Reaching for the sky

M. G. K. Menon

A tribute to Bernard Peters, who not only pioneered investigation of cosmic rays, but also established a thriving school and a great tradition of research in India.

When I was at the University of Bristol in 1949, doing research in the laboratory of C. F. Powell, I saw a paper which had appeared that year in *Reviews of Modern Physics*, entitled 'Evidence for heavy nuclei as a component of primary cosmic radiation'. This represented a major discovery and was the first evidence that the primary cosmic radiation had in it not only protons and possibly helium nuclei, but also complex, heavy nuclei. One of the authors of that paper was Bernard Peters. This discovery was made with nuclear emulsions sent up in high-flying balloons. In exploring the first detailed chemical composition of the primary cosmic radiation Peters and his colleagues found that it was very similar to the 'universal' chemical abundance of stars and galaxies. However, an important difference lay in the fact that in the primary cosmic radiation they appeared to observe the light nuclei, namely lithium, beryllium and boron, in much greater abundance than the universal abundance. This led them to conclude that the primary radiation had a composition similar to the universal abundance at its origin, but in its traversal of space before it arrived in the vicinity of the earth there were collisions with interstellar matter, resulting in fragmentation of heavy nuclei and creation of light nuclei, which therefore appeared in an abundance much greater than at source. This work opened up questions relating to the places where the cosmic rays originated; the distance in space through which they had traversed, including the matter density in this space and the time taken for traversal; and mechanisms for acceleration of heavy, complex nuclei. The group in Bristol was naturally interested in these fundamental questions that could be studied with the nuclear-emulsion techniques pioneered by Powell and his collaborators at Bristol. This represented a wholly new area of cosmic ray-related astrophysics in contrast to the great discoveries at Bristol in the field of elementary-particle physics. Peter Fowler and his colleagues David Dainton and Don Kent set out to work in this area, and came out with initial results in which they did not see the same high abundance of the light nuclei observed by Peters and his collaborators. This area thus developed into a very interesting one of different viewpoints for further work and analysis.

I then got to know that Bernard Peters had gone to India to make flights near the equator. This was to try to answer the question whether cosmic rays consisted only of ordinary nuclei or had in them antimatter; the approach was to make use of the so-called east–west effect. In this experiment, carried out in the tradition of the earlier expeditions to India for cosmic-ray studies by Millikan, Compton and their collaborators, Peters *et al.* could find no evidence for antimatter in the cosmic radiation. But it was important in establishing close links between Peters and Homi Bhabha. At the invitation of the latter Peters decided to accept a professorship at the Tata Institute of Fundamental Research (TIFR). He was not happy with the political climate in the United States, characterized by McCarthyism. Even more important, he identified himself with India, its culture, its people and its problems.

In 1953 I met Bernard for the first time at the cosmic ray conference in Bagneres de Bigorre in France. This time he came not to present results of primary cosmic radiation, but equally exciting results obtained with his colleagues Devendra Lal and Yash Pal using stripped emulsion stacks flown over India. They presented some very interesting results on the masses, modes of decay and 'associated production' of strange particles. As pointed out by Murray Gell-Mann at the Pisa conference (1955), I had first introduced this term (in unpublished correspondence) for the phenomenon. This work was of a particular interest to me, since I had been working on the masses, decay modes and interactions of heavy mesons at Bristol. Cormac O'Ceallaigh and I had presented at the Bagneres de Bigorre conference (1953), and indeed earlier at the Bristol conference (1951), that, apart from the well-known tau decay mode involving three charged pions, there were two other distinct decay modes of heavy mesons each involving a single charged secondary particle. In one of these modes, the charged secondary was a mono-energetic pion, and in the other a muon with varying energy. The work from the Bombay and the Bristol experiments were complementary and largely consistent, considering the accuracy of the observations at that time. I was very impressed with what had been done by Bernard and his group in Bombay in this highly competitive and new field.

In 1955 I had a formal letter of invitation from Bernard to join TIFR. This was a follow-up of the earlier discussions Bhabha had with me when we met in 1954 and 1955. I accepted the invitation because I wanted to return and work in India rather than work abroad, though there were excellent offers from various places in the US and Europe, some on a long term, and in some cases at an attractively high level. I also wanted to return to India for family reasons. I was convinced that it was possible to do front-line research on a contemporary basis at a place like TIFR, even in highly competitive areas. The examples of Bhabha and Peters were striking confirmation of this. (We did show this later also, in work deep underground in Kolar with the first evidence for natural neutrino interactions and the first really large-scale dedicated calorimetric detector to look for proton decay; the construction of and the results from the Ooty radio telescope and the plans for the giant metre-wave radio telescope (GMRT); the cosmogeophysics work; and much else.)

As soon as I joined TIFR one of the first things that Bernard and I discussed was the situation that work relating to elementary particles, of the type pioneered by cosmic-ray physicists until 1955, with the major discoveries relating to the pion, pi-mu decay, strange particles, etc., had now moved into the domain of accelerator physics. It was no use, on the basis of the past great tradition, to continue to work on an 'also-ran' basis. It was important to develop capabilities to move into other important aspects of cosmic-ray physics such as the primary radiation and extensive air showers. Bernard had been to the international cosmic ray conference at Guanajuato in Mexico in 1955, where he was deeply taken up with the large areas of astrophysics that could be explored through cosmic-ray studies.

For work on the primary radiation it was important to have balloon flights of long duration (of many hours), at very high altitude (of over 100,000 feet). This necessitated development of the plastic-balloon technique; the earlier rubber-balloon technique had reached the limit of what it could do for cosmic-ray studies. Bernard felt that we could rapidly make large plastic balloons by welding together polythene sheets to make long cylinders of great diameter, and then tying or sealing these properly at the top and developing a load system at the bottom. This approach did not work; but it led to rapid development of technology for sealing long lengths of polythene sheets, dealing with very large quantities of hydrogen used as filling gas, and the usual launch procedures for such large balloons. The approach was not successful because of the tremendous stresses at the top of the balloon, which had not been taken into account. Several of us (Gokhale, Balasubramaniam, Redkar and I) therefore worked in detail on the best shape for minimizing the stress distribution and experimented with designs that had been used at Bristol. We gradually worked our way to what is now known as the 'natural shape'. The entire balloon technology was unique in that one institution concerned with basic research, namely TIFR, dealt with all aspects of this technology: selection of polymer, extrusion of film of the right thickness and quality, balloon design, sealing, launch technologies, having to overcome the extraordinarily low temperatures encountered in the equatorial tropopause which led to brittleness of the balloon film and failure, tracking (involving radars), altitude measurements, recovery procedures—the whole gamut. Bernard had a responsibility in initiating this major effort. The early flight expeditions were full of hard work, failures, anxiety, humour and occasions for us to laugh at our stupidities, and good friendship. Today, very large balloons are regularly launched for cosmic-ray studies by the national facility set up for this in Hyderabad.

During 1955 to 1958, Bernard had two further major interests. The first related to the production of radioactive nuclear fragments with widely different half-lives as a result of the collision of cosmic radiation with atmospheric nuclei, and the use of these as tracers in a variety of geophysical domains. The pioneering isotope in this regard was, of course, carbon-14, which had been discovered earlier by Libby. Bernard decided to start studies on the long-lived isotope beryllium-10 (^{10}Be) and put a wholly new group for this work under Lal, whom he shifted out of nuclear emulsion work. They not only discovered, in deep ocean cores, ^{10}Be produced through cosmic-ray interactions, but were able to demonstrate its wide usefulness as a tracer in many geophysical areas. This led to work also on many other radioisotopes and their uses in the new areas of geocosmophysics such as meteorology, ocean sediment studies, land erosion, time variation of cosmic-ray intensities, planetary studies and the like: Bernard demonstrated tremendous courage in starting up work in a wholly new area involving chemical techniques to extract minute quantities of these radioactive tracers, and counting their exceptionally low-level activities.

His other area of interest arose through the work of the Russian group (Nikolsky *et al.*), which had indicated a possible change of behaviour of the primary cosmic radiation at energies around 10^{14} eV and above. Bernard felt that extensive-air-shower studies could be carried out in India to answer these and allied questions. He was interested not in the very large shower arrays to detect the highest-energy showers, but in detailed investigations of the nucleon and muon components close to the air shower axis in showers of less than 10^{15} eV, which were not that rare. (This programme, led by B. V. Sreekantan, has been a continuing activity over the past three and a half decades. They have used neutron detectors, large multiplate cloud chambers, total-absorption calorimeters, air Cerenkov counters, muon detectors, and, in experiments at Kolar, underground muon detectors, in interesting combinations to answer basic questions in this area.) Bernard was also interested in the development of models to fit the various properties of cosmic-ray radiation. He worked with Yash Pal on high-energy interactions and later with Westergaard he developed a 'closed galaxy' model, which has clear predictive properties that could be tested in experiments. This model envisages that particles of even the highest energies can be accelerated as well as contained within our galaxy.

It was only on getting to know Bernard that I discovered that he had worked initially on the separation of uranium isotopes in connection with the Manhattan Project, and that he was one of the authors of an early 1948 paper in *Physical Review* on the development of the frequency-modulated cyclotron with Ed Lofgren and others at Berkeley.

Bernard has thus truly covered a wide spectrum of physics. He was always prepared to think out completely new ideas and to get to work on them very rapidly. As a result he was responsible for spectacular results, such as the discovery of the heavy nuclei in the primary cosmic radiation, the first work with stripped emulsions which led to interesting observations relating to strange particles, the pioneering thrust relating to the use of a variety of cosmic ray-produced radioisotopes as tracers in various fields of geocosmophysics, and aspects relating to the origin, acceleration and propagation of cosmic rays. In my view he was a scientist of Nobel prize class. He built up in Bombay a very diversified and strong group in these various fields. S. Biswas, R. R. Daniel, B. V. Sreekantan, D. Lal, Yash Pal, K. M. V. Apparao, Rama and many others are members of this group, and Bernard had a great deal to do with training, nurturing and encouraging the group*. They have gone on to develop on their own, branching off into new creative endeavours, and continued the *guru-shishya* tradition. Thus Bernard's ideas, attitudes and enthusiasm will live on through these new generations of Indian scientists. When you met Bernard at first you might think of him as a gruff, aloof personality who could be very combative in his discussions. But as you got to know him you discovered a person who is extraordinarily affectionate, very warm and deeply concerned about people, their problems and welfare. And with Bernard you always had Hanna, his wife, so kind, affectionate, always smiling, and a mother to all his students and younger colleagues. Indian science has cause to be grateful to them for the years they spent in India and their commitment to the country, and for the programmes and people they developed and grew.

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*See articles in this issue: Peters page 717, Biswas, 724; Daniel, 719; Sreekantan, 718, Lal, 722 and 744; Apparao, 756; Rama 751.

The decade of my association with research in India

Bernard Peters

To the study of cosmic-ray nuclei and the search for cosmic ray-produced radionuclides, Bernard Peters brought hard work and new ideas.

My first contact with India occurred soon after it was established that the primary cosmic radiation (CR) bombarding the earth consists of a variety of different nuclei. An obvious but very important question arose: Does this, the only material accessible to us from beyond the solar system, consist of matter only, or does it contain also antimatter (i.e. atomic nuclei of negative electric charge)? As far as it is known, both matter and antimatter are always created together and in equal amounts. Why only one kind (i.e. matter) can be found in the solar system and beyond is still an enigma.

At equatorial latitudes the geomagnetic field produces an asymmetry in the CR. It prevents positively charged particles in certain energy intervals from reaching the earth from the easterly direction, and, at the same time, it prevents negatively charged ones (i.e antinuclei) from reaching it from the westerly direction. Thus, a very basic problem in physics and cosmology could be investigated by measuring the arrival direction of complex CR nuclei in the equatorial stratosphere.

I met Dr H. J. Bhabha in New York in October 1949 to discuss this problem. He had then already initiated a CR research programme at the newly created Tata Institute of Fundamental Research (TIFR), Bombay, and the programme included balloon flights into the stratosphere. Obviously, a cooperation on this fundamental experiment between the TIFR and the University of Rochester at which I was then teaching was indicated.

I arrived in Bombay on 31 August 1950, with an apparatus designed to measure such an asymmetry. It would keep airborne packages of nuclear photographic emulsions oriented in space, independent of wind-induced rotation of the balloon clusters.

An early letter to my family describes my first impression of the people and the conditions available for this cooperative experiment:

> My first impression of the Tata Institute of Fundamental Research is very favourable, much more so than I had a right to expect. I have met a good many scientists, people with knowledge and interest, whom one likes at first sight. The institute, housed in a former royal officer club, has so much space, it would make everyone in Rochester green with envy. The library is large and adequate. The electronic labs and the machine shops look good. It is located at the Bay, from which it is separated by a small park with flowers. Mr Godbole, the secretary of the institute has reserved my room at the Taj Mahal Hotel ... looking out over the ocean with fishing boats and islands in the distance. It is hard to describe how beautiful it is.... I have not broached the subject yet, but who can pay for such royal quarters? I certainly can't....
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> I have already talked to some of the physicists. I am very impressed and relieved that they have already made successful balloon flights to 95,000 feet for several hours with rubber balloons; so they know a great deal more about the subject of flying than I do. This was my greatest worry, so my hope for success is much increased. Their percentage of recovery, 60–90% return of equipment within two days, also sounds exceedingly good.... My first impression about facilities, competence of people, and their character is extremely favourable, but so far based on only four hours of contact.... Dr Taylor is the chief of the cosmic ray emulsion group, the division using photographic plates. He is half-time professor teaching at a college belonging to Bombay University and half time at the Tata Institute. An Englishman about 50 years old....

And:

> Bombay, Sept. 29, 1950
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> Taylor and I are flying to Madras on the 8 October, then drive to Bangalore, stopping at several places to pick out observation stations for theodolites. We will meet some of our crew in Bangalore, tranship equipment from there to Madras, and start flying on the 15.

A large number of rubber balloon flights were begun on 15 October from the cricket field of Madras Christian College in Tambaram. Another letter, of 22 October 1950:

> I was sitting in a moonlit night at a brick fireplace in the woods, cooking balloons from mid-night to 3 a.m. Then I woke up our group, and, as planned, at 5.40 a.m, twenty minutes before sunrise, our 24-balloon flight went off. It was the most elegant launching operation with 25 people involved. Many college students volunteered. The flight stayed up for at least 10 hours and was observed by our theodolite stations in Madras, Vellore, Kolar and Bangalore. Wireless communication and weather perfect. Except for the fact that the old balloons did only go up to 75,000 feet instead of 95,000 feet, it was a perfect flight. Our score is now: first flight 15 October, a failure but recovered. Second flight, 70–80,000 feet. Weather permitted only four hours observation, but judging from the place at which it came down, it probably was an eight-hour flight. I am leaving tonight for Kuppam to recover it. Third flight today, probably 10–12 hours above 70,000 feet. I have no doubt that we will get excellent flights if we have better balloons and more rope.

The flights continued into November.

Subsequent examination of the emulsions at Rochester and Bombay gave a clear answer: The region of the galaxy where CR originates contains matter only; less than one in a thousand of the nuclei consists of antimatter. This answer stands today. The upper limit for antimatter has since been lowered from 10^{-3} to about 10^{-5}. This result is quite surprising and disturbing, if one accepts big bang models of cosmology. Various attempts to explain the asymmetry are still quite tentative.

Peters (*right*) and assistant, producing hydrogen gas for balloons in high-pressure autoclaves, Tambaram, 1950

The enthusiastic work of the TIFR emulsion group led by H. J. Taylor* of Wilson College in Bombay and the

*See *Curr. Sci.*, 1990, **59**, p. 1267 for article by H J Taylor excellent help and cooperation we got from the Madras Christian College staff and students in carrying out this very arduous balloon campaign in Tambaram motivated me to accept Bhabha's offer to return to India for a longer stay. And so, having returned to Rochester at the end of 1950, I returned to India at the end of 1951 and brought my family.

The 1950 experiment was an auspicious beginning for my work in India, which should last throughout the decade. I realized then that the geographical position of India, combined with the facilities built up at the TIFR, presented a unique combination for research on many other basic problems related to CR, a fact which Bhabha had realized already when he founded the institute. The geomagnetic field at low latitude prevents the bulk of low-energy CR from reaching the earth's atmosphere, so that the very rare high-energy processes could be studied here without being swamped by background, a great advantage over the situation in the USA and Europe. It remained to identify feasible experiments, which could be expected to yield new and relevant results in high-energy physics. We chose to investigate the following problems:

1. The chemical composition of high-energy CR, especially a search for evidence that it may reveal traces of its prehistory, its acceleration by as yet unknown processes at unknown sources, and its passage through interstellar space. Are all atoms completely or only partially ionized before acceleration? In other words, what is the temperature in the source region? How many, if any, long-lived radioactive nuclei which may be present in the source have survived the transit to the solar system? How long have the particles been on the way, how much interstellar matter (mostly hydrogen gas) have they traversed?, etc.

2. What happens when CR nuclei of energy far greater than could then be produced in laboratories collide with other nuclei? How do complex nuclei then disintegrate? What are the collision cross-sections for the various disintegration products?

3. What unstable particles are created as the result of the prodigious energies released in these collisions? (Some of them were known to be pions which had been discovered a few years earlier by C. F. Powell in Bristol.) How many subatomic particles are created? What is their angular and energy distribution when they emerge from the collision centre and what can this tell us about the interaction of subatomic particles

Stimulating discussions

B. V. Sreekantan

The Tata Institute of Fundamental Research was founded by Homi Bhabha in June 1945, and immediately after that experimental cosmic-ray research was organized under three different groups—the high-altitude studies group under A. S. Rao, the nuclear emulsion group under H. J. Taylor and the cloud chamber group under A. B. Sahiar. I joined the cloud chamber group of the institute in August 1948, and as suggested by Bhabha I started to build fast-pulse electronics circuits and detector systems for a systematic investigation of μ-meson decay. In 1949 we moved from the Peddar Road premises, which was Bhabha's own house, to the spacious Yacht Club building next to the Gateway of India, and by the time Peters came in August 1950, the activities in all the three groups were in full swing and the institute had also started work in other areas like nuclear spectroscopy, under B. V. Thosar, and nuclear reactions, under R. Ramanna. Towards the end of 1950, Bhabha organized the first international conference on elementary particles, which was attended by many leading cosmic-ray physicists and theoretical physicists. Peters was already there in connection with his heavy-primary experiment carried out in Madras in collaboration with TIFR. Just around this time Bhabha suggested to me that I should take a Geiger telescope down the Kolar Gold Mines and measure the intensity of the penetrating component, and then using the μ-decay set-up check whether all underground penetrating particles are indeed muons. By the time Peters returned in December 1951 from the US to join TIFR on a more permanent basis, Naranan and myself had completed the intensity measurement up to a depth of 1000 ft below ground and were busy building the detector system for measuring the angular distribution of particles at various depths. Our very first paper entitled 'Cosmic rays underground', published in the *Proceedings of the Indian Academy of Sciences* in 1952, was based on extensive discussions with Peters. The second paper entitled 'On the angular distribution of penetrating cosmic-ray particles at a depth of 103 MWE below ground' (which had a bearing on the proportion at production and lifetime of the just then discovered K-mesons), which also appeared in the *Proceedings of the Indian Academy of Sciences*, was communicated by Peters himself.

Peters stayed on at TIFR till 1959 and during the eight years that he spent there played a major role in not only leading the activities of the nuclear emulsion group, but also in influencing the activities in other areas of cosmic-ray research. In 1955 he started, along with Lal and Rama, investigations on cosmic-ray-induced radioactive isotopes in the atmosphere.

Though in the beginning Peters was not very enthusiastic about my starting extensive air-shower work at TIFR since India did not have any special advantage over other groups, he did change his opinion and supported me later. In starting this work I had taken the stand that the opportunity for developing frontline electronics and detector systems was equally important and challenging and an air-shower investigation did provide this wonderful opportunity. The nanosecond timing system that we developed in this spirit, and also the total absorption spectrometer, became extremely important assets to much of the later investigations on the time structure of particles in extensive air showers. Interestingly, Peters, after moving to Copenhagen, started studies on time structure of muons in air showers in search of 'heavy-mass particles' which he called plutons, and at Ooty we started time-structure studies on hadrons using a total absorption spectrometer. These studies led to one of the most important results from Ooty in high-energy-interaction studies, namely the dramatic increase in the cross-section for nucleon–antinucleon production at tens of GeV energies much before the advent of the CERN accelerators. Peters spent almost a month with us at Ooty when we were doing this exciting experiment in the summer of 1965. Discussions with him were stimulating and always made us feel more confident.

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