

William Thomas Astbury

1898-1961

W. T. Astbury was one of Sir William Bragg's earliest and most devoted disciples. Born of humble parentage—a fact of which he was always proud—in Longton, Stoke-on-Trent, in the pottery district of England on 25 February 1898 he received his secondary training in Longton High School from 1908 to 1916. Scholarships enabled him to go to Cambridge in order to study Chemistry. He was there 1916/17 and 1919/21, war service intervening. In both parts of the Natural Science Tripos he obtained first class, 1920 in Chemistry, Physics and Mineralogy, and 1921 in Physics. He immediately became Demonstrator in Physics at University College, London, under Professor W. H. Bragg who took him along as Assistant to the Royal Institution and the Davy-Faraday Laboratory when he moved there in 1923. Astbury remained at the R.I. for five years and was the soul and activator of the keen group of young workers that Sir William had brought together there (conf. J. D. Bernal and K. Lonsdale in Part VII). The reason for this lay in his unlimited enthusiasm for the new subject of crystal structure analysis, his temperamental approach, and the unexpected and sometimes provocative, but often most helpful turns in his conversation.

In 1928 Astbury came to Leeds on Sir William's recommendation, where basic research on the physics and chemistry of wool was to be started at the University. He became Lecturer in Textile Physics in 1928, Reader in 1937 and in 1945 Professor of a newly established Department and Laboratory of Biomolecular Structure. From the very beginning of his career Astbury stressed the close connection between the chemical and physical changes in wool, as in other fibres. By their X-ray diffraction effects these substances do not provide information of the same precision and in the same profusion as single crystals and this makes it necessary to combine with it all possible evidence that can be gleaned from their physical and chemical behaviour—a discussion that often requires great imagination. It was hereby that Astbury's unsinkable optimism helped him along where more anxious scientists might have feared to tread. The designation of the Department for Astbury was the first of its kind, and Astbury was proud of the name:

Biomolecular Structure; this type of name has since been adopted by departments or laboratories in other universities, British and foreign. As Astbury conceived it, it was to be a place where biological structure and texture on the molecular scale could be attacked in a catch-as-catch-can style, using chemical, physical and biological properties in conjunction with microscopy, electron microscopy, X-ray and electron diffraction and whatever else appeared hopeful. When I last saw Astbury in Leeds in 1959 I found him at the daily laboratory tea party, presiding over about eight co-workers, all of a very mature scientific stature, and leading as challenging and lively a discussion as ever.

Astbury's most important scientific contributions are, according to his own evaluation, his three papers on the Structure of Hair, Wool, and Related Fibres (*Trans. and Proc. Roy. Soc.*, A 1931–35), his studies on the Denaturation of Proteins (1935; poached eggs he used to speak of), and his Diffraction Studies of Bacterial Flagellae (1949, 1955). He wrote a book *Fundamentals of Fibre Structure* (Oxf. Univ. Press 1933), contributed a noteworthy essay on 'The Forms of Biological Molecules' in the presentation volume to d'Arcy Wentworth Thompson *Essays on Growth and Form* (Oxf. Univ. Press 1945), and gave the Croonian Lecture in the Royal Society in 1945 'On the Structure of Biological Fibres and the Problem of Muscle'. Besides, he was a prolific writer of papers in physical, chemical, biological, textile-technological, and general journals, and a great lecturer. The acceptance which his outstanding work found is documented in a long list of medals, prizes, and honorary degrees and memberships he received.

Astbury had undertaken to write for this volume, together with Dame Kathleen Lonsdale, about the early years of X-ray diffraction in England, and especially at the Royal Institution. He began writing, and among his papers a draft page was found which is so characteristic of him, in spite of the severe heart affliction which then already often incapacitated him, that no better description of his buoyant personality could be given than by adding it to this Notice. It will explain better than many words why Astbury was one of the most beloved among the early crystallographers.

P. P. Ewald

*Early days at University College,
London and the Davy-Faraday Laboratory
of the Royal Institution*

In sharing out who shall write what in these X-ray diffraction memoirs, the two 'oldest hands' available to recall those early days at U.C.L. and the R.I. were obviously Kathleen Lonsdale and myself, but what was not clear was who should do the actual writing. We could hardly say 'we remember' when it was sometimes only one of us who remembered, and anyway we like to think we have distinctive styles; so we compromised—I mean, she agreed to let me do the job. But it is to be understood, though, that it is a joint effort and unless otherwise stated, any particular memory or anecdote may be privy to either or common to both. For the purposes of this article we are to be considered, like the two unresolved heirs in 'The Gondoliers,' as a single unit.

I am the older hand of the two, in years and crystallographically, because I joined the staff of the Physics Department of University College in 1921 after graduating two years late through war service, while Kathleen (then Yardley) joined in 1922 after graduating two years younger than most people. I had read chemistry and (classical) crystallography at Cambridge besides physics, and I was also married in 1922, so this experienced old man ventured to take the precocious child under his wing, for a very brief start at least. It was a good case of the blind leading the blind (to mix the metaphor a little), but that of course was the fun of the thing. It has been well said that the greatest asset of scientific research is its naivety, and we must have been wonderful examples of that—all of us, not even excluding Sir William at his own high level. And before going any further let me make it crystal clear that Sir William—the Old Man, or Bill Bragg, as we called him behind his back—never 'led' any of us, in the technical sense. That was not his way—if he had a way—and if you were stupid enough you might even claim that you 'led' him, since, especially after we migrated in 1923 to the Davy-Faraday Laboratory of the Royal Institution, as often as not when he popped into your room (not terribly often) it was to ask *you* a question in connection with some lecture that he was due to give. Or you might meet him on the stairs and he would say: 'Hello! How's the family', or some such. He turned up at tea as often as possible, where we rarely talked 'shop' in any case.