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Acceptance of the Roebling Medal of the Mineralogical Society of America for 1989

HELEN D. MEGAW
22 Dunamallaght Road, Ballycastle, County Antrim BT 54-6PB, United Kingdom

It was a wonderful and startling surprise, a year ago, to receive a telephone call from the president, Charles Burnham, telling me I had been awarded the Roebling Medal. I had never thought of such a thing and was indeed delighted, and *very* grateful to all my friends who had thought of me and found my work worthwhile.

I had been looking forward all year to coming to receive it, and then just when I was working on my travel plans disaster struck. For no known reason I fell and cracked my femur. It was successfully pinned and a good recovery is predicted but takes time, and meanwhile travel to St. Louis is out of the question. This is a great disappointment, a double one—not only for my own loss of what would have given me tremendous pleasure, but for letting all my friends down.

I tried to make a note of some of the things I would have liked to say, for Professor Smith to tell you on my behalf.

I am particularly proud to be the first woman to receive this medal. Much has been said about the difficulties of women in science, but I would like to say explicitly that I at least was never or rarely aware of any discrimination. Perhaps I was particularly lucky, in that everyone who advised me on my education and guided my career assumed that women should be given the same opportunities as men. First and foremost I am thankful to my parents for this, and then to those far-sighted women of earlier generations who founded Girton College as a college for women, within Cambridge University and an integral part of it. Thereafter came my fortunate choice of a career in structural crystallography, a young subject, whose founding fathers, notably the Braggs, Ewald, and von Laue, were exceptionally good at inspiring an atmosphere of friendship and happiness among those who came to work in the field. This atmosphere persisted for many years through all the meetings of the International Union for Crystallography, and the X-ray Crystallography Group of the Institute of Physics. It was taken for granted that women played their full part, and there was never any suggestion of unequal treatment.

At these meetings I have made friends from all over the world, at all levels of seniority, each successive conference adding to their numbers. The numbers were again augmented by those I met on their visits to Cambridge, whether they came for periods of work, or just looked in for a day or two in passing. These friendships have been an important part of my life, and I regret that I am able only to give such a brief acknowledgment. In addition these friendships were an important stimulus. My ideas

only germinated and grew when I could talk to a variety of people—not so much for what I actually learned from them (important though that was) as for the stir and ferment set up in my mind by trying to express things, to argue and contradict, and hoping to convince others that there was something worth thinking about.

I think my interest in crystallography began at school, when I read Bragg's *X-rays and Crystal Structure.* On entering Cambridge University to read Natural Sciences, I expected my first-year work to consist of physics, chemistry, and mathematics, but I found that the regulations demanded a third laboratory subject. My director of studies at Girton College, Miss M. B. Thomas (a chemist, who would have liked to do original research if the circumstances of her young days had allowed it), suggested that I should choose mineralogy under Professor Arthur Hutchinson. I thoroughly enjoyed this, including his lectures on minerals, by which I was stimulated to my first contact with feldspars, hammering out a Carlsbad twin of orthoclase (which I still possess) from a stone built into

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a roadside wall, on a walk among the Mourne Mountains. I also greatly enjoyed crystal geometry and the formal techniques of crystal drawing. In my third year I went back to my earlier love, physics, but the professor, Rutherford, was unwilling to admit me for postgraduate work. At the time, it shook me; but I have been heartily glad of it ever since. Again, helpful advice came from Miss Thomas, who wanted younger women to have the research opportunities not available to her. She advised me to go to Professor Hutchinson, who accepted me for the Mineralogy Laboratory.

In the Mineralogical Laboratory, J. D. Bernal had recently been appointed assistant director of research, and I worked under him there, for four years for my Ph.D. Dorothy Crowfoot (later Dorothy Hodgkin) arrived two years after me. It was a most stimulating time. Bernal had a very wide-ranging mind, interested in every kind of knowledge and full of enthusiasm, which he communicated to those around him. His interests at the time infected me and remained with me for life: a love of symmetry, a fondness for the architecture of crystal structures, the packing ideas of Bragg, supplemented by the then-new directed-bond ideas of Pauling (in their geometrical rather than their formal chemical aspect). I learned much from the first two volumes of the *Strukturbericht*, edited by Ewald. Another important influence from Bernal was his concept of the hydrogen bond, not narrow, as the chemists would have it, but including what we then called the hydroxol bond.

For my postdoctoral years, Bernal encouraged me to go to Oxford, to learn some thermodynamics under Professor Simon (later Sir Francis Simon). I enjoyed my time there; I greatly liked and admired Simon, and learned much from him—but not a love of thermodynamics! At that time, University posts were hard to come by, and after two years I moved to school teaching, having had a fondness for teaching all my life. But one of the odd shakeups of the war years took me into Mitcham Works Laboratory, a very minor subsidiary of Philips of Eindhoven, then cut off by war. There, more or less by chance, I was asked if could I tell them the structure of $BaTiO_3$. That was easy, but getting permission to publish it was not. Thence I moved to Bernal's new laboratory at Birkbeck College, London, which was concerned with Ca silicates and cement minerals. But almost at once, in 1946, came the invitation to work in the Cavendish Laboratory, under W. H. Taylor.

Though I was sorry to leave Birkbeck just as things were getting started, the Cambridge invitation was irresistible (and Bernal, with characteristic generosity, would not stand in my way). It thrilled me enormously, not only because of its research aspects, but because it was held jointly with a teaching fellowship at Girton College.

Here let me state my firm belief that provision for the combination of research and teaching is vital for the progress of knowledge. Of course there are people who love one and hate the other, and they have their own roles to play, but it is essential that those who love both shall not be shut out and made to feel inferior by the organizational structures in universities or elsewhere, or the demands on their time made by a doctrinaire imposition of workloads. In teaching, when you explain concepts to your students, you clarify them to yourself, detect flaws in conventional thinking, and develop new ideas; and your research imbues what you have to say with an enthusiasm for discovery and a sense of first-hand acquaintance with progress that do, I think, rub off on your students and make them feel that science is alive and not just a compilation of facts discovered years ago.

In Cambridge I returned to my interest in the role of H_2O and OH groups in minerals, and I tackled the structure of the hydrated Ca silicate afwillite, which provided pretty examples. Next I thought a bit about the infrared aspects, with the help of Howard Petch (from Canada) and Norman Shepherd. With Christine Kelsey (McKie) I looked at the layer silicate tobermorite. Two others working in the Cavendish at about this time were Friedrich Liebau from Berlin and Joe Smith, whom I had taught as an undergraduate—and to whom I am indebted for many kindnesses, not least his willingness to speak for me in acceptance of the Roebling Medal and the way he did it.

Throughout these years, though I had had to leave $BaTiO_3$ behind me in Mitcham, I was still thinking about ferroelectrics and following the literature. I never *did* like Rochelle salt, considering it messy; but KH_2PO_4 was very pretty, with interesting hydrogen bonds. Then, at a conference in London, organized by Ubbelohde, I suddenly realized that I just couldn't accept the way other people were thinking about these transitions and twinning, in terms of thermodynamics and free energy. Back in Cambridge, I knew that my picture was much more in terms of structure, with specific changes of atomic positions or linkage. As this line of thought developed, I found myself at odds not only with the chemists but with the physicists, who wanted to explain everything in terms of particular electrical or electromechanical coefficients. That seemed to me to be going round in circles until one could derive the coefficients from the structure. But I had hard work learning enough crystal physics to be able to get my meaning across.

Then, partly through the help of Eric Cross, who provided me with material, came the opportunity to work on other perovskite structures, notably $NaNbO_3$. This I did with the help of research students and visiting associates, whom I mention in chronological order: M. Wells, I. Lefkovitz, L. Katz, K. Łukaszewicz (from Poland), A. Sakowski-Cowley, and (later) N. Darlington. I was no longer doing any of the experimental work myself, except for interpretation of X-ray photographs. But I should comment on the ingenuity of "Lef" in suggesting new devices (which he loved)—but his time was all too short.

About 1956, Will Taylor invited me to come in on the feldspar program, in which Bill Bailey and Bob Ferguson, among others, had already been working with him for some time. (I am delighted to learn that Bill Bailey is to

be the 1990 Roebling Medalist.) I'd never given much active thought to feldspars before then, just casually following progress; but I accepted with pleasure and thoroughly enjoyed myself. Will left me a free hand and encouraged my ideas even when they cut across his own. He and I and a succession of students and visitors, among whom were Bob Newnham, John Kempster, Ted Radoslovich (from Australia), Stephen Fleet, S. Chandrasekhar (from Madras), Peter Williams, John Waring, and (later) Paul Ribbe, consulted and discussed things happily; but all the time we were aware we were building on the foundations Will had laid. We had also the valuable stimulus of several NATO conferences, notably that in Manchester in 1972.

While I worked on feldspars, I never turned my back on perovskites. I was lucky to receive a new stimulus as I approached retiring age in the arrival of two postdoctoral assistants, Maija Ahtee from Finland, and Mike Glazer from Kathleen Lonsdale's laboratory in London. This was the beginning of a most fruitful collaboration. Mike absorbed my ideas on perovskites and developed them experimentally in ways far beyond my capacity and at the same time brought me a new awareness of the importance of lattice modes, which between us we were able to apply to the perovskites.

In mentioning names of coworkers, I am regretfully aware that my acknowledgments are incomplete; neither have I said anything of the many whose visits were shorter, but also very helpful. Of my indebtedness to all, I can only make this very brief mention. Many of those named have gone on to be leaders in the scientific field—many well known to you. But others chose to change direction and have made their mark in quite different careers.

However, I cannot complete these notes without saying, however inadequately, what a lot I owe to W. H. Taylor. He was a *very* good man to work with, or under, very straight and fair, absolutely reliable, always watching the interests of the people, students or visitors or colleagues, for whom he was responsible, and giving up a very large portion of his time to them. He thought ahead for them too, as well as for the lab as a whole, and had great generosity combined with a strong sense of justice. I was delighted that he should receive the Roebling Medal.

I must also express particular thanks to two others mentioned above. One is Joe Smith, who always kept in touch with us in Cambridge after he left for America and has maintained his friendship throughout all his progress, his undertaking to speak for me in the acceptance of the Medal, and the manner of it, being an example. The other is Bob Newnham, who also has been a steady friend to whom I am indebted for many kindnesses, most recently for his heartwarming and flattering citation. It is exciting, and a great pleasure, to see one's associates of earlier days going on to such heights of achievement as these two men have reached. Indeed, the work of Bob Newnham points out the bridge that always exists between mineralogy in the strict sense and materials science.

Perhaps I may illustrate that last sentence with a few remarks about my own habits of thought. These have always been in pictures, qualitative rather than quantitative, based on the conviction that you should know what you are talking about before you try to say how big it is! My pictures might be described in engineering terms, with semirigid interatomic distances, which may be bonds or packing contacts. I think one learns more from comparing occurrences in actual structures than from theoretical calculations. And here my unconventional juxtaposition of perovskites and feldspars, and my older interest in H_2O and OH groups, have given me a mine of material to work on. As a guide throughout one has symmetry and geometry, and here again I find that the simplest common-sense treatments have helped more than all the complexities of group theory.

To conclude, I can only say thank you again to all my friends for all you have done for me and meant to me.

As a postscript, may I be allowed to add that since retirement I have taken on a new interest, botany, or rather returned to an old love, of wild flowers and gardens, that has been with me all my life, but the freer rein now allowed to it gives me great happiness. One plant I specially chose to grow is *Perovskia*!