Memories of India

D. Shoenberg

When I came to Cambridge as an undergraduate late in 1929, I knew very little about India beyond having read exotic stories such as Kipling's *Jungle Book* and, of course, almost daily news of the struggle for independence, with which I found myself in sympathy. There were not then many Indian students in Cambridge, but in my second year I got to know S. Chandrasekhar, then a recently arrived graduate student from Madras, through whom my acquaintance with India eventually became much closer. We were both attending a course by A. H. Wilson on the application of the then relatively new wave mechanics to the interpretation of atomic spectra and I was intrigued by this young Indian with such a strikingly intelligent look, who seemed to know the subject much more intimately than I did.

Over the next few years we became good friends and would meet fairly often over an evening cup of chocolate and talk about all sorts of things. I was particularly fascinated by the exciting ideas he was developing which took account of the fact that electrons in stellar conditions moved so fast that relativity seriously modified the usual Fermi degeneracy equation of state of an electron gas. Much of what he told me went over my head, but I gathered that the relativistic modification implied major modification of the theory of stellar structure. Chandra was greatly concerned that Eddington – then the leading authority in astrophysics – did not accept his point of view, though he was supported by theoretical physicists, such as Fowler and Peierls and by some astrophysicists, such as E. A. Milne. In spite of Eddington's doubts, Chandra was successful in the annual research fellowship competition at Trinity College. Election to a fellowship was a considerable distinction because a rather higher standard was looked for from candidates who had not been undergraduates at Trinity than the already high standard from those who had. Moreover, Chandra was the first Indian to be elected since the famous pure mathematician Ramanujan.

Some minor features of Chandra's style of work still remain vividly in my memory. One was his use of special sheets of paper for systematic recording of numerical data; he had great stocks of these sheets and let me have a generous supply for my own purposes which I continued to use for many years to keep a tidy record of my experimental data. On these sheets he would record the results to 6 significant figures of the numerical calculations he performed with a Brunsviger hand calculator to establish the results of his theoretical formulae. I remember asking him once what was the point of such precision when his theoretical formulae were at best only approximations to reality. His reply was that even if the theory might be only approximate, the functions involved in the working out of the theory were definite mathematical functions with precise numerical values. So why not calculate them as precisely as his calculator allowed, in case some future theoretician might need the higher precision for his own problems.

But although I continued to meet Chandra occasionally on both sides of the Atlantic and to admire the outstanding contributions he continued to make to the end of his life, his particular relevance to the theme of this article is that he introduced me to K. S. Krishnan, an old friend of his, who later enabled me to get much more closely acquainted with India. Krishnan had in 1928 collaborated with C. V. Raman in the discovery of the frequency shift of light scattered by liquids – what has come to be known as the Raman Effect. Soon afterwards Krishnan struck out on a line of his own and had acquired a considerable reputation not only for his experimental skill and ingenuity in the study of magnetic anisotropy, but also for his grasp of chemistry and crystallography in the interpretation of his results. Chandra persuaded Rutherford to invite Krishnan to give a few lectures about his work at the Cavendish Laboratory in the spring of 1937 and I greatly enjoyed Krishnan's presentation and the elegant demonstrations which illustrated how the torque on a single crystal in a magnetic field and hence the magnetic anisotropy could be measured in various ingenious ways.

At that time I was studying the de Haas-van Alphen effect in bismuth (the oscillations of magnetic moment as the magnetic field was varied) by the well-known Faraday method of measuring the force on the single crystal sample in an inhomogeneous field. The oscillations have a uniform period in reciprocal field and so became more and more crowded at low fields. Eventually, when the field interval of a period becomes comparable to or smaller than the variation of the inhomogeneous field over the sample, the oscillations get smeared out. It struck me in the light of Krishnan's lectures that measuring the torque in a *homogenous* field rather than the force in an *inhomogeneous* field might prove a better method of studying the de Haas-van Alphen effect. A convenient opportunity for trying out this idea occurred only a few months later when I visited Moscow to spend a year at Kapitza's newly built Institute for Physical Problems. I had been Kapitza's graduate student in Cambridge until, during a visit to his family in the Soviet Union in 1934, he was denied permission to return to Cambridge and had eventually agreed to set up a new laboratory for his work in Moscow (A detailed account of this rather extraordinary story can be found in Shoenberg[1]). I was happy to renew my connection with my former teacher and it occurred to me that setting up the torque method might be the ideal project for my visit. It was sufficiently simple to carry out in a relatively short time and yet sufficiently significant to be worthwhile. With the excellent facilities Kapitza had provided in his new Institute, the project got off the ground quite quickly. I was very lucky too in the presence of the outstanding theoretician L. D. Landau at the Institute, who developed the basic theory of the de Haas-van Alphen effect in a form which made possible the interpretation of my experimental data in a more fundamental way than would otherwise have been possible[2]. So, through Krishnan's inspiration, Kapitza's hospitality and Landau's theoretical guidance, I had in a few months effectively determined the Fermi surface of bismuth – I think the first Fermi surface

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of a metal to be determined experimentally and the forerunner of many more.

Because of the interruption of the war, my next contact with Krishnan came only about 10 years later. In the meantime he had become one of the leading scientific figures in India, a Fellow of the Royal Society since 1940 and knighted in 1946. Most recently he had been appointed Director of the newly created National Physical Laboratory of India and he received considerable personal support from Nehru in planning and setting up an impressive complex of buildings on the outskirts of Delhi. During a trip to the West in 1949 to pick up relevant technical experience and establish useful contacts, he visited Cambridge briefly and told me a lot about his plans for the new NPL. I was intrigued by his suggestion that I might come to Delhi for a year and set up a low temperature division in the NPL with facilities for experimenting at liquid helium temperatures and in high magnetic fields. Money from United Nations funds was available to buy a helium liquefier, a large electromagnet and other auxiliary equipment, and UNESCO would pay me a generous tax-free salary sufficient to enable me to bring my family for the visit and look after other essential expenses. The invitation sounded very attractive and I tentatively agreed to come in 1952 provided various facilities and miscellaneous pieces of equipment were made available ahead of my visit. However, although the helium liquefier and magnet were ordered in time for delivery in the summer of 1952, I could not get any information about what sort of laboratory accommodation would be available and what relatively minor items of equipments such as measuring instruments, vacuum pumps, glass for making Dewar flasks, supplies of liquid air and many other things, easily available in the West, would be available or, if not, would need ordering from the West well in advance of my visit. Krishnan just seemed to be unable to answer the many letters I sent him asking specific questions. So when, early in 1952, I received a formal letter from UNESCO in Paris informing me of my appointment as 'Low Temperature Expert at the NPL of India' from mid-1952 and setting out the generous terms of the appointment, I was sorely puzzled as to what I should do. A visit to UNESCO in Paris revealed that they knew even less than I did of the detailed situation at the NPL, but they agreed to finance a much shorter visit – six weeks or so in the summer – to explore the situation on the spot and see if I could arrange to provide for what was needed in time for a worthwhile year's visit starting in mid-1953.

This was my first visit to the East and in those days the air journey involved many stops, including an overnight stop at Karachi where the passengers slept in small cubicles at the airport. It all seemed rather exotic and I particularly remember my surprise at seeing small lizards running up the walls of the room. Delhi proved even more exotic with important looking turbanned servants in the luxurious hotel where I first stayed before moving to Constitution House, a large international hotel (sometimes facetiously called Constipation House) and later to a somewhat grander place called Western Court. When I came to the NPL to look for Krishnan, I discovered that he had just gone off to a conference in Japan but he had left instructions for me to use his luxurious office while he was away and left a message of greeting looking forward to meeting me when he returned a week or two later. In the meantime, with the help of K. N. Mathur (the administrative director) and K. G. Ramanathan, who had recently been my Ph D student in Cambridge and had been appointed to work with me in getting the low temperature division going, I set to and drew up long lists of all sorts of bits and pieces which were not available on the spot and mostly needed ordering from the West. Another important thing was to arrange suitable accommodation for the low temperature work. In fact, quite a generous space proved to be available – almost a whole wing of the NPL complex, but it was in a very primitive condition and considerable planning, building and decoration work was needed to make it a going concern. In particular, it was essential that at least the areas where the liquefier and magnet would go, should be adequately prepared in time for their arrival from Boston, USA, which was scheduled for only a few weeks after my arrival. In retrospect, I find it difficult to believe that I managed to get everything sufficiently on the way during those six weeks to feel that a year's visit from the summer of 1953 would be a practical proposition.

In due course, Krishnan came back from Japan and I met him as he was playing tennis early the morning after his arrival. When I asked why he had not replied to my many enquiries from Cambridge he looked a little embarrassed and smiled saying something like 'I'm sorry. I'm not very good at writing letters'. But it was delightful to meet him again during the remainder of my stay and he was very interested in my plans and very supportive. The most difficult problem of my six weeks stay was to synchronize the delivery of the liquefier and magnet to the NPL not only with proper preparation of the rooms where they were to go, but also with the short visit of the American engineers who were to set up the equipment and moreover to complete this before my own return to England about two weeks after the equipment was due to arrive by ship in Bombay. The lab administration assured me that getting the equipment from Bombay to Delhi would take at least three weeks so it seemed to be a complete impasse but I thought it might just be possible to do better by a personal approach. So, armed with suitable introductions from high-up officials in the appropriate ministries, I went to Bombay just before the ship docked and personally identified the relevant package cases in the hold and managed to get them loaded on to a goods train within a day or so. Soon after my return to Delhi, Milton Streeter and Walter Vossberg, the American engineers from A. D. Little arrived and made good use of the further

few days before the goods train arrived to make everything ready for the installation. There was even time for a weekend visit to the Taj Mahal. Eventually, a little less than a week after the ship had docked in Bombay, the equipment arrived safely at the NPL and then followed a hectic rush to get everything together and working in time. This effort was enlivened by many minor crises such as crucial small missing parts having to be improvised in the NPL workshop and a blockage in the magnet cooling circuit which proved to be caused by a dead rat. The culmination of all the effort came on the eve of my departure when the first helium was successfully liquefied and drawn off into a glass Dewar vessel. I have always had rather a weakness for marking significant events in a suitably symbolic fashion and for this occasion I had prepared a lead bowl and got hold of a small permanent magnet and was able to show off the well-known trick of making the little magnet float above the bowl when the liquid helium had cooled the lead to below its superconductivity transition temperature. Alas, by then it was nearly midnight and this symbolic demonstration was witnessed only by Streeter (Vossberg had already left after installing the electromagnet), Ramanathan, myself and one or two chowkidars and workmen. Krishnan and other top brass of the NPL had followed the preparations all day, but had sensibly retired to bed before the denouement.

During the following year, Ramanathan and a small group of young scientists and technicians had a busy time fitting out the research accommodation and getting familiar with the operation of the liquefier. The various auxiliary items I had ordered during my summer visit had arrived and by August 1953 when I returned to Delhi, the low temperature division was a going concern. My sabbatical year at the NPL was a memorable one for my family and myself. Our most abiding impressions were the warmth of the friendship and hospitality we received from the Krishnan family and from many others and also the overpowering warmth of the climate. My son Peter, then 9 years old, summed up the situation succinctly in a letter to a school friend in England after we had spent some hours being shown the sights of Delhi during the monsoon season. He wrote 'It is very hot; there are many tombs'. I acquired a smattering of what might be called kitchen Hindi, but I often got mixed up between similar sounding words, such as 'chapattis' – a delicious kind of Indian bread – and 'chaprassis' – the lowest grade of civil servants – who sit around in the corridors of Government offices waiting to carry things from one office to another. I remember causing great astonishment and perhaps some awe when someone asked me what I usually had for breakfast and I replied 'I like to eat one or two chaprassis with my coffee'. Early on in our stay we called on the medical adviser to the British High Commission to pick up useful tips about health precautions for the family. He was somewhat of a legend for his gloomy pessimism and displayed this to the full when we asked particularly about 'do's and don'ts' for our 3 young children. 'If I were you', he said, 'I should have left them in England'! This was a somewhat exaggerated comment, but in fact they did have various problems and as the very hot season approached in April we thought it wise for my wife to take the children back to England to avoid any more serious health problems. I was luckier and managed to stay out the full year without any dire consequences beyond suffering from the heat when out of reach of air conditioning.

Indian Government regulations were very generous in providing official holidays on days which were significant not only for the many various religions but for political reasons (e.g. Independence Day). By combining these official holidays with weekends and perhaps a few days of official leave it was possible to find time to do some fascinating sightseeing. While the family was still there we managed to visit places such as Agra, Jaipur, Mussouri and on my own I was able to get further afield and visit Hyderabad (for an Indian Science Congress), Kashmir, Bombay (to see my family board ship for their return to England), Calcutta and Darjeeling. The Calcutta visit was for a conference at the famous Indian Institute for the Cultivation of Science, and it proved rewarding because there I met Akshay Bose, who had once worked with Krishnan and had expertise in the field of paramagnetic salts. I persuaded him to come for a few months to the NPL and we got some interesting results[3] on the anisotropy of paramagnetic crystals in conditions approaching magnetic saturation, which could be achieved with the low temperatures and high magnetic fields available in the NPL.

Soon after I came to the NPL, helium was liquefied nearly every week and several lines of research got started. With J. S. Dhillon, I set up a torque magnetometer and we got some new results on the de Haas-van Alphen oscillations in bismuth and zinc, which later provided evidence for some subtle aspects of the basic theory[4]. K. G. Ramanathan and T. M. Srinivasan[5] developed new techniques for calorimetric and resistive measurements and two promising new graduates, P. N. Dheer and S. L. Surange[6] pioneered electronic techniques for measuring thermal expansion at low temperatures and looked for dimensional changes at the superconducting transition.

Although my year's visit in 1953/1954 for setting up the low temperature division at NPL was my most prolonged stay to India, I continued to visit occasionally and was happy to be warmly received by friends I had made during the year's visit as well as extending my acquaintance with people and places further afield. During a UNESCO-sponsored trip in 1959 I made my first visits to Bangalore and Madras. It was exciting to meet C. V. Raman at the Raman Institute and to enjoy the hospitality of his nephews Ramaseshan and Pancharatnam. I also met Chandrasekhar's father (C. V. Raman's brother) who gave me a long disquisition on his theories about Indian music (which, alas, went over my head) and in Madras renewed my acquaintance with G. N. Ramachandran whom I had known from his graduate student days in Cambridge. In Delhi I was welcomed by many old friends, but sadly this was the last time I saw Krishnan – he died suddenly only two years later. The other side of the coin was receiving old friends from India when they came to England. It was, for instance, a pleasure to get to know the Bose family better when they spent a sabbatical year in Oxford and later to have his daughter Malini and her husband Mihir Bhattacharyya to stay in our home when they spent a year in Cambridge on post-graduate studies in English. Another welcome visitor has been Krishnan's younger son, Raman, whom we had known as a teenager in Delhi. He came to study accountancy and

has settled permanently in England. He has married Vasantha, a Cambridge-educated biologist and they have a now grown up son, Krishna, who is a lawyer.

Other visits to India which were particularly memorable were in 1980, when I gave the Royal Society's Rutherford Lecture in various centres in India and Sri Lanka, and in 1988 when I gave the Krishnan Memorial Lecture at the NPL which I called 'Sealing Wax and String'. The title was intended to characterize the style of much of Krishnan's experimental work, which involved only simple equipment but was capable of producing significant results if used with ingenuity and skill – qualities with which Krishnan was so well endowed. To illustrate the theme, I set up a demonstration of one of Krishnan's elegant methods which he himself had demonstrated in his 1937 lecture in Cambridge. A crystal is suspended in a magnetic field by a very thin fibre and sets itself with its direction of strongest susceptibility along the field. If the top of the suspension is now twisted, typically by a few complete turns, the crystal gradually turns away from its original position until, when it has turned by 45°, it becomes unstable and turns rapidly and spectacularly to relieve the torsion of the fibre. The strength of the magnetic anisotropy is measured by the angle through which the top of the fibre has to be twisted to reach the instability. I had prepared most of the necessary equipment before I left for India, but, when I tried setting up the demonstration at the NPL, I was not skillful enough to handle the delicate suspension. Luckily, one of the young men, R. G. Sharma, in the low temperature division did have the skill and the patience which I lacked and the demonstration eventually worked perfectly.

Just before I gave the lecture I was a little taken aback to be conducted away from the lecture room and informed that I was to inaugurate the Krishnan Vatika – a rose garden in Krishnan's memory which was a very appropriate memorial in view of his great enthusiasm for gardening. The inauguration was made by my cutting a red ribbon across the entrance to the garden and I have always regretted that I was not quick enough to think of saying that I hoped cutting the red tape might be symbolic for the future efficiency of the NPL. Sadly, the bureaucratic methods introduced to India by the British so long ago have been maintained, even after 41 years of Independence and I can only hope that they will not last forever! My travelling days are over, but I shall always remember with affection the many benefits my contacts with India have brought me over the years.

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