

ved in the laboratory as isolated particles, the quarks always resided inside hadrons and no one was able to isolate a single quark. This was one of the most puzzling aspects at that time.

Towards the discovery

The situation in high energy physics was even more confusing in 1970 when Ting wrote his proposal. Since 1965 Ting had been working on the tests of quantum electrodynamics at high momentum transfer. It may be noted that at small momentum transfer, there is agreement between quantum electrodynamics and experiment about anomalous magnetic moment of electron to seven decimal places. I know of no other theory in physics (may be even in science?) in which there is such an unprecedented agreement between theory and experiment. From 1965 to 1969 Ting and his group observed the production of heavy photons (vector mesons) ρ , ω and ϕ (whose masses were around 1 GeV) and their subsequent decay to electron-positron pair (e^+e^-). One obvious question was: how many heavy photons exist and what are their masses and other properties? Ting wanted to study this question but his proposal was rejected by both Fermilab and CERN. Finally he submitted a proposal to Brookhaven on 11 January 1972. He wanted to look for heavy photons (vector mesons) through fixed target production experiments in which high energy protons slam into a target

$$p + p \rightarrow V^0 + X, X \dots \text{anything,}$$

and they will try to look for the heavy vector meson V^0 through its decay to e^+e^- pair. The interesting part of his proposal was his assertion 'contrary to popular belief, the e^+e^- storage ring is not the best place to look for vector mesons. In the e^+e^- storage ring the energy is well defined. A systematic search for heavier mesons requires a continuous variation and monitoring of the energy of the two colliding beams, a difficult task requiring almost infinite time. Storage ring is best suited to perform detailed studies of vector meson parameters once they have been found'. The subsequent events have confirmed this assessment and that is why a hadron machine is popularly termed as a 'discovery machine' while e^+e^- machine is meant for precision studies.

Richter, on the other hand, was involved with e^+e^- storage ring. He was interested in understanding about the production of hadrons in e^+e^- collisions. In 1965, SLAC submitted a proposal to the US atomic energy for such a machine with an energy of 3 GeV for each beam. Funds were made available for this collider (SPEAR) only in 1970 and the machine was built by April 1972. The SPEAR group was primarily examining the ratio R , which roughly speaking, is the number of hadrons divided by the number of muons produced in e^+e^- collisions. More precisely

$$R = \frac{\sigma(e^+e^- \rightarrow \text{hadrons})}{\sigma(e^+e^- \rightarrow \mu^+\mu^-)},$$

where σ denotes the cross-section. They wanted to study the variation in the value of this ratio as the total energy is changed from 2.4 GeV in steps of 200 MeV. From their preliminary study they found that this ratio R was rising from 2 to 6 as the total energy increased from 2 to 5 GeV. Richter presented these results at the biannual Rochester conference held in London in the summer of 1974. John Ellis also spoke at the same conference reviewing the production of hadrons in e^+e^- collisions from different models. He showed that depending on the model, this ratio could be anything from 0.36 to ∞ (i.e. 0.36, 2/3, 2, 10/3, 4, \dots , ∞). The most widely accepted three-quark model (with colour) predicted R to be 2. Thus the situation appeared totally confusing as late as the summer of 1974.

The discovery

Ting's proposal to Brookhaven was approved in May 1972 and was awarded a thousand hours of beam time. It took the group almost 18 months to build the detector which was enormous in every way: in size, intricacy, sensitivity, and cost. The actual experiment started in April 1974. They first looked at $\phi(1020)$ meson, the idea being, if one plots the number of e^+e^- pairs produced in this experiment as a function of total energy then one should see a broad peak whose maximum height is at 1020 MeV. This detector could measure the energy of the e^+e^- pair with great accuracy. This made the detector very costly and Ting was criticized for making the detector needlessly accurate since no one at that time

thought that there could be a heavy vector meson with very narrow width. On 22 August, the team turned the detector to energies between 2000 and 4000 MeV and took data for two weeks. Within a couple of days, two teams independently started to analyse the data (normally only one team analyses the results but Ting being a very careful man always had two) and both independently realized that when they analysed the number of events from 2875 to 3225 MeV in gradation of 25 MeV, nearly all events were piled up at 3100 MeV, i.e. *instead of a hill, they actually had a needle!* And that was the big surprise as till then no subatomic particle was known which had such a narrow width (i.e. such a large lifetime). It seemed to be 1000 times narrower than expected! This is where the personality of Ting came into the picture.

Several members of his group urged him to publish the results immediately but he decided to doubly check the results. He knew that SPEAR could discover the peak in a day if only they knew where the peak was! On the other hand, Ting could observe it in fixed target experiment only because of his obsessive insistence on fine-tuning the detector. During this period the MIT group members were making discrete enquiries about the energy at which SPEAR was running and when they heard that it was running between 4.5 GeV and 6 GeV, the group breathed freely!

Ting's frustrations increased when the machine restarted on 2 October but developed problems immediately. He then thought about announcing the results during the 17-18 October MIT festival to honour Victor Weisskopf on his retirement. However, he backed out at the last moment. On 22 October, Ting got back the machine to further recheck the data and one of his group members, Ulrich Becker gave a previously scheduled seminar at MIT where he disguised his very narrow peak by presenting the number of events over a sufficiently wide energy range. However, it did not fool Martin Deutsch who took Becker aside after the seminar and asked him to publish the results immediately. On the same day, Mel Schwartz of Stanford stopped at Brookhaven to assess the progress of his experiment. His assistant Jayashree Toraskar then told him about the bump at 3.1 GeV in Ting's experiment. Schwartz met Ting to get a confirmation about it. The conversation

that followed had far reaching consequences. It is therefore worth reproducing the conversation (as per Schwartz's recollection).

Schwartz: Sam, I hear you got a bump at 3.1.

Ting: No, absolutely not. Not only do I not have a bump, it's absolutely flat.

Schwartz: I will make you a bet. Ten dollars you get a bump.

Ting: Absolutely. I will bet.

Clearly, at least after this conversation, Ting should have announced the discovery as he knew very well that Schwartz was going back to Stanford (where SLAC is situated) and once the SPEAR group hears about it, they will just get it in a day. By denying the rumour so flatly he in fact weakened his case and eventually he had to share the Nobel prize with Richter. If only he had been honest with Schwartz he would have got the full credit.

On 25 October, Deutsch again pressed the MIT group to publish the results soon; otherwise SPEAR would get to it, but still nothing happened. Apparently, Ting now felt that there could be more than one bump and he wanted to get credit for it too.

On 9 November, the SLAC group decided to stop their run between 4.5 and 6 GeV and instead go back to 3.1 GeV. Apparently, one member, Roy Schwitters felt that the SPEAR group needed to write a paper on their experiment and hence he started to look at the data carefully. While doing so, he noticed that there was something inconsistent in the data around 3.1 GeV. He talked with other group members including Gerson Goldhaber and Richter and they agreed that indeed there was something odd and that one should go back to 3.1 GeV. It may be noted that it is not easy to change the energy. One has also to retune the beam and reset all the magnets. The official reason for going back is hardly convincing to say the least and it appears that the conversation of Schwartz with Ting two weeks ago as well as other rumours floating around played some part in this decision. As expected, within a day (i.e. on Sunday, 10 November) they had confirmed the existence of an unusually narrow hadron at 3095 MeV.

As luck would have been, on the same day, Ting arrived at SLAC to attend a

previously scheduled meeting of the SLAC Programme Advisory Committee. At the hotel, Ting got a frantic message from Deutsch who had heard that SPEAR had found something at 3.1 GeV. Ting was obviously horrified and immediately telephoned Brookhaven and asked them to announce the discovery right away and informed them that he will announce the discovery at SLAC the next day. Ting also called up the e^+e^- machine at Frascati about the discovery and within two days they too confirmed the existence of the particle. The three papers were published back to back in the December 2 (1974) issue of *Physical Review Letters*.

Why did Ting not publish his data earlier when so many of his group members were urging him to do so? Was he ultra cautious or was he not very confident of the results? Or was he greedy? Was he so naive to believe that his conversation with Schwartz will not reach SLAC just because he had denied the rumour?

While it is true that perhaps Ting should have got full credit for the discovery of J/ψ , the SPEAR group took over from that moment onwards, doing all the precision studies related to the production and decay properties of J/ψ . Further, within ten days, the group discovered another vector meson $\psi'(3695)$ MeV.

Soon after the announcement of the J/ψ discovery, there followed a host of theoretical speculations about what it was. The big question was, why is J/ψ so narrow and hence long-lived? Some of the suggestions were: it is an intermediate vector boson, Higgs boson, lightest coloured particle, lightest particle with a new quantum number called parcharge, charm-anticharm ($c\bar{c}$) quark bound state, (i.e. charmonium bound state). There followed vigorous theoretical activity trying to figure out the correct answer. The SPEAR group also discovered three more states through radiative transitions whose masses were between those of ψ' and J/ψ . Within about one year after the discovery it was clear that J/ψ was $c\bar{c}$ bound state so that the basic constituents of nature were four quarks and four leptons. The other states discovered at SPEAR were also easily understood as the various states of the charmonium ($c\bar{c}$). Remarkably, it was shown that the $c\bar{c}$ spectrum can be well understood within the framework of non-relativistic quantum mechanics plus

spin-dependent corrections. One last obstacle in this picture was the existence of 'Charmed mesons'. However, even these were discovered by the middle of 1976 and it convinced even the most die-hard skeptics about the validity of the charm hypothesis.

Around this time, two more rather unexpected discoveries followed, namely those of τ lepton at 1786 MeV and $b\bar{b}$ bound states, where b is the fifth quark. I would say that these two were the last two surprises and in the last twenty years we have not had any more surprises in high energy physics.

It was clear by then that there are twelve basic constituents of nature, i.e. six leptons (τ -neutrino being the sixth lepton) and six quarks. Though only five quarks were known till then, the community was confident that there must exist the sixth quark and such a quark (t -quark) was indeed discovered at Fermilab in 1994.

The present picture

As of today, the basic constituents of nature are six quarks and six leptons. The strong interaction between quarks is due to their colour degree of freedom and the corresponding gauge quanta are called gluons and this theory is known as quantum chromodynamics. On the other hand, the electromagnetic and weak interaction between quarks and leptons is given by a unified electroweak theory $SU(2)_L \otimes U(1)_Y$. By now all its predictions have been experimentally verified except for the prediction of a neutral Higgs boson. A Large Hadron Collider (LHC) machine is being built at CERN specifically to look for this particle and it is expected that this issue will be resolved by the year 2007.

It must be made clear that there are several basic questions that have not been answered by the above (so called) 'Standard Model'. For example, the standard model has several arbitrary parameters. Besides, the origin of fermion masses is unclear. Further, there is only a partial unification of the basic forces. In recent years, a truly unified theory called the *superstring theory* has been proposed which unifies all the four interactions. One remarkable break from the past is that here the basic constituents of nature are not particles at all! Rather the basic object is a string of length 10^{-33} cm. The quarks, leptons and the gauge bosons are

merely the different modes of vibration of the string. The unification ideas have brought closer the seemingly contrasting worlds of the smallest and the largest. In particular, these ideas hold the promise to explain how the universe evolved over a very short time after the big bang. Another possibility is that the quarks and

leptons are themselves composed of more elementary objects. Unfortunately, so far we have no experimental evidence for any of the ideas beyond the standard model.

1. Crease, R. P. and Mann, C. C., *The Second Creation*, Macmillan Publishing Company, USA, 1986.

2. Ting, S. and Richter, B., *Nobel Lectures*, 1976.

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PERSONAL NEWS

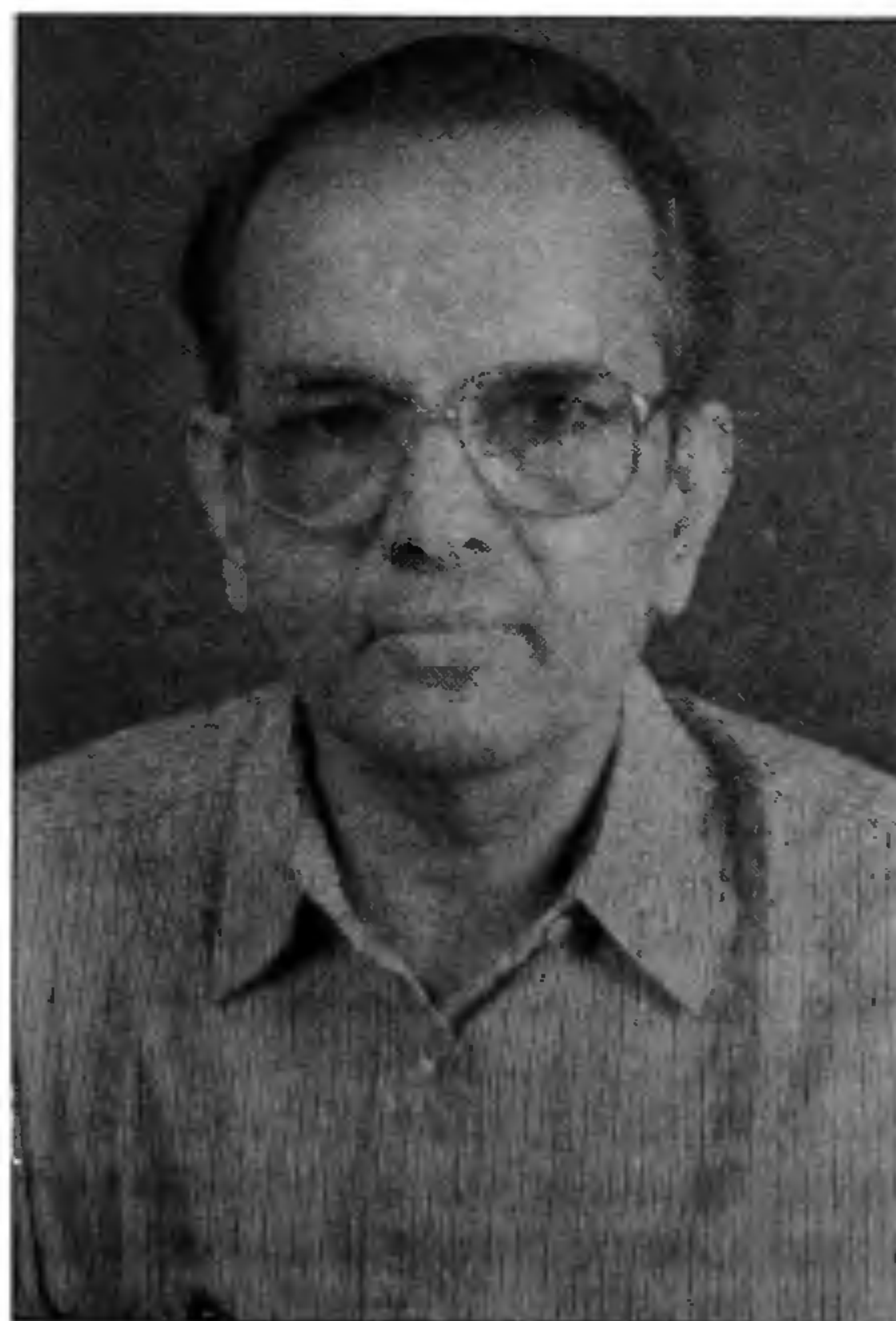
Chief designer and rocket technologist

An obituary of S. Srinivasan

S. Srinivasan was a close friend of mine since 1972 and our friendship continued for nearly a quarter century. For about a decade, we worked together at the SLV Complex, Thumba.

Srinivasan, a *Karmayogi* made Vikram Sarabhai Space Centre (VSSC) self-reliant in launch vehicle design. He was responsible, with his team, in designing isogrid structure, sandwich construction, segmented rocket motor casings, composite design and production capability. In the launch vehicle technology, he introduced 'design to cost' and 'design to reliability' concepts and spread them in VSSC. He established design, technology and test centres in the Valiamala complex. I remember an event, the next day (19 July 1980) after the successful launch of SLV-3 that put ROHINI satellite in the orbit. When the country, the organization and the teams were busy celebrating the success of the completion of the SLV-3 project, Srinivasan organized a special Project Review Meeting at the SLV-3 complex at Thumba. The SLV-3 team greeted each other and saw the long and big black board in the Conference Hall fully filled with Srinivasan's beautiful and yet powerful message of what will be the future tasks for the preparation of the next SLV-3 flight, identified with actions needed and responsibility. It was indeed a great experience learning and working with Srinivasan. He was a source of inspiration for the team. He ignited young minds – to aim at greater goals. Unless

the thoughts get into actions, Srinivasan would not accept anything else. Wherever I was, a phone call from Srinivasan made me reschedule all my other programmes and reach Thiruvananthapuram. That was because of the special bond of friendship.



*Dr Srinivasan was intellectually great,
yet he can communicate to all.
He worked himself on tough problems,
And trained his team confident to meet
tougher.
He always lit the candle when there was
darkness all around,*

*At success, Dr Srinivasan was calm
and full,
At failures, he stood like a mountain and
his team climbed over.
He will preside over, empower many to
contribute and dream,
Criticism for Srinivasan, was indeed a
turning point for good,
Many with pain would go to him, would
remove with a smile.
Oh Almighty God, we pray and pray,
Here is your beautiful creation,
Bestow on him, all your grace.*

Great events take place in a nation where great men dream and sweat. Sweat transforms into design, product and finally the system. Systems are like rockets to multiple launch vehicle systems. The VSSC campus created a great scientific and technological mind and a great soul. Srinivasan was the Chief Designer of various types of rocket systems. His ideas and thoughts transformed into launch vehicles like SLV-3, ASLV, PSLV and the forthcoming GSLV. The nation gratefully remembers his contributions.

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