The detection of the isotopes with very long half-lives was difficult. We finally succeeded in measuring the ^{10}Be concentration in deep-sea sediments using one of the very earliest deep-sea ocean cores, which had been obtained by Petterssen and B. Kullenberg in Göteborg, Sweden. Another important long-lived CR isotope, ^{26}Al, was not isolated in ocean sediments until many years later. The tiny concentration of ^{32}Si in ocean water was not measured until Lal extracted this isotope by a novel technique from hundreds of tons of water *in situ* and identified it through its short-lived radioactive decay product ^{32}P.

This branch of CR research has been prospering since its beginning in 1955; CR-produced isotopes continue to play a role in oceanography and other branches of geophysics.

It was primarily for family reasons that I decided to leave India in 1958. I then accepted a position at the Niels Bohr Institute in Copenhagen but my connection with India did not ce visited India and the TIFR repe over the years. Scientists from the have been guests in Copenhage worked with me both at the Niels Institute and later at the Danish Research Institute. This connectic remained intact even after my ment.

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The discovery of cosmogenic ^{10}Be in India

D. Lal

The search for beryllium-10 was an exciting mix of brilliant ideas, ingenious and heroic metho thousands of gallons of Bombay rain water and Himalayan snow melt—and tenacity.

The story of the discovery of cosmogenic ^{10}Be produced in the earth's atmosphere is the story of independent evolution of scientific ideas in two groups in distant continents. This often happens in important scientific discoveries—and the discovery of ^{10}Be was indeed an important milestone in nuclear geophysics/geochemistry. More than a dozen groups all over the world are now measuring the concentrations of ^{10}Be in a wide variety of samples to learn about various parameters; past cosmic ray and geomagnetic field intensities, subduction of marine sediments along the plate margins and rates of erosion of natural surfaces, and many other leading questions in geosciences. I relate here the story of discovery of ^{10}Be by B. Peters in India.

If the title of 'king' had to be given to a terrestrial cosmogenic nuclide (a nuclide produced by nuclear interactions of cosmic-ray particles (with matter), it would no doubt go to ^{14}C (half-life = 5730 yr), the very first to be discovered[1-3]. By any standards, its detection was a brilliant accomplishment. The ratio $^{14}C/^{12}C$ in modern carbon is $\sim 10^{-12}$. The detection[4] of ^{3}H (half-life = 12.3 yr) with electrolytic enrichment was later accomplished by Libby and his colleagues for rain-water samples having $^{3}H/^{1}H$ ratios of $\geq 10^{-18}$. This was another significant milestone in the field of cosmogenic nuclides. The third long-lived terrestrial cosmonucleus to be detected was ^{10}Be (half-life = 1.5 m.y.). Peters[5] discussed the potential applications of this nuclide in 1955. The nuclide was detected unambiguously and independently in 1956 in marine sediments by J. R. Arnold[6] in Chicago*, and by B. Peters[7] and his colleagues in Bombay. Amongst the terrestrial atmospheric cosmogenic nuclides (henceforth called cosmonuclei; cosmonucleus for singular), ^{10}Be occupies a high rank as a radiotracer because of its long half-life, 1.5 m.y. It is the longest lived of the terrestrial atmospheric radioactive cosmonuclei and is useful for the study of processes and time-scales back to the Late Miocene. The detection of ^{10}Be in the late fifties was therefore another milestone in the field of cosmogenic nuclides, but its studies, although the only means for determining accumulation rates of marine sediments and manganese nodules back to 10 m.y. in the past, remained confined to a few scientific groups in the world. This was a direct consequence of the fact that the measurement of ^{10}Be activity involved very sophisticated radiochemical and low-level beta-counting methods. With the development of the AMS meth 1977, leading to substantial im ments in the detection sensitiv ^{10}Be (and other long-lived nu there was an almost immediate application explosion. This nucl being currently studied[8,9] in c samples of air, rain water, snow rocks, ocean water, marine sedi etc. to answer a wide range of que in palaeoclimatology, glaciology, chronology, subduction of the sphere, geomagnetism and cosm physics. Studies of ^{10}Be have bec sort of industry. This nuclide rank to the 'king', of cosmonuclei, ^{14}C.

The first detection of cosmo (terrestrial or extraterrestrial) was rally accomplished by chemist instance, of ^{14}C by Libby. For Ar chemist, it would probably be natural and a relatively simple t go after this nuclide! Peters was, ever, a physicist. He launched a fledged attack to discover this n after he had convinced himself th was an important nuclide in view chemical behaviour and long half-l the first paper[4] on ^{10}Be in 'Radioactive beryllium in the sphere and on the earth', he said:

It is estimated that about 1000 nu radioactive ^{10}Be (2.7 m.y. half-lif

*See article by Arnold, page 727 this issue.

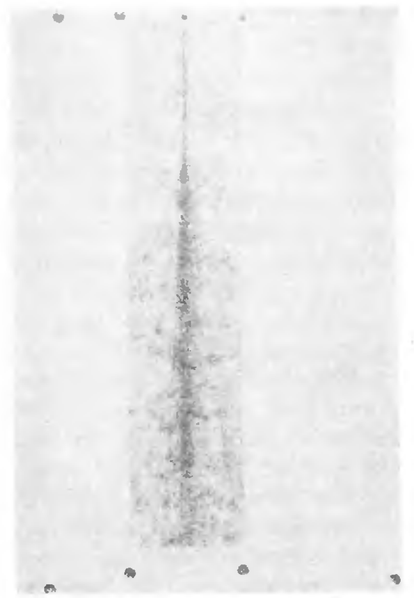
produced per square metre per second by cosmic-ray-induced nuclear disintegrations in the atmosphere. The conditions for observing the resulting activity in rain water and in various regions on the earth are favourable and may be useful for measuring sedimentation rates and other geological surface changes during the Tertiary.

Peters then set out with full vigour to detect this nuclide in snow, and in marine sediments. He fully recognized the importance of having at hand both a first-class chemical procedure, and a low-level beta-counting system. He consulted with the best radiochemists in the nuclear chemistry division of the Atomic Energy Commission at Bombay. These discussions soon convinced him that the needed techniques were not available. But Peters had his own way of looking at the problem. He decided that the task had to and could be accomplished; and to those who showed pessimism, he explained how to go about it. He came up with explicit suggestions in the field of radiochemistry, an area of science entirely new to him. His basic approach was correct, but many of his ideas have still not materialized. Nevertheless he soon succeeded in getting the chemists and physicists working together and coming up with a very specific chemical procedure which could extract $\sim 10^{12}$ atoms of ^{10}Be from a hundred grams of marine sediments.

I was then a graduate student at the Tata Institute of Fundamental Research (TIFR), Bombay, and was working with Peters, studying the primary cosmic radiation, and the nature of characteristics of elementary particles. In 1954, Peters talked to us about his idea of looking for ^{10}Be in nature. ^{14}C was then a well-established fact. The global mean production rate of ^{10}Be was estimated by Peters to be 0.1/(cm^2 column-sec), more than an order of magnitude smaller than that of ^{14}C. Several senior scientists were most sceptical of the ^{10}Be idea of Peters: (i) its small production rate, and (ii) finding it in the depths of ocean sediments where Peters claimed it would ultimately find itself after production in the atmosphere. I had then decided to work on the ^{10}Be project. Several scientists at TIFR had severe doubts on Peters' idea; some even thought that he had probably cracked up, and told me so. Did I believe in Peters' ideas? I had no doubts in my mind, and in fact I set out immediately to do the experimental work with other colleagues, P. S. Goel and N. Narsappaya. My conviction partly came about from the ^{10}Be production and deposition model clearly set out by Peters, but more from the fact that *I believed that Peters must be right because he says so — a sort of blind faith in him. To me he had repeatedly demonstrated that good science meant thinking deep, trying to look deeper into things, and then doing experiments to test the ideas.* It was then not important whether finally one succeeded or not. Some of us who were suitably exposed to the Peters method of working had become converts. It was a different matter however whether they would choose to work with him.

Devendra Lal, Yash Pal and Bernard Peters (Lal, Pal, Peters), Denver cosmic ray conference, 1973.

To fully appreciate my sentiments and the blind (!) rationale of joining the newly conceived ^{10}Be project, it would be useful to narrate some of my earlier research work with Peters and how I got conditioned to accepting his ideas. I joined the cosmic-ray research group at TIFR in late 1949. This group was nicknamed the nuclear emulsion group after the tool used to study cosmic rays. H. J. Taylor[10] of Wilson College, Bombay, was then looking after the research work of the group. A first-class physicist, an outstanding teacher, he worked hard and inspired us to study the nature of cosmic-ray nuclear disintegrations (stars, as they were called then) in nuclear emulsions, a subject close to his heart. About the same time, Peters had some questions about the anti-matter content of cosmic rays and decided that the problem can best be explored by exposing an E–W oriented package of nuclear emulsions to cosmic radiation at high altitude at equatorial latitudes (to eliminate the background due to low energy primary cosmic-ray particles). He



Photomicrograph of nuclear emulsion tracks showing interaction of a cosmic-ray Si nucleus of total energy 2×10^{14} eV with a nucleus in the emulsion producing more than 200 secondary particles (pions, kaons, etc.).

conducted[11] a series of balloon flights jointly with Taylor in Madras during October–November 1950 with the participation of several TIFR scientists, including R. R. Daniel, M. S. Swami, Y. Pal and myself. For me it was a great experience, not just the flights but to see first hand how basic research is done — from an idea to its implementation. And then a year later, as luck would have it (for me at least), Peters joined the TIFR towards the end of 1951 at the invitation of H. J. Bhabha, Director, TIFR. He then started a series of exciting research projects which electrified the intellectual portals of the institute. First there was an intensive study of a high-energy meson shower in a glass-backed nuclear emulsion plate; the event was an interaction of a Mg nucleus of kinetic energy, 7.8×10^{12} eV/nucleon, producing more than 350 charged and neutral mesons. Both the event and the analysis were first of their kind, leading to an insight into nuclear interaction at ultra-high energies.

At that time Peters was convinced that the field of elementary particle research then exclusively belonged to cosmic-ray research, and proposed a novel way of obtaining a large sample of rare elementary particle events in nuclear emulsions exposed to cosmic rays. He proposed exposing and developing 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

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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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Devendra Lal and Bernard Peters at the Jaipur cosmic ray conference 1962.

extraction from the cation exchange resin continued during 1955, but it had become apparent that we had to develop a low-level beta-counting system to allow ascertaining the energy of the beta-radiation detected. Work on this and development of the chemical methods for extraction of ^{10}Be from marine sediments began after the monsoon of 1955. We obtained the marine sediment samples courtesy of Prof. H Pettersen of the Swedish Oceanographic Institute at Goteborg; the core was raised by the Swedish Albatross expedition in the Eastern Pacific Ocean.

The chemical procedures were developed using both ^{9}Be and artificially produced ^{7}Be as tracers. ^{7}Be was produced by proton bombardment on lithium (^{7}Li) using the Van de Graaff generator at TIFR. Appreciable amounts of ^{7}Be could have easily been produced but Peters was very firm on our using the minimum amounts to ensure that the laboratory was not contaminated. He was very seriously concerned about contamination of the laboratory. Once I had, while walking to the bus from the Chemistry Division, Atomic Energy Commission, dropped the tube containing ~ 30 cpm ^{7}Be on the road. When he learned about it, he asked me to return to the spot as well as I could, and wipe off the shot on the road using a wet (acidified) paper towel, which I did.

We learned of Arnold's discovery of ^{7}Be some time around the fall of 1955. His paper[18] on ^{7}Be in *Science* (1 April, 1955) had measurements on rain samples collected during October 1953–April 1954. We thought we had discovered ^{7}Be for the first time.

And subsequently we learned that he was also looking for ^{10}Be in marine sediment samples. An exchange of letters began with Arnold who first learned about Peters' ideas on ^{10}Be from Prof. S. Chandrasekhar. Before this time, Arnold had not seen Peters' paper[5] in the *Proceedings of the Indian Academy of Sciences*.

^{10}Be was detected unambiguously in 1956 by the two groups[6,7] in sediments from the Eastern Pacific Ocean. Arnold analysed seven samples of 0–120 cm depth from two cores and Peters' group in four upper and lower sections of a 15-m-long core. The ^{10}Be activity was shown to follow Be chemistry by the two groups. The energy of the beta radiation was measured by both the groups by absorption measurements. Arnold showed that the measured ^{10}Be concentrations in his samples were in accord with its production estimates and sedimentation rate of ~ 1mm/10^6yr. The results of the Bombay group for samples separated by ~ 12 m allowed estimation of sedimentation rate, ^{10}Be deposition rate and limits on temporal changes in the cosmic ray intensity in the past 1.6–3.2 m.y. Peters was quite excited about the potential of ^{10}Be for determining both the chronology of marine sediments as well as changes in the terrestrial cosmic-ray flux, and had elaborated on the application of the ^{10}Be method in a second paper[19] on the subject. We of course know now that temporal and spatial fluctuations in some geochemical/geophysical parameters preclude a straightforward application of the ^{10}Be tracer method. However, the approach taken by Peters illustrates the method of his work. To most scientists in his situation, it would have been sufficient to first detect ^{10}Be, and then to think of the next step. The problem of detection of ^{10}Be was indeed hard—both on accounts of radiochemical and radiation measurement requirements. For the Bombay group, radiochemistry was quite a challenge; even Arnold, a professional, has remarked[20] that isolation of ^{10}Be was a difficult task.

The detection of natural ^{7}Be and ^{10}Be activities at Bombay produced great excitement in the scientific community. Independent search in the two continents for natural ^{10}Be added both spice and a comradeship. It is interesting to comment here that Arnold decided to look first for natural ^{7}Be and then ^{10}Be; a logical thing to do since the former was easier. Peters, on the other hand, looked for ^{10}Be first. He was convinced that it was the most important thing to do, but later on looked for ^{7}Be, to use it also for ascertaining the production rate of ^{10}Be.

It should be mentioned here that the half-life of ^{10}Be was earlier believed to be 2.5 m.y., a value given by Macmillan[22]. Discrepancies in the ^{10}Be cross-sections measured by counting ^{10}Be beta activity and by mass-spectrometer led Yiou and Raisbeck[22] to check on its half-life. They revised its half-life to 1.5 m.y. Later Macmillan[23] explained that by an oversight the value he published earlier was the mean-life of ^{10}Be and not the half-life. Introducing the factor of $\log_e 2$ would make his half-life estimate to be 1.7 m.y., in good agreement with the value of 1.5 ± 0.3 m.y. published by Yiou and Raisbeck[22].

Peters worked in India at a time when national fervour for science was high. Science was being nurtured by Nehru, Bhabha, Bhatnagar and others. Encouragement was all there, so that any idea howsoever costly could be undertaken! Several young and bright scientists were looking for opportunities to work on good ideas. Peters provided this opportunity to some of us. As you can see, I was one of those who grabbed *this* tightly. I graduated from a microscope to radiochemistry and low-level counting — with varied experiences on the side: balloon flights, leopard hunt, etc. Peters made a severe impact on the scientific community at TIFR (and in India as a whole). It was often very difficult to work with him (an understatement!): he had too many ideas and demanded immediate attention. However those of us who decided to work closely with him have reaped a life-long benefit. We try to emulate him, and work hard ourselves and with our colleagues. The story of detetion of ^{10}Be

Devendra Lal, Bernard Peters, Mrs Dayton, Bruce Dayton; Copenhagen, 1990 (See article by Dayton, page 729 this issue) in India is not an unusual story. Such things happen all the time in science. For me and many of my colleagues, for instance Pal, Goel and Rama, it demonstrated that we could do good science in India if we had confidence[24] in ourselves.

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The discovery of cosmogenic ^{7}Be and ^{10}Be

James R. Arnold

Halfway round the world from Bombay, in Chicago, an independent discovery of cosmogenic beryllium radionuclides.

Willard Libby[1] published his first note on cosmic-ray production of nuclides in the earth's atmosphere in 1946. He followed this in the next few years by developing the ^{14}C dating method[2–4] and by the measurement of ^{3}H in natural waters[5], achievements which attracted worldwide attention. Doubtless many persons were led to consider the possibility of finding and using other nuclides made in the same way. Libby himself did so, as he told me years later.

Since N_2 and O_2 are the primary targets in the atmosphere, the list of other interesting products of high yield is short. Among these radionuclides whose half-lives exceed a few hours it has only two entries: ^{7}Be (53 days) and ^{10}Be (1.5×10^6 years). There are many more products from spallation of ^{40}Ar but because its abundance is only about 1% they posed a severe challenge to the counting methods available in the early 1950s. Thus it is not surprising that many people were thinking about the Be isotopes, and that some reached the point of doing experiments.

My own interest and taste for such things had of course been aroused by my participation in the ^{14}C effort. In addition, I was excited by the possibilities of the then new scintillation-counting technique, and tempted to try my hand at developing a sensitive, low-background system using this method.

In 1952, together with Dr Thomas Sugihara, also a veteran of the Libby group, I had assembled a small NaI (Tl) counting system, in a low-background shield, and used it to determine the gamma spectrum and later the decay scheme of natural ^{176}Lu (ref. 6). This was an interesting radionuclide but there were no immediate applications. The search for ^{7}Be seemed more promising, since its half-life and mode of production in the atmosphere suggested usefulness in studying atmospheric circulation and exchange processes. I guessed that it would be found attached as BeO to atmospheric dust, and hence be washed out in rain, much like the bomb fallout that was then so abundant. I did not then know of a number of other efforts, earlier or contemporary, to isolate and detect it. I thought ^{10}Be even more promising for applications, but more difficult, both because of its long half-life and because it was known to be a pure beta emitter. My plan was to do ^{7}Be first.

Rain and snow are frequent in Chicago, so I did not have to go far for samples. I found an old storage building on the university campus with a single drainpipe, and bought a modest supply of tin-plated square five-gallon steel cans. I was also fortunate in hiring a strong and ingenious Iraqi technician, Hussein Ali Al-Salih. When it rained we would sally forth and collect 5–50 gallons of rain water (full of black industrial dust), acidify it, add carrier and lug the cans back to the laboratory. The samples in the cans were well mixed, I'm sure, in transit. Then followed a couple of days of rather messy and sometimes unreliable chemistry, which I devised based on the scattered literature and improved bit by bit. The end result was a small mass of (usually) white BeO powder. The content of ^{7}Be in these extracted samples was variable, of course, depending on the details of the history of the air mass and the clouds from which the rain fell. Fortunately in some cases it was quite high, so that extreme low-level methods were not needed, and the identification could be made both from the energy of the