loping a continuous nuclear emulsion block (with no backing) following certain rather convincing lines of reasoning. The experimental task was a hard one, to say the least — getting unbacked emulsion pellicles from Kodak, England, without much exposure to cosmic rays before flight, making a continuous block of a large number of sheets, and developing them with minimum distortion, and finally mounting them on frames such that any charged particle track could be traced either backward to its point of origin (or entry in the stack), or forward to the point where it either left the stack or was brought to rest by ionization (or interacted with a nucleus in the emulsion). Hurdles, as they may have appeared to active experimentalists in the programme (including the normally calm scientists, Yash Pal and G. Friedman), never really concerned Peters; he always had solutions and gave advice to all of us with confidence, even in areas he had not thought about earlier. For example, he felt quite at ease designing efficient refrigeration packages and new schemes to generate hydrogen with 1–2 orders of magnitude higher output than the designed output.

To make a long story short, the emulsion block experiment was carried out successfully[12], demonstrating that the elementary particle events could be located with high efficiency by following backwards tracks of pi-mesons. Within a year studies of the block, some 20 elementary particle events were discovered, leading to characterization of elementary particle decays[11,13–15]: Q-values, associated production, nuclear interaction strengths, etc. The results were presented by Peters at the 1953 Cosmic Ray Conference held at Bagneres de Bigorre and proved very significant in understanding the nature of elementary particles.

Peters had started seriously thinking of the cosmonucleus ^{10}Be in 1954. As far as I recollect, he was very impressed by a paper by Kaufman and Libby[16] on 'The natural distribution of tritium', published in *Physical Review* in 1954. During one of the train journeys from Bombay to Delhi in connection with balloon flights, he would often step down on the platform from his air-conditioned compartment and look for us (whenever the halt was of appreciable duration), to talk to us about exciting work described in the paper. His interest in the field of nuclear geophysics was manifest even before this. He was very impressed about the exciting work which was being done on the

Early years of high-energy physics

Sukumar Biswas

It was a cool, bright day in July 1952 when I met Bernard Peters for the first time in his well-organized office in the Old Yacht Club building of the Tata Institute of Fundamental Research. I had returned to India from my sojourn in Australia the previous day, and my first scientific interaction in Bombay on this day with two eminent scientists had a profound influence on my scientific career.

In the meeting with Peters I spoke with much enthusiasm on our new-found exciting results 'on the nuclear interactions of high-energy cosmic rays', which was the title of my Ph D thesis submitted to the University of Melbourne, Australia, in 1952. I was specially fond of one new result, which I explained to Peters — it was on a proton–proton collision at 1000 GeV, leading to multiple meson production. In this event we were able to measure directly, for the first time, the degree of inelasticity in p–p collision as 0.1 at 1000 GeV. This was measured in a very flat event in nuclear emulsion flown in a balloon in Australia and the primary proton energy was determined from Lorentz transformation in the centre-of-mass system. The secondary-particle energies were measured by careful multiple-scattering measurements in nuclear emulsion. These results, published in *Physical Review* in 1951, were one of the few that established the meson production as multiple and not plural. Peters showed great interest as, during the same time, the TIFR group observed a very-high-energy cosmic ray–Si interaction in emulsion, producing a giant meson shower of more than 300 particles. I discussed with him the new areas that I would like to explore in this field, and found enthusiastic support. My first meeting with him ensured my joining TIFR in 1952.

Towards the end of my doctoral work in Australia where I had a UNESCO fellowship from 1950, I wrote to my professor in Calcutta, M. N. Saha, about my interest in developing the new area of studies of high-energy cosmic rays with the newly developed technique of nuclear emulsions flown in balloons. I had earlier done doctoral work in nuclear physics in Calcutta University with Saha as my guide. Saha's reply was highly encouraging. He wrote that Bhabha has started this new area of research on a big scale under the direction of Peters and advised me to write to Bhabha. At that time, in 1950–52, Peters' name and fame were world-wide, and in Melbourne we were reading with great interest his epoch-making papers in *Physical Review* on the discovery of heavy nuclei in cosmic rays and their astrophysical implication. This work opened up a new dimension in cosmic-ray physics. From Melbourne I wrote a letter to Bhabha informing him of my new scientific findings in cosmic rays and of my keen interest to develop this new area with the new techniques of nuclear emulsions flown in balloons, and that the presence of Peters in Bombay gave special impetus to the idea. Bhabha replied in a telegram that I should see him on my arrival in Bombay. Thus, on the first morning in Bombay, I had the opportunity of discussing my research programme with Peters and Bhabha. My first meeting with Bhabha was a very enjoyable one as he enquired with keen interest about the details of the new results on high-energy interactions and how we obtained them. I explained to him how two of us, Hopper and myself, conducted balloon flights with meteorological balloons, tracked them with theodolites, processed the nuclear emulsions, scanned and analysed the data, and finally wrote the papers. Bhabha expressed much interest in our new results on the degree of inelasticity of high-energy interactions in nuclear emulsions as he himself was engaged in theoretical work on meson production.

After about an hour of discussions, I thankfully accepted Bhabha's offer for me to join TIFR in Peters' group. A few months later, after a brief vacation and completion of formalities, I joined TIFR on 1 November 1952. I became close friends with the young team members of the nuclear emulsion group. Two of my colleagues of that time, Devendra Lal and Yash Pal, became my very active scientific collaborators and life-long friends. Soon discoveries were made of several striking events of heavy unstable particles produced in high-energy cosmic-ray interactions in a stripped emulsion stack. To make systematic and detailed studies of their diverse

isotopic composition of lead at Cal-Tech during the early fifties, about which he learned from Harrison Brown during one of his visits to TIFR. In any case, Peters' ^{10}Be idea started to jell some time in the late 1954, and he decided that the first experiment to do should be the measurement of its concentration in Kashmir snow. So we began chemical experiments on how to extract ^{10}Be from ca. 200,000 gallons of melt water at pH 2–3 using cation exchange resins.

Analogous to the heroic task of making an ideal nuclear emulsion block, the task of ^{10}Be chemistry and measurement confronted us with several problems. The task was probably much harder since a lot of micro-level radiochemistry was involved. Let me give you a flavour of some of the experiments which were done. It was planned (more appropriately, proclaimed by Peters) that nearly all the ^{10}Be atoms present in 200,000 gallons of Kashmir snow melt water would be first removed on some 50 kg cation resin; then after several steps the same atoms would be concentrated on one resin particle of about 0.5 mm diameter, and then this resin bead would be placed on a freshly poured nuclear emulsion sheet (to ensure no earlier background tracks) for a month or so. Much of this was done, even fairly successfully. It was always a challenge to try new things, howsoever hard they may be. It was easier for some of us to try out such ideas. especially those who had faith in what Peters proclaimed. When he spoke, it looked like a task which was just ready to be done easily. Plans for the preparation of large-glazed ceramic ion-exchange columns and transporting the columns with accessories (large porcelain sinks, tubings, etc.) to Kashmir finally culminated in May, 1955. I was married on 17 May 1955 and within a week left for Gulmarg, Kashmir, with my eighteen-year-old bride, Aruna Lal. After acclimatization at a lower altitude, we camped at Khilanmarg (Kashmir). We had four tents, one for the cook and helpers, one for Peters, one for P. S. Goel and B. S. Amin, and one for Aruna Lal and myself. The experiment began soon enough, all as planned, but then within a week, Peters decided to leave alone for Bombay, to catch the first rains, and to measure the concentration of the cosmic-ray-produced ^{7}Be in the rain water. We continued the ^{10}Be-extraction experiment from Khilanmarg snows.

^{7}Be was detected[17] during the Bombay monsoon. The work on ^{10}Be

properties, Peters organized two parallel groups—one, with Lal, Pal and Peters, was working on production and decay properties; the other, with myself, E. C. George (later E. C. G. Sudarshan) and Peters, on their mass measurements We were engrossed in hectic work during these first studies in a stripped emulsion stack, and after six to seven months of exciting work in November 1952–June 1953, a series of four spectacular papers from the nuclear emulsion group were presented at the third international cosmic ray conference at Bagnéres de Bigorre, France, in July 1953 by Peters. Bhabha also attended the conference. Immediately the TIFR emulsion group achieved international fame and these papers were referred to all over the world. These new studies were later published in detail as companion papers in the *Proceedings of the Indian Academy of Sciences A*, in November 1953—

Nikita Krushchev observing a balloon-flown nuclear emulsion containing the record of a very-high-energy cosmic-ray interaction, TIFR, about 1955. President Bulganin is also seen, second from right. Also in the picture are, from left, Bernard Peters, Homi Bhabha, Sukumar Biswas, the Soviet interpreter, K. A Neelakantan, P. J Lavakare (behind Biswas) and P. S Goel (behind the interpreter)

one by Lal, Pal and Peters, 'Observations on τ- mesons and on K-mesons giving rise to capture stars' (*ibid.*, 1953, **38**, 398); and the other by Biswas, George and Peters, 'An improved method for determining the mass of particles from scattering vs range and application to the mass of K-mesons' (*ibid.*, 1953, **38**, 418). In the latter paper I had the good fortune of applying my earlier knowledge and experience of multiple-scattering measurements to discover a new method of constant sagitta scattering, and this had become a standard method followed in all the laboratories of the world in the following decades. Using this new method we were able to show in 1953, for the first time, that the masses of the τ-meson and K^+- and K^--mesons were the same, $974 \pm 42\ m_e$, in contrast to the different masses suggested by the UK and other groups. All these studies bear the stamp of the methodical planning of Peters and of his directions and guidance for their meticulous execution in all the details of the problem. These, together with the support of a highly dedicated team of young scientists, enabled the nuclear emulsion group of TIFR to be one of the top-ranking in the world. Soon afterwards, the work of the group diversified in many areas.

I had the good fortune to be intimately associated with Peters, and I acknowledge learning from him the scientific methodology of how a complex scientific problem can be solved by subdividing it into separate individual components and pursuing these to their logical conclusions. My close association with him continued till his departure from India in 1959; outside work my wife and I had a very friendly and cordial relationship with him and his family. On both scientific and personal planes, I have continued to have a most cordial relationship with Peters and his family in Denmark On invitation from him I visited, a few times, his laboratories at the Danish Space Research Institute in Copenhagen for scientific lectures and discussions and I greatly benefitted from them and enjoyed his excellent hospitality.

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