Front Line
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Interview: B.V. Sreekantan
Peerless trail-blazer
By R. Ramachandran

In conversation with B.V. Sreekantan, cosmic ray physicist and astronomer. By R. RAMACHANDRAN

NANJANGUD, a small town near Mysore in Karnataka, is more famous for its pink-coloured tooth powder than for producing the great cosmic ray physicist and astronomer Badanaval Venkatasubba Sreekantan, who was a student of Homi J. Bhabha. Sreekantan was born into a scholarly family of Nanjangud and his father, B.V. Pandit, who was an Ayurvedic doctor, was the originator of the famous herbal tooth powder.

Sreekantan completed high school in Nanjangud and obtained a two-year intermediate degree from Mysore. He then moved to Central College, Bangalore, where he obtained his BSc Physics (Hons.) degree in 1946 and MSc (Physics) in 1947, with a specialisation in wireless. He then joined the Communication Engineering Department of the Indian Institute of Science (IISc), Bangalore, as a research scholar. In 1948, Bhabha, who had carried out theoretical studies on cosmic rays in Cambridge and had decided on cosmic rays as one of the areas of research at the Tata Institute of Fundamental Research (TIFR) in Mumbai, selected Sreekantan as one of his first students to carry out research in experimental cosmic ray physics. As the cosmic ray scientist P.C. Agarwal points out in a recent article in *Current Science*, Sreekantan is that rare scientist whose work ranges from experiments a few kilometres deep underground in a mine—the Kolar Gold Fields (KGF)—on cosmic ray particles and proton decay to altitudes up to several hundred kilometres with balloon and rocket-borne detectors to study X-ray emissions from neutron stars and black holes.

He was part of the team that first detected atmospheric neutrinos at the KGF. The planned launch by the Indian Space Research Organisation (ISRO) of Astrosat, a multi-wavelength X-ray astronomy spacecraft, in September, and the proposed India-based Neutrino Observatory (INO) in Theni district of Tamil Nadu are milestones in the programmes that Sreekantan initiated at the TIFR.

During his 39 years at the TIFR, he put India on the world map of high-energy physics and built up a vibrant school of experimental high-energy cosmic rays, which carries forward his legacy in experimental cosmic rays and astrophysics using a variety of detectors and techniques.

After serving as the TIFR's Director from 1975, he superannuated in 1987 but continued until 1992 as the Indian National Science Academy Srinivasa Ramanujan Professor. In 1992, he moved to the National Institute of Advanced Studies, Bangalore, as the Dr S. Radhakrishnan Visiting Professor, a position he continues to hold. He is also Chairman of the Gandhi Centre of Science and Human Values, Bharatiya Vidya Bhavan, Bangalore.

Interestingly, he is also chairman of the board of Sadvaidyasala, the company founded by his father for making Ayurvedic products, including the pink tooth powder.

On June 30, Sreekantan turned 90. His fitness, alertness, amazing memory and continuing awareness of current developments in physics and his current research interest in philosophy and metaphysics hardly betray his age. On July 10, the TIFR felicitated him to mark his 90th birthday. *Frontline* caught up with him on this occasion and had a wide-ranging interview with him (in two parts). Excerpts:

You hail from a small town. How did you get interested in physics?

Essentially because my eldest brother was always interested in physics. He was a medical man and he was interested in both physics and philosophy. That influenced me. In Central College, Bangalore, I did Physics (Honours). That was the motivation. We come from a very orthodox and scholarly family. Therefore, there was this influence of the Bhagvad Gita and other scriptures because every Saturday my father used to arrange a lecture on these by some scholar or the other from Mysore. Since my father was fairly rich, he would invite these scholars. We had a very big hall where 30-40 people used to gather. Even as a young boy, I was exposed to the Bhagvad Gita and Vedanta philosophy. These Gita sessions continued for several years. So, whenever I had a chance, I would sit and listen to those lectures, partly understanding and partly not. When I came to Central College, there was one professor of physics by name Subbaraman who had worked with C.V. Raman. And he was a follower of [the] Sringeri [mutt]. He influenced me quite a bit in my interest in theoretical physics and its relationship to philosophy and also in my decision to do research in nuclear physics. When I left Central College, I had a good grounding in theoretical physics and I wanted to do theoretical physics with [Homi] Bhabha.

When I was a student in Bangalore during 1943-47, Bhabha was at the Indian Institute of Science, and he was there till 1945. Occasionally, he would come to Central College and give lectures on nuclear physics and related topics. At that time, nuclear physics was just beginning and it interested me and then, of course, elementary particles. I came here to the TIFR after spending a year in the Communication Engineering Department at the IISc. I had done MSc in wireless because the theoretical physics option was not there; [you could do] wireless or X-rays. I preferred wireless. I did not know much about the TIFR at that time, so I joined communication engineering. I was there with Professor Chatterjee, who was an electronics expert. During that year I learnt quite a bit of electronics. At that time, in colleges, electronics was mostly radio physics. When I went to him he started me on ultra high frequency (UHF) waves. I started doing this but my eyes were still on physics. And Chatterjee encouraged me. He said, "You are more suited to do research in physics. If you continue here you will end up as an engineer in All India Radio or somewhere." He encouraged me to apply to the TIFR and also the PRL [Physical Research Laboratory, Ahmedabad].

When you were at the IISc, and Bhabha was also there, did you have any chance of interacting with him?

Not very much. But I listened to Bhabha's lectures. I must mention one thing that happened which brought me to cosmic rays. There was a Mexican physicist by name [Manuel] Vallarta who came and gave two lectures on cosmic rays in January 1948. He gave very beautiful lectures. On the second day, as we were coming out of the hall, we heard about the unfortunate death of Mahatma Gandhi. Vallarta's lectures were presided over by Bhabha. Somehow, after those lectures I got interested in cosmic rays. So, when I had gone for the interview [at the TIFR] he asked me what I would like to do. I was interviewed thrice that day; the first [was] on experimental physics. Bhabha chaired the interviews. When I entered the second time for the theoretical interview, Bhabha told the others: "I have examined him in physics. He is quite good. Now you examine him in mathematics." When I was called in for the third time, Bhabha asked me: "What do you want to do?" Though my mind was on theoretical physics, I said: "You have interviewed me. I will go by your decision."

But you had made up your mind to come to the TIFR even though the IISc had an established school of physics and Raman was already there. What attracted you to the TIFR?

Yes. The TIFR was then a small set-up. There were only six people in it. IISc physics was very poor at that time. There was this quarrel between Raman and others, and Raman was trying to build his Raman Research Institute. He was practically sent out of the IISc. Therefore, nobody encouraged me to go to Raman. The obvious thing at that stage would have been to join Raman. I hesitated and my brothers also told me not to go to Raman. Then the choice was between the TIFR and the PRL. And I was interviewed by Vikram Sarabhai too.

One week before [the TIFR interview] I came to Madras [Chennai] and the interview was held at Ammu Swaminadhan's house and he also selected me. Ammu Swaminadhan, his mother-in-law, was a member of the Legislative Assembly in the Madras government. But next week I was selected by Bhabha and I decided to join the TIFR.

You did not have any trepidation about joining a new field like cosmic rays that was yet to develop fully.

When you are entering research, at first, there would be anxiety about any field.

But you had to choose experimental physics, building instruments and electronics for the particle detectors, despite your primary interest being theoretical physics…

That's what Bhabha wanted me to do. He said I had experience in electronics and so would be ideal for starting work in the field of cosmic rays. And that proved true. That is when electronics too was becoming more and more complicated and it was in a developmental stage. Pulse electronics was not known in India. There used to be a magazine called *Nucleonics* that would carry some circuits and we would copy them. We all—[Raja] Ramanna, who joined a year later, and others—worked together. We used to get a lot of electronic parts [discarded from Second World War equipment] from Chor Bazaar.

And Professor [D.Y.] Phadke was there. He was a little more knowledgeable in electronics. I think it was in Germany that he had done a semester in electronics. A.S. Rao came, but he was not directly doing any electronics. He became more of a manager of the experiments.

You have mentioned in one of your reminiscences about a "missed opportunity" to meet Bhabha in 1947 itself. What was that incident?

Immediately after I joined the IISc, Bhabha came to Bangalore and he was staying at the West End Hotel. Professor Chatterjee told me to contact him if I was interested and seek an interview. I rang him up. He asked me to meet him at 9 a.m. I went there around 8:30 a.m. But he came down only at 11:30 a.m. and then said, "Oh! I am sorry. I have to have my breakfast." Then he went into the breakfast hall. There was one other person waiting. I don't remember who it was but probably for the same purpose.

But Bhabha did not come back at all. Probably, he completely forgot. So I was very upset and I just went away. I went and told Chatterjee that he basically doesn't seem to be interested. That became quite an aborted interview.

Then you formally applied next year.

Yes. I formally applied the next year. Actually, he remembered and sent me an application form. I did not revert to him. After confirming the appointment but failing to meet that morning, I had kind of rejected him. He asked me to send in the application form and come.

And immediately after joining you had to give a colloquium…

After a week or two he called me and said that I had to work on the 'mu-meson' [now called muon]. At that time the only other particle that had been discovered then was the mu-meson. The pi-meson [now called pion] had been discovered, but we did not know about it. In fact, Bhabha went to England and got the news of its discovery and when he came back he told us and also showed us some photographs of its production in cosmic rays. But the mu-meson was of particular interest to him as he was working on scattering properties of elementary particles and the mu-meson was supposed to be a spin-½ particle and was a lepton. The anomaly was its mass was much higher than that of the electron; about 200 times. And also there was this problem. There were both positive and negative mu-mesons and when negative mu-mesons entered an absorber, absorption and decay would compete with each other and, therefore, its lifetime was difficult to determine. So that's the experiment he wanted me to do; to separate out the positive and negative muons and see if the lifetimes of positive and negative mu-mesons were different. I started on that experiment but very soon a person from the United States—I forget the name—published the results from such an experiment.

In December 1950, Bhabha organised the First International Conference on Elementary Particles. That was the first elementary particle conference anywhere in the world. Many very important people—[P.M.S.] Blackett, [Gregor] Wentzel, [Rudolf] Peierls, [Leon] Rosenfeld, [Peter] Fowler, [Pierre] Auger, [Eduaordo] Amaldi, etc.,—came. One night, as I was getting the papers for the conference ready and sitting and typing something, he called me. "Look, there appears to be some discovery of a new particle. Why don't you take your mu-meson detector underground and check whether there are any new types of particles in cosmic rays which cannot be explained in terms of mu-meson type of penetrating particles?" That year [January 1951], he was the President of the Indian Science Congress, which was held in Bangalore at the IISc campus. I had gone to the Science Congress with some others. Ramanna had also come. After the conference I went to KGF and saw various places there; Ramanna also came with me. By October we got all our equipment ready and went there to first measure the cosmic ray intensity. [S.] Naranan also came with me. Before starting any experiment we had to know the intensity in the mines.

Had the TIFR already started producing Geiger-Mueller (GM) counters by then?

No. We started in parallel. When I started work on the mu-meson, the problem was you had to measure the lifetime of a particle within microseconds. One of the problems in the Geiger counters was "after pulsing"—the main pulse followed by the secondary pulse—and that was in the microsecond region. So, I had to work hard on finding out a suitable quenching gas that would not give rise to this problem.

That developmental work we did for about six months. R.V.S. Sitharam, who later on became the Director of SAMEER, Mumbai, also helped us that time. It was he who developed all the radiation detectors for the survey of uranium and other [atomic] minerals. With his help I was able to make alcohol the quenching gas and then it worked.

H.L.N. Murthy was another person who was a glass technician. Bhabha had known about him. When he created the TIFR he took him along and gave him a job. Poor chap had a heart problem and Bhabha came to know about it, sent him to England on the pretext of getting trained at the Harwell (atomic) establishment, and then sent him to MIT [Massachusetts Institute of Technology] from there. There he learnt quite a lot of technologies. And he got him operated. That gave him another 15-20 years of life. He had a very bad heart condition. That's the kind of person Bhabha was.

After this experiment of variation in the intensity of muons with depth [down to about 300 metres] that you carried out, you had to, in a manner of speaking, give up that research.

What happened was I measured the intensity [of muons] as a function of depth, the angular distribution, etc., and on that basis I wrote up my thesis. Then Bhabha asked me to spend some time in various other

laboratories… three months here, three months there, like that. During that period first I went to Jungfraujoch [in the Alps in Switzerland at an altitude of 3,460 m] where they [Cecil Powell's group from University of Bristol] had started experiments on K-mesons, etc.

Then I spent some time with Amaldi's group, which was originally concentrating on detectors [for cosmic rays]. Then I spent nearly one year at MIT. First I was supposed to be there only for three months. But then [Bruno] Rossi said three months is not enough, and Bhabha agreed. I was there during 1954-56 and in 1956 I came back. During that period, MIT had started Extensive Air Shower [EAS] studies and also scintillation counters. So when I came back here I started work on scintillation counters. I also got Prof Phadke interested in making these counters. And also A.B. Sahiar, one of my seniors, had moved the cloud chamber that he had built [in Mumbai] to Ooty [Udhagamandalam].

When I came back, I changed the detectors by replacing GM counters with scintillation counters. In 1960, we started various air shower experiments, underwater experiments, emulsion photographs, etc. Then one Japanese professor by name [Saburo] Miyake visited the TIFR. He was an expert in building cloud chambers.

With his help we built the world's largest multiplate cloud chamber. When I was at MIT, I also learnt milli-microsecond pulsing techniques. Using that technique we started experiments and were able to measure the relative arrival times of air shower particles.

We established that there were delayed particles in air showers compared with electrons and photons, and these delayed particles are nucleons [protons and neutrons]. That is what actually formed [S.C.] Tonwar's thesis later.

The surprising thing was that the nucleon-antinucleon production cross-section in high-energy interactions was supposed to be small. But our experiments showed that it was much more—about 12-15 times higher—before the CERN experiments did. We published that and the CERN people confirmed it much later. That led to a different line of work. Then we started [work on] the differences in the characteristics of interactions between pions and protons.

When you completed your thesis work in 1954 based on your underground muon studies, you may have already got the idea that one could do neutrino experiments underground.

No. What happened was when we finished the first phase of our underground experiments, the KGF people said that they are going to close down the mines. That happened very early. Also, Naranan's work was on interaction of mu-mesons. So we did that in a tunnel in Khandala. It was his thesis programme.

When I came back, three years later and in 1958 started Ooty air shower experiments and all that, we came to know that the mines had not closed down and they were going to expand it. Then we started the second series of experiments. By this time the Durham group and the Japanese group [from Osaka University] were also interested. That is how we went in for neutrino interaction experiments.

But before that what happened was we first started doing measurements further deep. We had done experiments—lifetime measurements—only up to 1,000 feet below. We could go up to 8,000 ft.

We started deeper and deeper level experiments. At the deepest level [2.76 km], in three months of operation, not a single muon was found. That told us that this was the place where we could look for neutrino interactions.

Soon after that you had to go back to MIT for the second time...

That was for X-ray astronomy studies.

That was the time when neutrino astronomy began in our country and though you were involved in its initial phase, particularly in the first-ever discovery of the cosmic ray neutrino, you did not continue neutrino studies underground…

In fact, [M.G.K.] Menon must have reported our work [on the discovery of the first atmospheric neutrino] at the London Conference in 1965.

I had an eye operation at that time and with the bandage on I wrote the paper. I was on my way to MIT.

There was an incident that you have also mentioned in your reminiscences somewhere that [Frederick] Reines [who won the 1995 Nobel Prize for discovering the neutrino in 1956] showed interest in coming to KGF…

When we wrote this paper on the possibility of underground experiments for neutrino detection, he bypassed us completely. He went to the IISc, met Professor Satish Dhawan and then went to KGF and worked out an in-principle collaboration with the IISc—there was one Venkatasubrahmanya, who was perhaps a research fellow at that time and had done some work in radioactivity and did not know much about cosmic rays.

Reines was very unfair to us; he knew us but wanted to take all the credit. He went and told the KGF people that he was going to bring a lot of money and set up a huge experiment and what we were doing was on a small scale, and so on.

And they signed the agreement. At that time there was a rule which Bhabha had got passed in Parliament that anything connected with nuclear science should get the clearance of the Department of Atomic Energy. When the KGF people saw that it had something to do with nuclear interactions, they referred it to Bhabha.

We did not know that Reines was doing this in parallel. Then Bhabha called us and told us: “What is this? You people have closed your eyes. Reines is going there and doing all this.” Then that was stopped. Then Reines explored the possibility of collaborating with us. But by then we had already established collaboration with the Japanese group and the Durham group. So then he went to [gold mines in] South Africa. We had visible detectors, neon flash tubes, and he had only scintillators. Our detection results were published a couple of weeks earlier and then he also found the same thing. This was in May and both of us presented the results at the London Conference, which was in August.

Then when you went back to MIT, you got involved in X-ray astronomy.

Yes. I got involved in X-ray astronomy and when I came back I started X-ray astronomy here.