In this issue

Dorothy Hodgkin

The articles in the special section (page 447–489) on Dorothy Hodgkin in this issue are based on a one-day symposium held in her memory on 12 July 1996 at the Royal Society, London, organized by the British Crystallographic Association. The occasion marked the formal release of the *Collected Works of Dorothy Crowfoot Hodgkin* published in three volumes by the Indian Academy of Sciences. This issue of *Current Science* also reproduces the text of introductory articles in the *Collected Works* which provide a wide ranging view of her impact on crystallography worldwide.

I had the rare privilege of editing the *Collected Works* and the even greater privilege of having known her for three decades. This may be the place to look back upon my association with her*.

Seeking Dorothy

The *Bible* says seek and you shall find. Yes, I sought Dorothy since the early 1950s, when I was charged with and accepted the responsibility of building up a school of X-ray crystallography in Bangalore; But I found her only a decade later in 1964. When I had read most of her papers in the fifties I felt that she was foremost amongst those who were changing the face of X-ray crystallography, in that she was solving structures of substances whose structures were quite unknown, thus elevating the very status of the science of crystallography. I felt she could be the salvation of crystallography in India if Indian crystallographers could hitch their wagons to her. I longed that my students after obtaining their Ph D degrees in India could work with her, get inspired by her, come back to India and contribute, if possible, to the new wave in crystallography she had started.

Strangely enough and to my utter surprise I found that Dorothy had heard of me and my struggles in India with multiwavelength anomalous scattering techniques. She told me that she had heard of me from the late Gopinath Kartha (who met her in Oxford on his way to USA to work with David Harker on ribonuclease) and also, as far as I was concerned, most unexpectedly from the late N. V. Belov, the renowned Soviet crystallographer, who had met me in India in the fifties. She sent some gentle enquiries as to whether I could spend a year or two with her trying out the anomalous scattering methods in larger molecules, particularly in proteins like insulin. I informed her that she could do much better by offering a fellowship to my student K. Venkatesan (Van) who was just finishing his post-doctoral year with Jack Dunitz in Switzerland. This I thought could be the thin end of the wedge by which my students could join Dorothy and she too could have an opportunity of judging their quality and that of the Bangalore crystallographers. Dorothy did offer Venkatesan a fellowship. It was a successful experiment because since then 7 or 8 of us from Bangalore have worked in Dorothy's laboratory in Oxford and have tried to imbibe her spirit and culture of crystallography. I understood that Jack Dunitz himself strongly recommended Venkatesan to Dorothy for which I shall always be very grateful to him.

My going to Oxford to be with Dorothy is another story. I conjecture David Phillips (now Lord Ellesmere), who gave the beautiful inaugural lecture in the symposium, played a vital role in it. He came for a conference convened by G. N. Ramachandran in Madras. I was then at IIT Madras. As soon as David came he told me that he would like to have a long chat with me. I suspected that he had come to meet me on behalf of Dorothy. On a free day I arranged for David and myself to go to Mahabalipuram, one of the gorgeous beaches close to Madras. We had a long chat there. I feel that he must have given a good chit about me to Dorothy because a few days after his return I got a letter from her inviting me to Oxford offering me a senior fellowship saying that she would also need my help for looking after her Ph D students when she would be away in Ghana with her husband Thomas. I was greatly flattered with Dorothy's offer and I accepted it with alacrity and thanked my stars.

Looking after Dorothy's students was really no problem at all for they did not need any looking after. During my stay in Oxford I had the pleasure of seeing a group of young talented crystallographers flowering. Every week each one of them would come and tell me what they had done. I put on a wise look and made a few comments and I was told later by Dorothy that they were very useful to them. I also found that often the problems that were being tackled in Dorothy's lab by her students were indicative of her innovative thinking. For example, one of the problems was solving the structure of a comparatively small molecule in a crystal that displayed non-crystallographic symmetry. The solving of the crystal structure itself was by no means difficult but I feel what Dorothy had in mind was to investigate whether, after refining the structure, any phase relationships emerged due to the presence of the non-crystallographic symmetry. Dorothy's lab was one of the earliest ones to combine the photographic method and a computer-controlled microphotometer for the quick collection and indexing of crystallographic data. She foresaw that photographic methods may probably

*Based on an after dinner speech delivered at the Royal Society, London on 12 July 1996. The speakers at the meeting included Lord Phillips of Ellesmere, Prof. David Blow, Dr Bill Duax, Dr Peter Roach, Dr Phil Evans, Prof. Jenny Glusker, Prof. Tom Blundell, Prof. Guy Dodson, Prof. Jack Dunitz, Prof. Michael Rossman. Sessions were chaired by Prof. Louise Johnson, Dr Eleanor Dodson, Prof. Bob Williams, Dr Margaret Adams, Dr Max Perutz.

finally replace diffractometric ones, especially when data on hundreds of thousands of reflections had to be collected, indexed and their intensities measured. This work was done in her laboratory long ago and in my view paved the way for the later sophisticated methods which were developed afterwards and which are now in vogue. Dorothy's lab was amongst the earliest to cool protein crystals to reduce their being denatured by X-radiation. She was amongst the earliest to use computers for solving the structures of large molecules.

New phasing techniques

Dorothy was always looking out for new techniques of phase determination which she could apply in her work. It was only recently that I knew from David Sayre that it was Dorothy who invited him to Oxford to develop his now-renowned triplet method. When the Patterson synthesis came out she adopted it and mastered it. One has to read her Patterson Memorial lecture to know about her views on this subject and how she used this technique all her life. When the Bijvoet method of phasing using anomalous scattering was published she got into it heart and soul and tried it out on two fairly large non-protein molecules – cobyric acid and monocarboxylic acid of Vitamin B_{12}, which were in my view landmark structures to be solved *completely* using *only* anomalous scattering methods. Dorothy was obviously building stepping stones for her to use the Bijvoet method for solving the insulin structure. Here again many innovative techniques for using anomalous scattering were developed in her laboratory. For example, in zinc insulin, HgI_4 was introduced to see whether a cluster of anomalous scatterers would be of use in phasing. It was shown that it was not so useful. Also, new techniques were evolved for calculating the contributions of the heavy anomalous atoms alone, using simultaneously, anomalous and isomorphous data. This definitely played a major role in the final solving of the insulin structure. At the symposium Max Perutz has described in his inimitable manner how Dorothy attracted his attention to Bijvoet's suggestion of using the multi-isomorphic technique for phasing and how she even berated Max for not having picked this out from Bijvoet's famous 1951 paper and in having lost time in not immediately commencing using this method for solving the haemoglobin structure. We all know that the final solution of the structure depended much on this method.

What surprised me greatly was that as early as 1964-65 Dorothy discussed with me the three-beam interferometric method of using Renninger reflections suggested by Lipscomb and Fankuchen (1949), and earlier by Bijvoet and Macgillavry (1939). Most practising crystallographers would not even have heard of the method at that time. She, however, knew of the valiant effort Ben Post was putting in to make the method work which he finally perfected only in the late seventies. When she asked me whether this interferometric method could be used in the case of proteins, my reply at that time (1964-65) was 'If Renninger reflections could be observed in protein crystals then there was no reason why it could not be used. However I felt that the experimental methods involved may prove quite formidable'. I still do not know whether my answer was correct or not, but this method is now being pursued by many like Weickert, Hummer and others and they seem to have succeeded in determining the phases of a few intense reflections in lysozyme.

When anomalous scattering was discovered also in neutron scattering, Dorothy asked me to investigate whether this could be applied in the phasing of very large molecules. When I found there was a possibility of doing this by putting in an anomalous scattering isotope into the structure and using the appropriate neutron wavelength, she immediately got insulin crystals doped with the required cadmium isotope and put them in the neutron beam of the Harwell reactor. This was in spite of the fact that her mentor, the great J. D. Bernal, had told us both categorically that while the idea was basically correct the intensity of the neutron beams from the then existing reactors may not be sufficient to give any definite result. As expected, Dorothy's experiments proved to be unsuccessful. But to my mind this was of no consequence; it just showed the spirit with which Dorothy pursued new ideas.

Since 1933 Dorothy has been writing long critical reviews of crystal structures (see *Collected Works of Dorothy Crowfoot Hodgkin*, Vol. 3). The first one was in 1933, a 52-page article. In this she says, 'The methods of crystallography have not radically changed but in the past two years they have acquired additional certainty and precision. Fourier analysis which is so laborious has been undertaken in a few laboratories.' These reviews by her are so thorough and so critical that even the scientists who solved the structures would have gained much by reading her reviews. I feel that it was during this process of study for writing these reviews that she acquired her intimate knowledge of inter-atomic distances and bond angles in molecules under different conditions as also molecular configurations and orientations which proved invaluable in her later work.

In another review she welcomed the Patterson technique and explained it in some detail. She applied it almost immediately in its three-dimensional version for solving the structure of cholesteryl iodide. I remember learning all about the intricacies of the Patterson technique from her clearly written paper. She made it look so simple. She admonished crystallographers for not sharpening the Patterson maps which gives much more information and is much easier to interpret. She was a master of the Patterson synthesis. I am reminded of Max Born saying of C. V. Raman, 'His mind leaps over mathematics'. This could apply to Dorothy as well. Dorothy could deconvolute any Patterson map in her mind and deduce exactly where the atoms should be situated.

As soon as I came to Oxford I had the unique honour of sharing for three weeks the same office as Dorothy; indeed I also shared a part

of the extra large table she used. I sat on the other end watching her work. When she worked her concentration was incredible. Nothing or no one could distract her. She always lightly hummed a tune and *invariably* had a light smile on her face when working on a structure. She held a molecular model in one hand and sheets of Fourier maps made using approximately estimated phases and sheets and sheets of 3D sharpened Patterson maps in the other. She used to pick up the model and the Fouriers and the Pattersons and study them most carefully to derive the positions of the atoms. To do all this it would appear as though she needed more than two hands and that is why I was very impressed with Maggie Hamblin's insight in the painting of Dorothy – a picture which is now in the National Portrait gallery – in which she has represented Dorothy with many arms and she looks almost like one of the many-armed fierce goddesses of the Hindu Pantheon. We are told by those who know these matters, that goddesses are created by the Almighty to do a specific job which normal human beings cannot accomplish. In the case of Dorothy it was obviously for solving structures which most mortal crystallographers could not. But one aspect of this portrait I do not like is the fierce face Dorothy has been given. Dorothy's face was always calm with a smile. It always radiated peace and tranquility.

Suddenly the humming would stop and she would have a beatific smile on her face. This was indicative of her having plucked out a few atoms from the entangled web of Pattersons and Fouriers and placed them in their right position in the structure.

Now a few words about Thomas Hodgkin. One must read Dorothy's biographical and historical papers which are published in the *Collected Works* to see how much study she puts in to write these. They are filled with quotable quotes. In the biographical memoirs of Kathleen Lonsdale Dorothy quotes, 'For a woman to become a first class scientist she must first of all choose a right husband.' Dorothy no doubt chose extremely well. I knew Thomas Hodgkin very well. We became good friends and we spent hours together conversing and arguing. He had a sharp and a quick tongue but there was absolutely no malice in him. He was a scholar with a deep knowledge of history and the classics – also languages like Latin, Greek and Arabic. He had a remarkable sense of humour which could keep everyone laughing most of the time.

In preparation for a later story I must mention here that both husband and wife had the disconcerting habit of falling asleep when one was talking to them, thus sapping away much of one's self-confidence. When Dorothy went to Stockholm to receive the 1964 Nobel Prize, Thomas also went. In one of the Swedish newspapers there was a picture of Jean Paul Sartre who had refused the Nobel Prize and under the photograph there was a sub-title 'He was not there'. Next to it was another photograph with the legend 'Neither was he'. It was a beautiful and clear picture of Thomas soundly asleep during the prize-giving ceremony.

When Dorothy came to India as a Raman Visiting Professor, Thomas accompanied her. Unfortunately the Bangalore weather did not suit him and his asthma and emphysema worsened and he fell seriously ill. We continually telephoned his doctors in Oxford who told us that on no account must he be moved and there must be no jerky motions at all as his frail lung tissues may collapse at any time. We took all the requisite precautions. I will not tell you the story how as we could not move him into the clinic, as it were the mountain went to Mahomet; in that a big X-ray machine was brought from the clinic to the IISc guest house to take the X-ray photographs of Thomas' lungs. After sometime I noticed that Dr A. R. Pai, our doctor in attendance, after giving all the requisite medication was chanting *mantras*. He told me that all the possible efforts by human beings had been made and it was time to seek divine intervention and he requested me that I should promise to pay rupees thousand to the god of the Seven Hills, Venkateswara of Tirupati if Thomas survived this attack. I told Pai that I did not believe in all this. Pai's reply was exactly the one Neil Bohr gave when he was asked about the horseshoe hanging in his office – 'it works even if you do not believe in it'. Fortunately for all of us, Thomas became well and Pai asked me whether I had sent in the thousand rupees to the temple of Lord Venkateswara. When I said I had not, Pai said, contract is a contract, even if it is made in one's mind and so I must send rupees thousand immediately to the Tirupati temple, particularly as the party of the second part had kept up his end of the contract. So without further ado I sent a demand draft of rupees thousand to the temple at Tirupati*.

In 1982 we got a letter from Dorothy saying that Thomas had died. It was sad. When we saw her next, there was a change: something was missing, Joy had flowed out of her and she appeared far more pensive and serene.

Dorothy often said that she greatly regretted that the four persons – P. P. Ewald, J. D. Bernal, A. Patterson and J. M. Bijvoet were not awarded the Nobel Prize, as they were the persons who started it all and cut new and successful pathways in X-ray crystallography. She also was keen that the Pugwash organization should also be honoured by a Nobel Prize for peace. (This did happen in 1995.)

I have often been asked when I got the idea of publishing Dorothy's Collected Works. I remember the exact moment when this happened. When Dorothy and I visited North-Bengal University, in Siliguiri we were close to the foot hills of the Himalayas. Dorothy who never asked anything for herself said that she would like to view the Himalayas and see at least some of the high peaks. We went up to Darjeeling in

*There is a foot note to this story. When Thomas was passing through the crisis Dorothy had to give the inaugural lecture at a symposium. She appeared at the proper time, and reportedly gave a memorable lecture. No one would have guessed that she had just left her husband fighting for his life in the guest house just a kilometre away.

a hired ramshackle car. Dorothy could not stand even the heights of Darjeeling and was quite sick and I with foolhardiness did not cancel the visit. Early in the morning we stood on Tiger Hill and saw the sun before it rose, picking up at some of the highest peaks starting with great Mount Everest. Finally, we saw it rise and the Kanchanjunga appeared as a pyramid of gold bathed in the golden sunlight in front of us. It was one of the most magnificent sights I have ever seen, comparable only to two of Natures grandest spectacles I have seen, the Grand Canyon in the US and the Victoria Falls in Africa. It was at that time standing at Tiger Hill along with Dorothy, when she was viewing and surveying the high peaks of the Himalayas and I was seeing Kanchanjunga that I got the complete import of the sentences often repeated by the late S. Chandrasekhar, the renowned Chicago astrophysicist:

'The pursuit of science has often been compared to the scaling of mountains, high and not so high. But who amongst us can hope, even in imagination, to scale the Everest and reach its summit when the sky is blue and the air is still, and in the stillness of the air survey the entire Himalayan range in the dazzling white of the snow stretching to infinity? None of us can hope for a comparable vision of nature and of the universe around us. But there is nothing mean or lowly in standing in the valley below and awaiting the sun to rise over Kanchanjunga.'

When I was with Dorothy on Tiger Hill, I suddenly got the idea that budding young crystallographers of the world may gain much if at least some of the *selected papers* of Dorothy could be published. I did not ask her permission then. Six years later in 1985 when I went to her home, Crab Mill, in Ilmington I asked her and her answer was a categorical *no*. Dorothy said that she had so much work still left to do. She also said that if I took up this project I would ask her to write the introduction which she could ill afford to do at that time. Later in 1991 I again visited her in Ilmington. She had had a fall and had broken her hip-joint and she was immobile and confined to a wheel chair. She was a pathetic sight. One felt extremely sorry for her. She looked so fragile and the main discussion then with the health workers was how to give her a bath. When this engineering problem was solved she looked at me with a smile and said, 'I think you could now go ahead and publish my Collected Works.' I was extremely happy. Almost immediately she said with another charming smile that she would be leaving in a day or two for the Lindau conference of Nobel Prize winners. I was surprised and shocked and wondered how she could do it in her state of health. But she did go in the wheel chair along with her niece. One could not but admire her courage and her indomitable spirit.

It is said that one does not become a great scientist by tackling insoluble problems but scientists like Rutherford and Dorothy who changed the very face of science did take up problems which to most were obviously insoluble, but by their sheer genius they solved them. In this sense they justified Peter Medawar's definition of science as 'the art of the soluble'.

S. Ramaseshan

Understanding the sun

The sun has been held in awe and wonder in the human mind from time immemorial; all along the course of development of knowledge, many attempts have been made to properly understand this heavenly object. The early telescopes employed by Galileo spent considerable time for solving the mystery of the strange markings on the sun; remarkable periodicity which was noticed in their appearance had opened up a series of questions, which continue to the present day. Observations through the ages by employing numerous techniques and devices continued bringing out a flood of new information, keeping scientists busy in unfathoming the mystery of this heavenly splendour.

The pathway to gathering the data has not been smooth; there have been many hurdles in this task. The day-night cycle at any observatory has kept the sun hidden from view for half the time. To circumvent this limitation, scientists have established chains of observatories all around the globe; but still the Earth's atmosphere has always prevented an unhindered view, by disturbing images and absorbing bulk of radiations which bring messages from the sun. Finally an observatory has been floated in space, far out of our planet which now provides a continuous, unrestricted view of the sun all the year round. An account of this unique observatory SOHO, and some question for which answers are awaited appear in this issue (page 437).

J. C. Bhattacharyya

Subhas Mukerji

Subhas Mukerji was responsible for the birth of India's first 'test tube baby' in October 1978. Three years after the event, he ended his life in 1981. In the intervening period, the medical establishment aided by an uncaring and unresponsive government machinery, instituted enquiries conducted by committees with suspect credentials, used the weapon of 'transfer' and eventually discredited Mukerji and drove him to his end. Many years after the event, T. C. Anand Kumar (page 526) analyses Mukerji's scientific work and comes to the remarkable conclusion that Subhas Mukerji was indeed the 'architect of India's first test tube baby'. Anand Kumar is in fact credited with being the leader of the team that orchestrated India's first 'official test tube baby' in 1986. The disheartening tale of Subhas Mukerji should make us pause to reflect on the need for balance and reason in assessing scientific claims. Anand Kumar's essay, however, reassures us that the tradition of scholarship is still alive and well.

P. Balaram