

I hope that I have painted a picture of a really great physicist who could not only make a difficult subject seem easy, but could teach it in such a way that others could make use of it. I would claim that there is no other man in recent years who attained leadership in his subject and kept it over so many years. It was a privilege to have worked under him.

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Personal Reminiscences

BY J. THEWLIS

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I first met W. L. Bragg in 1922 as a student in the School of Honours Physics at Manchester University. He had then been Professor of Physics there for some three years and was, at the age of 32, very young as professors go. Indeed his youth was regarded with some pride by his students and the story was told, with an air of triumph, of how he had, on at least one occasion, played centre-forward for the Departmental hockey team.

By modern standards the Department was small. There was only one professor, but, in consequence, the students were known personally to him and were not mere names. Indeed he took a deep interest in his students' careers; and I shall always remember the fatherly talk he gave to me at the end of my first year when, like many others before and after me, I had found University life too interesting to leave room for very much work.

As a research student I came to realize that Professor Bragg was actively behind all the work being carried out in the now famous Manchester School of X-ray crystallography. Bragg himself was then pursuing his study of the silicates with its significance for the study of ionic radii, optical properties and modes of packing, James his intensity work, and Bradley his work on the structure of alloys from X-ray powder photographs, work with which I was myself associated, particularly in the elucidation of the structures of γ-brass and α-manganese. Brentano, the only other powder worker (the rest being concerned with single crystals), was working on the technique of focusing in powder photography and E. J. Williams had recently arrived to make his distinctive theoretical mark. But in a chapter of reminiscences it is, perhaps, not so much the scientific work as the people that are of interest. E. J. Williams I remember for his vitality and exuberance – there was then no hint of that disease which led to his tragically early death during the war. He was the moving spirit in a group of table tennis enthusiasts who used to play on the library table out of hours, a table which was vastly beyond regulation size and made for some heroic games. James I remember for his absolutely first class lectures to his students, and for his part in Shackleton's Antarctic Expedition, about which one could manage to get him to talk only rarely. He also, on one occasion, exhibited an unexpected gift as an actor, when at a Departmental party he surprised everybody from Bragg downwards by playing the part of a bearded German professor who had invented an 'ultra-ultra-microscope' which revealed individual atoms marching about with, I rather think, clogs on their feet.

Bradley was remarkable for his powers of concentration, his intuition in matters of structural arrangement, his amazing absent-mindedness, and his theories about life in general. We worked with a home-made X-ray tube held together with sealing wax and evacuated by a primitive mercury-in-glass distillation pump backed by a Gaede oil-pump, backed in turn by a hand-operated pump of unknown origin. When the tube got too hot the sealing wax would melt and the whole filament assembly would gradually be sucked towards the target – ability to detect leaks and skill with a small flame and a stick of wax were just as important as the ability to think of atomic configurations in three dimensions and to work out endless structure factors, corresponding to all possible (and some impossible) arrangements of atoms.

Bragg himself was interested in all that went on and, on his regular visits to the laboratory, we would invariably receive the benefit of some very pertinent remarks before he continued on his rounds. He, too, as we all know, showed a remarkable intuition regarding crystalline structures. Once, when he had successfully worked out one or other of the silicate structures