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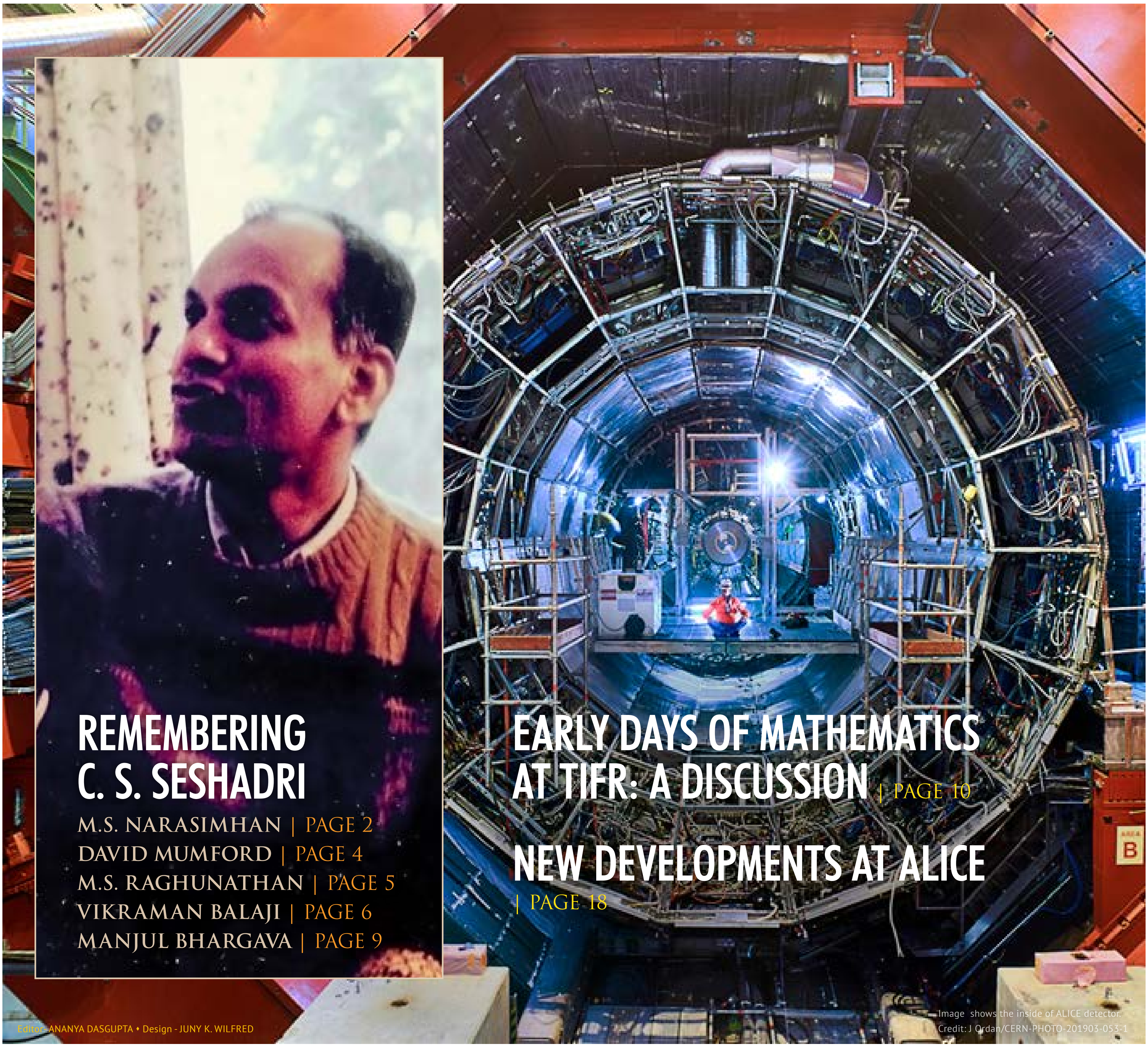
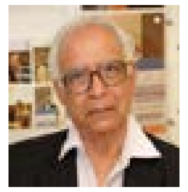


REMEMBERING C. S. SESHADRI

Image shows the inside of ALICE detector.
Credit: J Ordan/CERN-PHOTO-201903-053-1

Editor - ANANYA DASGUPTA • Design - JUNY K. WILFRED



C. S. SESHADRI

M.S. NARASIMHAN

C.S. Seshadri was a deep mathematician and a major figure in the field of algebraic geometry. His contributions to research and education in mathematics in India were invaluable. In particular, he played a key role in the development of the School of Mathematics of the Tata Institute of Fundamental Research (TIFR), Bombay, as a world-renowned centre of mathematical research, and in the founding of the Chennai Mathematical Institute (CMI).

I knew Seshadri from the days we were students in Loyola College, Madras. We joined what was known as the 'Intermediate class' there together, in 1948, and studied for B.A. (Hons) from 1950 to 1953. The mathematics students in the Intermediate class were divided into two sections; there was a special and difficult examination in mathematics, and the person who got the first place in each section was awarded a prize called the Racine Prize. Seshadri and I were studying in different sections, and having topped our respective sections, we were both awarded this prize. I believe this is how we first came to know about each other. The prize was named after Father Racine, a French Jesuit priest and mathematics teacher at Loyola College, who had a decisive influence on our early mathematical formation (and, indeed, on that of many young mathematicians of that time). It was Father Racine who suggested that both of us apply to the newly established TIFR to do our PhD studies.

Our close friendship started when we joined TIFR as research students in 1953 (on the same day!). From then on, we had parallel academic careers in TIFR, progressing up each step of the academic ladder together.

C.Musili, C.S. Seshadri, M.S.Narasimhan, M.S.Raghunathan and David Mumford at the TIFR Collonade. Credit Chennai Mathematical Institute

Already as research students we interacted very closely, reading and learning together an enormous amount of mathematics; this played an important role in our future mathematical development. We spent three years together in Paris in the late 1950's. In Paris, Seshadri came under the influence of, among others, Chevalley and Serre, and it is during this period that his definitive move to algebraic geometry took place.

We both returned to Bombay and TIFR in 1960. By that time, both in topology and differential geometry, fibre bundles were very well understood, and it looked as though the time was just ripe to go ahead and develop the theory of vector bundles in algebraic geometry. Around 1963, Seshadri and I started thinking about the problem of holomorphic vector bundles on compact Riemann surfaces, which resulted in the Narasimhan-Seshadri theorem, which is considered to be a major breakthrough. In retrospect, it looks as though we were unconsciously and independently equipping ourselves during our Paris days with all the knowledge and tools which would play a part in this work, although at that time we had no idea that we were going to work on this problem. We were also very lucky, because in our student days in Bombay, even before going to Paris, K.G. Ramanathan had told us about the work of Andre Weil on the generalization of abelian functions. He had heard about this work from Carl Siegel. For no particular reason at all, we had immediately started a seminar on this paper, and Seshadri gave some lectures. (So, in some sense, we were aware of this problem from our student days). This paper of Weil (which was not then well known) dealing with unitary bundles, and the work of David Mumford on stable bundles, were important inputs in our work.

When we first started working on this problem, we were initially a bit hesitant, wondering whether our capacities were sufficient to enable us to make a breakthrough, especially in a domain that many renowned mathematicians had already moved into. When we succeeded, we felt that our achievement was somehow more than commensurate with our powers. In a recent conversation, Seshadri and I were reminiscing about those days. We wondered how it was that we managed to succeed, that too where so many others had faltered. Seshadri had a simple explanation: *"It was grace!",* he said.

Although we did not formally collaborate after this work on bundles, I always profited by his scholarship in algebraic geometry and also by his advice on practical matters.

A Biographical Sketch

Seshadri was born in Kanchipuram on 29 February, 1932. His childhood and schooldays were spent in Chengalpet. He studied in Loyola college, Madras, from 1948 to 1953, and obtained his B.A. (Hons) degree in mathematics.

On Racine's advice, Seshadri joined TIFR as a research student in 1953. He received his Ph.D. degree in 1958 from the Bombay University. His thesis adviser was K. Chandrasekharan. He spent three years (1957 to 1960) in Paris, and returned to TIFR in 1960. At TIFR, he was a member of the mathematics faculty till 1984, when he moved to the Institute of Mathematical Sciences, Chennai. During his stay in TIFR, an active group in algebraic geometry was established. Some of his memorable and deep research work was done during this period.

Seshadri was also a good organizer and a successful academic entrepreneur. To many people, this came as somewhat of a surprise, as he gave the impression of being an extremely absent minded person; he was also well-known as someone who would often not complete his sentences when speaking, but instead trail off, leaving one to deduce the rest! However, for those who knew about the enormous amount of concentrated effort and energy he invested in his mathematical endeavours and the tenacity with which he would pursue a problem till a solution was found, this would not have come as a surprise.

In 1989, he established the Spic Mathematical Centre, later renamed the Chennai Mathematical Institute (CMI). CMI has been a success story, and Seshadri was proud of his role in its formation. Apart from being a first-rate research centre, CMI also runs excellent undergraduate and graduate education programmes in mathematics and computer science. One of the motivations for the creation of CMI was Seshadri's strong feeling that an undergraduate programme intended for training people for research should be taught by researchers, and that young undergraduates should directly come into contact with those doing research. He had, I think, as a model the Ecole Normale Superieure, Paris. In my opinion (shared by many), CMI is now among the best undergraduate institutions in the world in the fields of mathematics and computer science.

Seshadri retired as Director of CMI in 2010, but continued to pursue research actively even after retirement.

In addition to mathematics, his other great passion was music: Seshadri was deeply interested in Carnatic music, and was himself an accomplished musician. He had a genial personality, and enjoyed interacting with people.

He passed away on 17 July 2020. He leaves behind two sons.

Honours and Awards

Seshadri received several prestigious awards and honours. He was elected a fellow of the Royal Society, London, in 1988 and a Foreign Associate of the US National Academy of Sciences, in 2010. He was a Fellow of the Indian National Science Academy, the Indian Academy of Sciences and the American Mathematical Society. He received the Bhatnagar Award in1972, and the TWAS Trieste Science Prize for his distinguished contributions to science in 2006. He was awarded the Padma Bhushan by the President of India in 2009. He received Docteur Honoris Causa from Université Pierre et Marie Curie (UPMC), Paris in 2013, as well as Honorary Doctorates from the University of Hyderabad and Banaras Hindu University.

Seshadri's Mathematical Contributions

The following brief description of his work gives an idea of the depth and breadth of his work.

In response to a question of Serre, as to whether every vector bundle on the affine space is trivial (equivalently, whether every finitely generated projective module over a polynomial ring over a field is free), C.S. Seshadri proved in 1958 that vector bundles on the affine plane are trivial. This work attracted considerable attention and made Seshadri's name first known in the mathematical community. (The general case was settled by D. Quillen and A.A. Suslin about 15 years later.)

In 1965, M.S. Narasimhan and Seshadri proved the fundamental result relating stable vector bundles (an algebraic concept defined by David Mumford) on a compact Riemann surface to unitary representations of the (orbifold) fundamental group of the surface. This celebrated and influential result, known as the Narasimhan-Seshadri theorem, has served as a model for a large amount of literature intertwining algebraic geometry, differential geometry and topology, and was generalised in various directions. This result opened up a whole new field of moduli of algebraic vector bundles.

Seshadri (who had earlier constructed the Picard variety of a complete variety and proved its universal property) also constructed the compact (projective) moduli spaces of vector bundles on curves. This work, which introduced the notion of what is now known as S-equivalence (Seshadri equivalence), inspired and served as a model later for several constructions of moduli spaces.

He also introduced and studied the notion of parabolic bundles.

Seshadri worked extensively on the construction of quotients in algebraic geometry, and also on Geometric Invariant Theory, originally motivated by its connection with moduli problems. He proved Mumford's Conjecture (that a linear representation of a reductive group is geometrically reductive) first in the case of GL(2) and then, using global geometric techniques, in the case "stable = semi-stable". In this work he also proved a very useful criterion for ampleness of a line bundle, now known as Seshadri's criterion of ampleness, leading to the recent vast literature on "Seshadri constants". The Mumford conjecture was proved in full by W. Haboush. More recently, Seshadri has given a geometric proof of this theorem.

One important series of works, by Seshadri in collaboration with V. Lakshmibai, C. Musili, and others, is the theory of Standard Monomials, which began with a goal of giving characteristic-free descriptions of homogeneous coordinate rings of homogeneous spaces for classical groups, and ultimately became an important technique in representation theory. □

M.S. Narasimhan is an eminent mathematician, best known for the Narasimhan–Seshadri Theorem. He was among the main pillars of the famed School of Mathematics at TIFR, where he spent most of his professional life. He later spent some years building mathematics at ICTS, Trieste. He is now associated with the Indian Institute of Science and TIFR-Cam, Bangalore.

A group photo of the 1953 BA (Hons) mathematics class, Loyola College, Chennai.

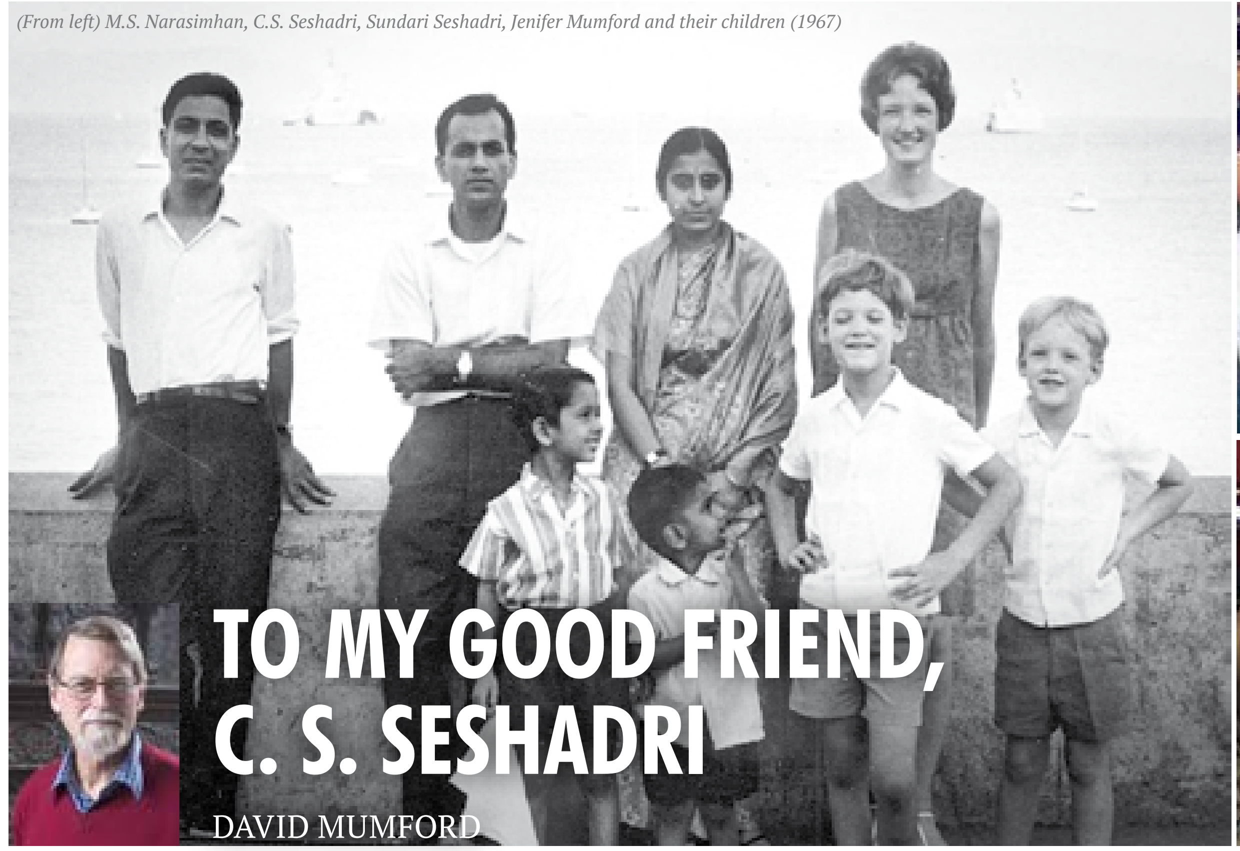
B.A.(Hons.) & M.A. 1953

Messrs. N. Sethuraman, A.P. Michael (Librarian), S.R. Seetharaman, T. Manivannan, S. Narayanan, Rev. Fr. Mathias, S.J.(Principal), Rev. Fr. C. Racine, S.J. (Head of the Department) V. Krishnamurthy, A.O. Kunju Paulo, S. Anantanarayana Iyer.

Messrs. M.C. Narasimhan, V. Vasudevan, V. Krishnamachari, V. Balasubramanian, K. Sadasiva Rao, S.C. George, C.S. Seshadri, M.J. Joseph, S. Gopalakrishnan, P.S. Ramachandran, T.N. Srinivasan,M.S. Rajagopalan.

Messrs. N. Ramabhadran, V. Padmanabhan, V.C. Varadachari, T. Narayana Rao, S. Ranganathan, N. Ragavendra Bhat, V. Seetharaman, T. Venkatachari, V. Thulasidoss, T. Thirumalachari, A.S. Vagulaparanan.

Messrs. M.S. Narasimhan, P.V. Sekhara Varier.



(From left) M.S. Narasimhan, C.S. Seshadri, Sundari Seshadri, Jenifer Mumford and their children (1967)

TO MY GOOD FRIEND, C. S. SESHADRI

DAVID MUMFORD

C.S. Seshadri with David Mumford, S. Ramanan and others. 1994

C.S. Seshadri with David Mumford

Seshadri, as I called him because this is his given name in the Indian tradition, was not only my mathematical colleague but one of my closest friends. Our connection began when, sometime in the early 60's, I received a letter with exotic Indian stamps on it. The letter was from Seshadri, whose work on Serre's conjecture I knew, but India, little more than a dozen years after independence, still seemed an exotic very distant place. The mathematical world was small in those days, largely concentrated in Cambridge, Princeton and Paris, and jet plane travel was just starting. I was thrilled to learn that he and M. S. Narasimhan, halfway around the world, had found exactly the same class of vector bundles on an algebraic curve as I had, a class for which a compact moduli space could be constructed. This was amazing because their method was totally different from mine, the sort of unexpected link that makes you fall in love with math.

We got together, first his visiting Harvard in 1966, then I went in 1967 with my family to Bombay, as the city was called then. The Tata Institute had just been built, standing like a mirage facing the Arabian Sea with manicured lawns and seriously air conditioned. I had to keep a sweater in my office. The Institute had a world class library, at the far end of whose stacks, I, like everyone else, did Saraswati Puja, the Hindu worship of the goddess of learning and music, on her sacred day. But best of all, working at the Institute was a powerful group of algebraic geometers who were rapidly uncovering the secrets of moduli spaces. We would talk daily, at morning coffee, lunch or afternoon tea, often in the West Canteen. The brass fittings in the elevator were "polished up so carefullee" just as in the Gilbert and Sullivan song. Fresh flowers were brought to my office every day too from the abundant gardens that Homi Bhabha, the Tata Institute's founder, had considered an essential part of a modern science institute. Bhabha and the Institute were the 20th century version of Kubla Khan and the *"stately pleasure dome"* in Coleridge's poem. Bhabha was similarly revered but sadly recently deceased.

Curiously, Seshadri and I never wrote any joint papers but instead, our research intertwined for many years. I recently found a letter written in 1970 where I wrote *"Thanks for your letter which is beautiful and very encouraging. I was personally getting quite discouraged about extending my result when your letter arrived. The blowing up trick is marvelous…"* This may be an odd thing to say but the wonderful thing he did led, with many twists and turns, to making my book "Geometric Invariant Theory" irrelevant! He went deeper than me and realized that the key thing you needed was a powerful criterion for ampleness and, bingo, you have a projective moduli space. My approach had been based exclusively on quotient spaces but, when extended from curves to surfaces, the method encountered baroque complexities, as shown when David Gieseker tackled this issue. Seshadri cut through all of this, one big piece being the "trick" (more precisely, an ingenious, startling idea) referred to in that letter. And, as time went on, in the hands of Janos Kollar and many others, his work led to a deep construction and understanding of many more moduli spaces.

Seshadri and his family also intertwined with me and my family during all the decades since then. Our boys played together, he became the godfather of my daughter Suchitra, and his family lived next door to us one year when he was visiting Harvard and Northeastern. I was truly honored when he came all the way from Chennai to Cambridge for my 80th birthday – as well as for the more significant Hindu milestone of a 1000th full moon. He opened his doors to all my children as they grew older and travelled on their own.

But what I really want to describe, the thing that made the deepest impression on me, was the way he integrated so fully and naturally his love of traditional Indian teachings and customs and his full awareness of the energy and force of Western culture. Unlike his fellow Tamilian mathematician S. Ramanujan who subsisted on boiled potatoes in Cambridge University, when Seshadri came to the West, Paris in his case, he sampled its food and drink and enjoyed all the pleasures of French culture. In all his visits to the West, he was comfortable wearing appropriate Western garb at the office and relaxing in his lungi when he got home. He lived in the most unpretentious way, resisting the Western impulse for the latest gadgets and for collecting expensive ornaments. Yet he could fly to Delhi and argue effectively with high-up officials and ministers for funding for mathematics. His great passion, almost as strong as his love of mathematics, was singing classical South Indian Ragas which he performed at a professional level. As the eldest son, he could haul out his sacred thread when rituals demanded this symbol. Walking with me once in some quiet Maine woods, uncut for a century with no houses or roads nearby, he remarked that this experience helped him understand the early Indian sages whose woods are now long gone, replaced by villages and fields. I will miss him for as long a time as is allotted to me. ☐

David Mumford is a distinguished mathematician known for his path-breaking work in algebraic geometry and his research into vision and pattern theory. He is currently a University Professor Emeritus in the Division Of Applied Mathematics at Brown University. Mumford has had close contact with C. S. Seshadri and his work over the years. He even spent a year at TIFR, Mumbai.

C. S. SESHADRI: GREAT MATHEMATICIAN, INSTITUTION BUILDER AND AFFABLE FRIEND

M.S. RAGHUNATHAN

Srinivasa Ramanujan is a household name in the country; and rightly so – he ranks among the greatest mathematicians of the twentieth century. But that very greatness has cast a long shadow on his worthy successors – there are indeed a few – that has obscured their public visibility. One of these was Conjeevaram Srinivasachari Seshadri who passed away on 17th July in Chennai at the age of 88.

Seshadri ranks among the greats of the mathematical world of the twentieth century. He did pioneering work in Algebraic Geometry (some of it in collaboration with M. S. Narasimhan, another great mathematician and a close friend, also from Chennai). His work has had tremendous impact on the very way in which the field developed during the last six decades. Despite that kind of professional achievement Seshadri remained always a pleasant, accessible human being. He was certainly aware of the high worth of his work, yet he was far from egotistical, and seldom spoke about himself. Another manifestation of his humility was his transparent enthusiasm for good work by others, especially students and colleagues at TIFR. He was one of the principal architects behind the rise of the School of Mathematics at the Tata institute of Fundamental Research (TIFR) from the fledgling thing that it was in the fifties to international eminence in a decade and a half. His own research contributed a great deal to this, but his mentoring of a large number of students contributed no less. He won several awards and honours, among them the Bhatnagar Prize, Third World Academy Prize and the Trieste Science Prize; Fellowships of the three national science academies and of the Royal Society and Foreign Fellowship of the U S National Academy of Sciences. He was awarded the Padmabhushan in 2009.

Seshadri had his early education in Chengalpattu, a small town in Tamil Nadu about 60 kilometres from Chennai, where his father practised as an advocate. The family originates from the temple town of Kanchipuram (Conjeevaram is the anglicised version). He went to College in Chennai and secured a B A (Honours) degree in mathematics from the venerable Loyola College. Seshadri had a good record of performance at school and college, but it was by no means exceptional. In Loyola College he came under the influence of Rev. Fr Racine, a fine mathematician; and it was on Racine's recommendation rather than his performance in the B A (Honours) examination that TIFR called him for a selection interview for a research studentship. Apparently he performed brilliantly and was selected. It was one of the shortest among the interviews of candidates who were eventually successful. He joined TIFR as a Research Scholar in TIFR in 1953. Some interesting early work resulted in his securing a post-doctoral position in Paris even while retaining his job as a "Research Assistant" in TIFR. In Paris he made the first dent in a problem posed by J.P. Serre which had attracted wide attention; with that and some more interesting work he became well known in the international mathematical community. During his 3-year stay in France he benefitted immensely by his association with some eminent mathematicians, particularly, C Chevalley and A Grothendieck. On his return to TIFR in 1960, he ran a seminar on Algebraic Geometry which introduced to his colleagues and students the then recent developments initiated by Grothendieck.

It is in the mid-sixties that Narasimhan and Seshadri came up with their path breaking work on "Modulii of vector bundles on a compact Riemann surface". This work brought out a connection between certain purely algebraic geometric entities and some transcendental constructions (devised by them). In the next decade Seshadri concentrated on questions connected with this work and came up with many interesting and important results, some in collaboration with colleagues. Later Seshadri turned his attention to another sub-area of Algebraic Geometry, the study of algebraic homogeneous spaces. Here again he did path-breaking work: his "Standard Monomial Theory" enabled one to get a good understanding of algebraic homogeneous spaces (which was earlier confined to Grassmanians). He continued working in this sub-area till the end, a good part of it in collaboration with his students C S Musili and Lakshmibhai.

Seshadri left TIFR in 1984 for personal reasons and joined the Institute of Mathematical Sciences (IMSc) where E C G Sudarshan had recently been appointed as Director. He had a 5-year stint at the institute during which he managed to gather a viable group of good mathematicians there. However he left IMSc because of differences with the director; also he wanted to implement a long cherished idea of his: to get undergraduates taught by active research mathematicians. Shri A. C. Muthiah, then Chairman SPIC, got that company to support Seshadri's idea by setting up a "School of Mathematics" within its "SPIC Science Foundation". Eventually this "School" became an autonomous independent institution – The Chennai Mathematics Institute. For many years during his TIFR days Seshadri avoided taking an active role in the administration. However, he did at some point take on the responsibilities of the Dean, School of Mathematics, and that too, during a difficult period; and he displayed one important

(From left) M. S. Narasimhan, C. S. Seshadri, S. Ramanan and M. S. Raghunathan

RAGHUNATHAN | *continued from Page 5 ...*

quality of a good administrator – the ability to delegate responsibilities to the right people. His talent for administration came to the fore especially with the creation of CMI. CMI which today has grown into a leading institution for undergraduate mathematics education in the country, stands witness to Seshadri's vision as an academic as well as an institution-builder.

Seshadri was affable and easy to get along with, both at the personal and the professional levels. He was a good listener and would seldom hold forth on any subject. He had a self-effacing charm that won him friends instantly. He had a deep interest in music, almost rivalling his passion for mathematics and had a large number of friends in music circles. He was almost a professional concert-level performer. Many of us at TIFR enjoyed good personal relationships with Seshadri and his wife Sundari. They would have people over quite often and their gracious hospitality and Sundari's culinary talents were much talked about in our circles. Sundari passed away last October, and that loss may well have hastened his own end.

Sesahdri's passing away is of course a great loss to mathematics and the country; and the loss is the greater for his personal friends. I had a long association with him from my student days. His encouragement was a great source of strength for me at that time as well as later when I became a colleague. My family and Seshadri's were close to each other; his death (as was Sundari's, earlier) is for us a grievous loss indeed. □

M. S. Raghunathan is a distinguished mathematician, who has played a pivotal role in the enormous success of the School of Mathematics at TIFR. He has made foundational contributions to the theory of Lie groups, algebraic groups and their subgroups. He is currently a distinguished visiting professor, DAE-MU Centre for Excellence in Basic Sciences, Mumbai.

BETWEEN THE SCIENCE

AMIT APTE has been selected for the SERB-VAJRA faculty scheme along with Amarjit Budhiraja of University of North Carolina at Chapel Hills, USA as overseas faculty.

ARCHAK PURKAYASTHA, a former doctoral student at ICTS, has been awarded the prestigious Marie Skłodowska-Curie Actions Individual Fellowship.

VISHAL VASAN was selected as an associate of the Indian Academy of Sciences.

C.S.SESHADRI, A MATHEMATICAL LUMINARY

VIKRAMAN BALAJI

Conjeevaram Srirangachari Seshadri was a mathematical luminary of the 20th Century in post-independent India. Seshadri was born on February 29th, 1932, in Kanchipuram. He was the eldest among twelve children of his parents, Sri C. Srirangachari (a well-known advocate in Chengleput, a town 60 kms south of Chennai) and Srimati Chudamani. Seshadri's entire schooling was in Chengleput. He joined the Loyola College, Chennai in 1948, and he graduated from there in 1953 with a BA (Hons) degree in mathematics. Seshadri married Sundari in 1962.

During his years at college, Prof. S.Narayanan and Fr. C. Racine played a decisive role in Seshadri's taking up mathematics as a profession.

Seshadri joined the Tata Institute of Fundamental Research, Mumbai in 1953 as a student. He received his Ph.D. degree in 1958 from the Bombay University for his thesis entitled "Generalised multiplicative meromorphic functions on a complex manifold". His thesis adviser was Professor K. Chandrasekaran who shaped the mathematical career of Seshadri as he did for many others.

Seshadri spent the years 1957–60 in Paris, where he came under the influence of many great mathematicians of the French school, like Chevalley, Cartan, Schwartz, Grothendieck and Serre.

He returned to the TIFR in 1960 and was a member of the faculty of the School of Mathematics until 1984, where he was responsible for establishing an active school of algebraic geometry. He moved to the Institute of Mathematical Sciences, Chennai in 1984.

In 1989, Seshadri became the director of the Chennai Mathematical Institute, then called the SPIC Mathematical Institute, founded by A.C. Muthiah.

Seshadri is a recipient of numerous distinctions. He received the Bhatnagar Prize in 1972 and was elected a fellow of the Royal Society, London in 1988. He has held distinguished positions in various centres of mathematics, all over the world. In 2006, Seshadri was awarded the TWAS Science Prize along with Jacob Palis for his distinguished contributions to science.

In the past five years since he received the National Professorship, Seshadri has been awarded the H.K. Firodia Award for Excellence in Science and Technology, Pune, 2008, the Rathindra Puraskar from Shantiniketan's Visva-Bharati University, Kolkata, 2008, the Padma Bhushan by the President of India, 2009. More recently, he was elected a Foreign Associate of the US National Academy of Sciences, 2010. In 2013, Seshadri was awarded Docteur Honoris Causa of the Université Pierre et Marie Curie in Paris.

He passed away on the 17th July in his home in Mandaveli, Chennai (1). He is survived by his sons Narasimhan and Giridhar and four grandchildren Sanjana, Rangasai, Dev and Anant. Seshadri had been suffering from Parkinson's for the past several months and after the passing of his wife Sundari in October 2019, his condition had been deteriorating.

Seshadri, the person

I have known Seshadri as his doctoral student since 1984 and later as his collaborator and colleague at the Chennai Mathematical Institute. I vividly remember his lectures. Notes were prepared with utmost meticulousness and the talks were quite spartan but always insightful. Every lecture had something as a take-away for an aspiring researcher. Getting praise from him was something of a rarity. This used to come only as an award for something which he considered insightful and this was hard to come by for most of us. Many years later, when I was in my forties, after I felt I had done something really significant, he came up to me and said *"There is meat in your work. Now I can say you are a mathematician!"*

I have witnessed his personality from close quarters for over three decades. As a mathematical personality, I saw someone unique in his vision and insight, an uncanny ability to consistently strike gold in a vast world of mathematics. He was extraordinarily generous with his ideas and shared his insights with one and all and this extreme generosity was his human side as well. His only caveat was that the listeners go back and pursue the ideas to the best of their abilities. There was a complete awareness of his own stature while being modest and humble at the same time. A unique sense of humour and sympathy was the hallmark of his personality, with interests ranging widely from mathematics, philosophy, politics and music. He was confident of his insights and this made him unperturbed during several moments of crisis that the institute faced. I quote Professor K Chandrasekharan, who in a letter to Seshadri on 10 Feb 2013 writes *"I cherish the values that inspired the creation of CMI and your unswerving commitment to those values"*. Seshadri will be remembered for these values.

Seshadri was also an accomplished exponent of the Carnatic Music and till a few days before his passing, he continued to share his musical knowledge and insights with a young musical student Maitreyi from CMI. Seshadri was trained by his grandmother who herself was a student of Nainapillai. Seshadri showed the same traits in his musical discipline as in his mathematical ones. He meticulously did riyaaz and his repertoire in Muthuswamy Dikshitar's kritis and Shyama Sastry's kritis was noteworthy. I have had several occasions of listening to his music which can be described as a royal gait profoundly suited to expressing Dikshitar's kritis. While singing, a distinctly spiritual side of his used to come to the fore. By a spiritual side, I do not mean anything religious, but a musical one which bore the stamp of an immense sadhana, where every nuance was expressed with a spiritual feeling which was way beyond religious emotion.

I close with lines from W.H. Auden (Hymn to the United Nations):

Like music when
Begotten notes, New notes beget.
Making the flowing of time a growing.
T'is what it could be ...
When even sadness, Is a form of gladness.

Seshadri's mathematics in brief outline

Seshadri's doctoral thesis entitled "*Generalised multiplicative meromorphic functions on a complex analytic manifold*" gave an independent proof of the so-called Birkhoff-Grothendieck theorem on vector bundles on the projective line.

Historically: Grothendieck proved this for arbitrary principal bundles, while the result on vector bundles goes back to Dedekind, R. and H. Weber (1882). Theorie der algebraischen Functionen einer Veränderlichen. J. reine und angew. Math. 92, 181–290. (2)

Seshadri shot to fame early during his visit to Paris. Serre posed the following problem: Over n-dimensional affine space, are there non-trivial vector bundles? In other words, is the following statement true? Is any projective Noetherian module over $K[T_1,...,T_n]$, where K is a field, free? For $n = 1$, the ring $K[T]$ is an integral principal ideal ring. Therefore, any Noetherian torsion-free $K[T]$-module (in particular, any projective Noetherian module) is free (see for example Lang's Algebra). For $n = 2$, there are no non-trivial bundles, either and this theorem is due to Seshadri.

The affirmative answer in all dimensions (Any projective Noetherian $K[T1,...,Tn]$-module is free) is due to A. Suslin and D. Quillen which was done independently by them in the 1970's. Much work arose inspired by Seshadri's ideas, beginning with the work of Pavaman Murthy (Seshadri's first doctoral student) leading to a large body of very impressive work from TIFR.

Seshadri worked broadly in an area of mathematics called Algebraic Geometry. Around the time that he finished his doctoral work, the subject itself was undergoing a unique revolution in the hands of a French mathematical giant by name Alexandre Grothendieck. Seshadri went to Paris in 1957 and very quickly entered the sanctum of this new temple of algebraic geometry. This provided a distinctively unifying perspective which connected it to all branches of mathematics at some level. André Weil had published striking conjectures linking number theory to topology, two very distinct branches of mathematics and this was one of the driving forces behind the renaissance in Algebraic Geometry in the hands of Serre-Grothendieck and several others. Algebraic Geometry was somewhat mysterious in that it provided a wonderful synthesis of ideas by the process of providing a powerful language for the expression. Seshadri gave three talks in the Chevalley seminar one on Picard varieties and their compactifications and one on Cartier operations.


C.S. Seshadri


Alexander Grothendieck, C.S. Seshadri and Emile Borel

It is in this setting that one could view one of Seshadri's deepest researches, which began in collaboration with his friend and colleague M. S. Narasimhan. This work had its roots in the work of André Weil on *"generalization of Abelian functions"* (1938) and its foundations were closely linked to the work of Poincaré on the so-called *"uniformization theorem"*. I quote from the paper of Atiyah-Bott (1982)

The connection between holomorphic and unitary structures was already apparent in Weil's paper, and in the classical case of line bundles it is essentially equivalent to the identification between holomorphic and harmonic 1-forms, which in turn was the starting point for Hodge's general theory of harmonic forms.

The Narasimhan-Seshadri theorem sets up a correspondence between two basic classes of objects, namely *"irreducible unitary representations of fundamental groups of Riemann surfaces"* and *"a class of geometric objects called vector bundles on algebraic curves"*. The first class of objects was what could be termed *"topological"* and the second *"algebro-geometric"*. Let me be a bit more precise.

The non-abelian version of the Jacobian needed to tackle several basic obstructions. The first paper of Narasimhan-Seshadri (1964) handled the non-abelian structure of the fundamental group of X. They showed that the space of irreducible unitary representations of the fundamental group of a compact Riemann surface X was naturally a complex manifold.

On the side of bundles, the basic issue was that any decent topology on the space of bundles of a given degree and rank is necessarily *non-separated*. Moreover, such bundles are not *"bounded"* i.e. they cannot be parametrized by a finite number of varieties. Thus to obtain a *"projective moduli space"*, one has to restrict oneself to a suitable subclass of bundles which would give separatedness.

In early 60's David Mumford had revived and newly built the Geometric Invariant Theory of Hilbert into a superstructure, laying out strategies for construction of *"compactifications of moduli problems"*. One of his examples where he applied GIT was in constructing moduli space of bundles. His GIT naturally gave him the correct subclass and he defined slope-stability of bundles and he constructed moduli of stable vector bundles of degree d and rank r on curves as a quasi-projective variety. Recall that to every vector bundle V on a smooth projective curve, Mumford defines the slope $\mu(V) := \deg(V)/\mathrm{rank}(V)$ and a bundle is Mumford-stable if the slope μ strictly decreases when we restrict to a proper sub-bundle.

Narasimhan and Seshadri showed that irreducible unitary representations of the fundamental group of X correspond precisely to stable bundles of degree 0 on X. These were two of the scripts of a trilingual inscription à la the Rosetta stone, the third came up in the work of Donaldson in 1986. What followed was spectacular. Many subtle and beautiful aspects of differential geometry, topology, mathematical physics and number theory got unravelled miraculously.

They do more, they show how this can be extended to the case when the degree need not be zero. This case was a precursor to "*parabolic bundles*" which Seshadri later developed along with Mehta. Mehta and Seshadri prove the analogue of the Narasimhan-Seshadri theorem for unitary representations of more general Fuchsian groups by relating these to parabolic bundles on X.

Very recently, in a paper which appeared in 2015, Seshadri and I completed the picture by setting up the correspondence for homomorphisms of these Fuchsian groups to the maximal compact subgroups of semisimple groups. Parahoric torsors are the objects which extend parabolic bundles.

I now take up two papers of Seshadri, the first entitled "*Some results on the quotient space by an algebraic group of automorphisms*" and the second being "*Quotient spaces modulo reductive algebraic groups*" to which I will return later. The aspect that I wish to highlight here is somewhat general and does not really require the group to be reductive or even affine.

Question: ***Let X be a scheme on which a connected algebraic group acts properly. Then does the geometric quotient X/G exist?***

Recall that a G-morphism $f{:}X{\to}Y$ is called a good quotient if (1) f is a surjective affine G-invariant morphism, (2) $f_*(\mathcal{O}_X)^G = \mathcal{O}_Y$ and (3) f sends closed G-stable subsets to closed subsets and separates disjoint closed G-stable subsets of X. The quotient f is called a *geometric quotient* if it is a good quotient and moreover for each $x \in X$, the G-orbit $G.x$ is closed in X.

It is known that the question as stated above fails in general but Seshadri gave some basic criteria under which it holds. He proved the following theorem: let X be a normal scheme of finite type (or more generally a normal algebraic space of finite type over k) and G a connected affine algebraic group acting properly on X. Then the geometric quotient X/G exists as a normal algebraic space of finite type. When the action is proper, a geometric quotient is simply a topological quotient with the property $f_*(\mathcal{O}_X)^G = \mathcal{O}_Y$.

Seshadri developed the important technique of elimination of finite isotropies which goes as follows. Let X be an irreducible excellent scheme over k and G affine algebraic group acting properly on X. Then there is a diagram:

$$\begin{tikzcd} Y \arrow[r, "q"] \arrow[d, "p"] & X \\ Z & \end{tikzcd}$$

where Y is irreducible and G acts properly on Y. Further, p is a Zariski locally trivial principal G-bundle and q a finite dominant G-morphism with Y/X Galois with Galois group Γ whose action on Y commutes with the G-action. In a sense completing the square is the goal. These ideas are central to the major developments by Kollar and Keel and Mori on *"Quotients"* in the nineties.

I now come to Seshadri's contributions to Geometric Invariant Theory (GIT). In his paper on "*Unitary bundles*" Seshardri relates unitary bundles to the natural compactification arising from GIT construction of the moduli space. This is important as much of the subsequent work revolves around the study of compact moduli spaces. Irreducible unitary bundles are the "*simple*" objects of this theory. Two bundles are S-equivalent if they have the same Jordan-Hölder decomposition. The points of the compact moduli space are then the S-equivalence classes of bundles of degree zero and rank r. The beautiful convergence of two lines of thought about these fundamental concepts of (semi)stable bundles was well expressed by Mumford on the occasion of Seshadri's seventieth birthday:

"*But I guess what thrilled both of us – it certainly thrilled me – was when our work on vector bundles on curves arrived at the same idea from two such different directions. What a strange thing it was that three people (you, me, M.S.) on opposite sides of the world (which, by the way, seemed a lot bigger in those days) using totally different techniques should construct the same compact moduli space.*"

I will very briefly touch on a few other papers of Seshadri in the subject of GIT. This will give a feeling for the breadth and depth of his contributions. The first one was Mumford's conjecture for $GL(2)$ which apart from proving the conjecture gave a restricted "*valuative criterion*" which predates the famous Langton criterion. This approach of Seshadri's became the standard prototype for all moduli constructions, the most general one being the one by Simpson in the early nineties.

Let me define geometric reductivity of a group G. *Let G be a reductive algebraic group over an algebraically closed field k. Then G is* GEOMETRICALLY REDUCTIVE *if, for every finite-dimensional rational G-module V and a G-invariant point $v \in V$, $v \neq 0$, there is a G-invariant homogeneous polynomial F on V of positive degree such that $F(v) \neq 0$.*

Mumford's conjecture says the following: Reductive algebraic groups are geometrically reductive. This was first proved for the case of *SL(2)* (hence *GL(2)*) in characteristic 2 by Tadao Oda, and in all characteristics by Seshadri. W. Haboush proved the conjecture for a general reductive G in the 1974. Haboush's proof uses in an essential way the irreducibility of the Steinberg representation. A germ of this idea can perhaps be traced back to the appendix to Seshadri's paper by Raghunathan!

There is also a different approach to the problem due to Formanek and Procesi, à priori for the full linear group, but the general case can be deduced from this. Seshadri in the late 70's finally extended geometric reductivity over general excellent rings which is a basic tool for constructing moduli in mixed characteristics. Seshadri's paper on "*Quotients modulo reductive groups*" which has already been referred to before, has several beautiful ideas. He introduces the notion of "*G-properness*" which under some simple conditions shows that 4 quotients, if they exist, are "*proper and separated*". One of the basic results in this paper is the following: *Let X be a projective variety on which there is given an action of a reductive algebraic group G with respect to an ample line bundle L on X. Let X^{SS} and X^S denote respectively the semi-stable locus and the stable locus of the action of G on (X, L). Suppose that X is normal, $X^{SS} = X^S$, and G acts freely on X. Then the geometric quotient X^S/G exists as a normal projective variety. Loosely put, this is Mumford's conjecture when "semistable = stable".* Seshadri then gives a general technique to ensure the condition $X^{SS} = X^S$ can be made to hold. These have played a central role in several subsequent developments.

Seshadri (in the late 60's) wanted to prove the general Mumford conjecture using the geometric approach which was roughly equivalent to showing that the set Y of equivalence classes of semi-stable points for a linear action of G on a projective scheme X has a canonical structure of a projective scheme. The first difficulty is getting a natural scheme theoretic structure on Y. The second one, more difficult is to prove its projectivity. When "*stable = semi-stable*" Seshadri showed that Y is a proper scheme and the proof reduces to checking the Nakai-Moishezon criterion for L on Y. *This process led to Seshadri's ampleness criterion and Seshadri constants.*

Around 2009, Seshadri and Pramath Sastry completed Seshadri's old argument. The key new ingredient (work of Sean Keel) was to be able to prove that under some conditions, line bundles which are "*nef*" and "*big*" are semi-ample. It was a recursive property for "*nef*" line bundles to become semi-ample, in a sense a "*Nakai-Moisezon*" for semi-ampleness.

I now turn to give a very brief account of his work on standard monomial theory much of which in its later developments was a collaboration with V. Lakshmibai and C. Musili. The modern standard monomial theory was initiated by C. S. Seshadri in the early 1970's which was a vast generalisation of the classical theory of Hodge for the Grassmannians.

The broad aim of this theory was the construction of bases for the space of sections of line bundles on Schubert varieties which reflects the intrinsic geometry of the Schubert variety and the intricate combinatorics of the Weyl group. The theory has led to very fundamental developments in the fields of Representation theory, Geometry and Combinatorics.

Following a series of basic papers written in collaboration with V. Lakshmibai and being guided by careful analysis and a study of Schubert varieties for exceptional groups, Lakshmibai and Seshadri formulated the LS conjectures. The key point of the conjectures was that it gave an indexing of the SMT bases which implied a remarkable character formula now termed the LS character formula.

There was a second aspect to these conjectures which constructed bases for the usual Demazure modules associated to the Schubert varieties. Peter Littelmann proved these conjectures by bringing in fresh inputs and new ideas from the theory of Quantum groups.

Seshadri's contribution to mathematics education

The Chennai Mathematical Institute in its present form was founded in 1998 but its roots go back to 1989 when Seshadri founded a new institute, then called the School of Mathematics, SPIC Science Foundation. The Chennai Mathematical Institute (CMI) is a unique institution in India which attempts to integrate undergraduate education with research; it grew out of Seshadri's vision that higher learning can be had only in an atmosphere of active research amidst the presence of masters in the subject. It was a brave venture in the face of extraordinary opposition and skepticism even from his very close friends and well-wishers. It was his dream to build a center of learning which can compare itself with the great centers such as the École Normale in Paris, the Oxford and Cambridge Universities in England and the Harvard University in the U.S. It opens up opportunities for the gifted students in India to learn in this unique academic atmosphere and also gives possibilities for the active researchers to participate in this experiment which one believes will leave an everlasting influence on the development of mathematics in India.

It would not be an exaggeration to say that the Chennai Mathematical Institute is now rated as one of the best schools in the world for under-graduate studies in mathematics. This is indeed a first big step in its stride and much still needs to be done to fulfill Seshadri's dream. □

FOOTNOTES

[1] He was with his family when this happened. I dedicate this article to his family, his son Giridhar, daughter-in-law Padma, grandsons Ranga Sai and Anant and Seshadri-Sundari's daughter-helper over the years, Vadivu, whose devotion to both of them was exemplary.

[2] http://wwwmath.uni-muenster.de/u/scharlau/scharlau/grothendieck/Grothendieck.pdf for a nice historical outline.

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'We must ensure CMI's noble mission continues with great energy'

MANJUL BHARGAVA

Prof. C.S. Seshadri was a true inspiration. While he was an incredible mathematician, he was an even more incredible human being. His fundamental contributions to mathematics were only exceeded by his generosity (with his ideas, and with his time and resources) and the training he gave to a new generation of students - including through his visionary establishment of the Chennai Mathematical Institute (CMI). His love of music and history were on par with his love of mathematics and teaching - he was indeed a master of many arts - and he employed these passions to excite students of all ages.

In the long term, his greatest contribution may be CMI. It is rare for someone of such academic stature in their discipline to also go out and build an educational institution of the calibre of CMI. CMI was set up with such thought, care, and attention to detail - and the results confirm this. CMI has become one of the premier (if not the premier) institutes for undergraduates to study mathematics in India in a multidisciplinary manner. CMI has already graduated hundreds of students, so many of whom have gone on to become leading professional mathematicians and teachers in their own right. That is a gargantuan and permanent contribution to the country that India and the world will never forget.

The best way we can continue his remarkable legacy is to ensure that CMI's noble mission continues with great energy, vigour, and adequate resources. We must make sure to support his institute, and create and foster others like it across subjects and across the country, so that future generations may continue to benefit from his vision.

On a more personal note, I always made it a point to visit Prof. Seshadri whenever I was in Chennai. His enthusiasm, optimism, brilliance, and deep knowledge made every moment with him a joy. We shared many common interests from music and history to literature and mathematics. We could talk for hours on all topics despite being generations apart. His childlike enthusiasm for all subjects was contagious. His generosity and desire to create outstanding opportunities for the education of young people inspired me. I will miss him deeply. ■

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MATHEMATICS AT TIFR
A DISCUSSION SESSION

In February 2020, before the global pandemic COVID-19 forced the country to lockdown, some of India's best-known mathematicians were in ICTS-TIFR to attend a program on Moduli of Bundles and Related Structures. The inaugural lecture of the Madhava Lecture Series was also delivered during this time by P. P. Divakaran. The Madhava Lectures are delivered by eminent scholars of the history of mathematics, science and technology. Madhava of Sangamagrama was a 14th century Indian mathematician and astronomer who made pioneering contributions to the study of infinite series, calculus, trigonometry, geometry and algebra. He is considered to be the founder of the Kerala School of Astronomy and Mathematics.

We took the opportunity and organized a discussion session to record the early days of the School of Mathematics at TIFR,
its culture of excellence and the history of the path-breaking work on vector bundles by M.S. Narasimhan and C.S. Seshadri. During this session we attempted to record how a remarkable ethos was nurtured and how it attracted the most brilliant minds of the country, as well as how the School of Mathematics further developed.

The American mathematician Carlos Simpson then told us about the impact of the Narasimhan-Seshadri theorem globally and its larger impact in mathematics. P.P. Divakaran talked about its connections with physics. We hope that this session would serve as a valuable resource for science institutions across India.

The people present were M.S. Narasimhan, M.S. Raghunathan, P.P. Divakaran, Spenta Wadia, Rajesh Gopakumar, Carlos Simpson, Rukmini Dey and Pranav Pandit. C.S. Seshadri joined in the discussion via video conference from Chennai.

Here is the full transcript.

MS Narasimhan: Let me start by saying that the historical conditions at the time of independence played a very important role [the success of the School of Mathematics at TIFR]. Of course that's not enough and you will hear about the other things that happened.

Soon after Independence, Jawaharlal Nehru became the first Prime Minister. He understood the importance of science, particularly of mathematics and what role it played in science and general economic development. He also understood the importance of mathematics as an intellectual activity. He has written about all this. And at the same time he gave support to some well-known scientists like Homi Bhabha. The time saw how politicians, very good scientists and also high level administrators came together and made it their mission to build science. I think the third point is very important. I also had to deal with some of these people later and they gave complete support. This was the historical background. Then many brilliant young people came together. And everyone wanted to do something important.

CS Seshadri: As Narasimhan was saying, the historical context is very important. At the time when India got independence, there was a certain minority that was somewhat idealistic. And it so happened that there was a person like Homi Bhabha, who had a broad overall view, including that of the importance of mathematics. He was a physicist but understood the importance of mathematics. And, of course, finally the stature of the Prime Minister – we were very lucky to have someone like that.

Before the Tata Institute came up, there was an effort to build a school of mathematics in Chennai. But that didn't go through so well. Then after independence, with the creation of the Tata Institute of Fundamental Research, India could show that there was a group of people and not just one person of the highest ability. Tata Institute also attracted some of the very best students in the country. K. Chandrasekharan [KC] played a very important role here. He invited the very best people from abroad, people he thought we should interact with. He believed we should not be only working on existing problems in arithmetic or number theory – most of the people then were working on analytical number theory. Of course, he understood that Ramanujan's work also required a greater culture in number theory.

At that time, the Institute had a host of brilliant people like Ramanan, C. P. Ramanujam, Raghavan Narasimhan, Ramanathan and a host of other people. Ramanathan was the person who followed our work in vector bundles.

So, you see, several things happened together. By chance and also by some deliberation. Here we were a group of people who were engaged in great work. And fortunately, we came up with some basic results – one of which was our vector bundles. We still have excellent people working at the Tata Institute.

Spenta Wadia: I would like to ask a question to both Narasimhan and Seshadri – your famous work on vector bundles on compact Riemann surfaces, how did you arrive at working on this problem? Was there some natural development that led to that theorem?

CSS: Narasimhan was running a seminar which I also joined. We had an excellent seminar on Hermann Weyl's old book, which covered the uniformization theorem in depth. Another catalyst for our work on vector bundles was our older colleague Professor Ramanathan who used to talk about Carl Ludwig Siegel, and André Weil's work on the generalization of abelian functions or theta functions to higher rank, non-abelian analogues. André Weil's paper Généralisation des fonctions abéliennes was a famous paper and that was the starting point of the seminar and led to really great problems.

Another thing that happened was that Narasimhan was in the hospital when we were visiting Paris. We were there for three years. He used to read Kodaira-Spencer and I was beginning to study algebraic geometry with Chevalley. We started asking questions about unitary vector bundles; questions like *what is the "tangent space of the moduli?"*. Nowadays one uses Schlessinger's theory to understand the tangent space of moduli problem, but its study started with Kodaira-Spencer and the question, *'if you have a moduli problem, what is the tangent space?'* We started in this way. Then [eventually] the connection with Mumford's work, etc, led to our work.

MSN: I think I should explain the atmosphere in Tata Institute then that made this possible. The person who was starting the culture was K. Chandrasekharan [KC], helped by KG Ramanathan [KGR]. For those who don't know about KC, he spent some time in Princeton and had been an assistant of Hermann Weyl. He was on very good personal terms with Weyl, Siegel, Von Neumann and of course Bochner as well. So he and KGR understood how mathematical research was carried out in the USA and as well as in Germany. They were open to a much wider scenario.

Now as Seshadri mentioned, you will be surprised but there were no graduate courses in India at that time. So the first graduate course we had was by Ambrose. The students of the course, who didn't know to start what a topological space was, at the end of three months knew Haar measure, spectral theorem for bounded self -adjoint operators, Peter-Weyl theorem and so on. Then next year, in 1955, Samuel Eilenberg visited and gave an excellent course on algebraic topology. I still have with me my hand written copy of his lectures. He taught that whole course from the functorial point of view without once mentioning the word category or a functor. At that time those concepts were new and we were exposed to functorial way of thinking (and to Eilenberg -Steenrod axioms by Eilenberg himself). Then there was the course of Laurent Schwartz on complex analytic manifolds, which dealt with, among other things, currents, de Rham theorem, harmonic forms and Kahler manifolds. This course had a profound influence on the future work of Seshadri and myself.

This was one background of the work culture at the Tata Institute at that time. The second thing was that students were given complete freedom to read and work on whatever topic they wanted, as long as KC and KGR were convinced that the student was capable and serious.

CSS: Let me add here – we never knew what field we were working on. *[Laughs]*

MSN: Yes.

Then we were also exposed to what was happening elsewhere in mathematics at the highest level. For example, the Cartan Seminars were published by some small institution in Paris and it was very hard to procure copies, but TIFR library had them all. In fact, I once asked Chandrasekharan how he had got them. He said, 'Andre told me to get them, so I got them'. Andre, of course, was Andre Weil.

So you see, we were aware of what exactly was going on in mathematical research. And also, later when we were worked on research, it was somewhat important to be away from big centres, not to be too much influenced or overpowered by very strong personalities. So this was the atmosphere.

CSS: I would say we were encouraged to work with a certain kind of originality. In India now, the students during their undergraduate years, want to go abroad and do their PhD. In my institution also there are such people and we don't discourage them. But then when a few excellent people are thrown together in a place, even in India, something big can be built. They need to work together and select their problems. I believe CV Raman also said something similar like that – you chose your problems.

Narasimhan and I agree that sometimes very good people get absorbed into the very big places in the United States and are a bit lost. I am not saying there is anything wrong with these places. That's why places like the Tata Institute with individual character, excellent standards and excellent people coming together is very important. And that is exactly what was happening in India at that time. And there were two such places in India then – the Tata Institute and the Indian Statistical Institute.

Rajesh Gopakumar: Was it very common at the time for mathematicians to be collaborating?

MSN: We never thought of it as a collaboration. Honestly we were learning things together. There were some great people, meetings that were exciting, excellent seminars and so on. So yes, it was a collaborative effort but not with the purpose of writing papers. It was a process of learning. We never thought of it as a collaboration.

Around the mid-50s, early 60s, by that time the topology of fibre bundles was quite well understood. There was already the classic book by Steenrod on this topic. At the same time, there was the work of Elie Cartan on differential geometry of fibre bundles, with modern expositions which were easier to understand. It was apparently influenced by general relativity. So from the point of differential geometry and topology, fibre bundles were well understood. This was a mathematical background to our work. There were the H.Cartan seminars as well. There were also seminars on sheaf theory, which were just being developed that time.

So we used to have seminars on these different kinds of topics. All these things were very exciting for people like me. KC and KGR encouraged these activities.

We were all in the end Bourbaki enthusiasts. Not sure about Raghunathan, though. *[Laughs]* Anyway, we never bothered about the controversies regarding Bourbaki; for people like us in India, who were away from big mathematical centres, these books by Bourbaki told us how mathematics was done at a higher level and how first rate mathematicians were thinking. Many of us followed Bourbaki approach to mathematics as it enabled us to acquire a unified view of a large area of mathematics. And I think this was very important.

In fact, we used to wait for these books by Bourbaki in the same manner that children do these days for Harry Potter. And I am not exaggerating – it was the same level of enthusiasm.

At that time complex analytic fibre bundles were coming in some way. For example, Grothendieck classified vector bundles on the projective line and Seshadri independently did it in his thesis.

Andre Weil also wrote some papers on vector bundles around this time. But surprisingly an earlier 1938 paper of his on *"generalization of abelian functions"* was not so well known, except for a reference in a paper of Atiyah. In fact, it seems that even Mumford did not know this work. Luckily for us, KGR told us about this paper. KGR had heard about this work from Siegel. Without any particular reason, we had a seminar on this paper and Seshadri gave some lectures. So already the topic was on our minds. And we really started working after the Kodaira-Spencer theory came. We soon realised that the essential problem was to find an algebraic characterisation of holomorphic bundles on a compact Riemann surface which arise from unitary representations of the fundamental group of the surface. We even felt that if only one could guess what the characterisation was, then it can be proved by the so-called continuity method, envisaged by Klein and Poincare to prove the uniformisation theorem for compact Riemann surfaces. (They did not quite succeed in making this method work and the uniformisation theorem was proved by some other method)

MS Ragunathan: I wanted to make a few comments. First about Jawaharlal Nehru – there is a passage in the Discovery of India, just two pages long, about Indian mathematics. And if you wanted to write about Indian mathematics in two pages, you can't do better than that. It's done remarkably well.

MSN: He sent some messages about mathematics to Tata Institute. In one of the messages he writes, *'Mathematics is the vehicle today of exact scientific thought. Mathematical research has widened the horizon of the human mind tremendously and has helped to the understanding, to some extent, of nature and the physical world.'* He thought about these things.

To continue with what I was saying earlier, it was very clear to us that the whole problem was to find some algebraic characterisation of *"unitary bundles"*. Then Mumford, in his ICM 1962 lecture, defined stable vector bundles (a purely algebraic notion) and announced that he could construct the moduli space of stable bundles as a quasi projective variety. It turned out that the notion of a stable bundle was the algebraic concept we were looking for. Using the continuity method, Seshadri and I proved that stable bundles of degree zero on a compact Riemann surface are precisely those vector bundles which arise from irreducible unitary representation of the fundamental group. There was also a generalisation to stable bundles not necessarily of degree zero.

(From left) Rajesh Gopakumar, Carlos Simpson, M. S. Raghunathan

It helped that we were familiar with many fields.

RG: So do I understand correctly that this paper by Weil on abelian theta function at first sight had nothing to do with vector bundles?

MSN: It had. Although it doesn't mention the word vector bundle at all. I think soon people realized that Weil was really studying holomorphic vector bundles on compact Riemann surfaces.

MSR: Chandrasekharan joined Tata Institute in 1949, just two years after Independence. He then started the graduate school and built it. The other comment I want to make is that Narasimhan said that he learnt set topology in four lectures and also remarked whether I am a Bourbaki follower or not – I am very much a Bourbaki follower and I learnt all my set topology only from Bourbaki. *[Laughs]*

MSN: Including filters?

MSR: Yes, filters also. Narasimhan said he learnt set topology in four lectures, I learnt deformation theory of complex structures a la Kodaira-Spencer from him in walks at the seashore in two days.

PP Divakaran: Many informal seminars were given on walks by the seashore, either from the Old Yacht Club to Radio Club or up to Mahim if the problems were big, all the way to Malabar Hills and back. I have myself benefited enormously from these. I learnt many things from those walks.

CSS: Coming back to Andre Weil – his definition of vector bundles is in, what is called adelic language. Chevalley introduced the adelic language and Andre Weil used it. He also realized through it the modern theory of vector bundles. He had a certain wonderful way of understanding what things mean. He had a brilliant intuitive computation of the dimension of the moduli space – intuitive in the sense that there was no real proof. Then he asked *"What would be the role of theta functions on this moduli space?"* That eventually led to the Verlinde formula, etc. One can say that he had this astounding overall view.

MSN: I wanted to make a remark on this paper of Weil. He says that bundles coming from unitary representations should play an important role. No one really had an idea what that meant exactly. Our theorem justified this remark.

MSN: Then Seshadri first constructed compact moduli spaces of (semi-stable) bundles. Many of us felt that a whole new field, namely the theory of algebraic vector bundles, was opening up. So a group of us – Ramanan, me, Seshadri, A.Ramanathan and others – started studying these moduli spaces in depth. And that's why it became a theory, otherwise it would have stayed as one nice theorem.

RG: You had already felt that there was something important?

MSN: Yes, we did feel we were doing something very

significant. I remember telling KC, *'I think we have done something important.' [Laughs]*

RG: So you could see it could become a theory.

MSN: We could see there was a big possibility of a theory. Then many mathematicians from outside TIFR started working in this field and developed the subject.

Incidentally, Raghunathan's thesis was indirectly coming from our work. And Ramanan's thesis on homogeneous vector bundles too is related to our work.

MSR: Seshadri's construction of Picard variety was also an influence, right?

MSN: There was no direct relationship. But Seshadri's insight and background in algebraic geometry helped.

MSR: There is another unexpected happening, a nice coincidence. Narasimhan suggested a thesis problem of deformation of connections. And I ended up showing that it's the same as deformations of representations of discrete groups. At the same time, they were working on the representations of the finite groups on Riemann surfaces. It was a happy coincidence.

SW: So what was it like to be a student of Narasimhan?

MSR: The first time I saw Narasimhan, he was wearing a suit. He had just come back from Paris. That already scared me. Because at that time the formal dress was a bit intimidating. But after a few conversations, I was completely comfortable with him. He made me feel at ease. At that time Borel was lecturing and apparently he made a mathematical comment that people couldn't figure out. We were discussing this outside. I came up with a solution to the problem, and Narasimhan came and congratulated me on that. That, of course, made me feel great.

PPD: These Paris return people were quite intimidating, I can tell you that.

CSS: What did Divakaran say?

MSN: Divakaran says that the people who returned from Paris, were both intimidating. *[Laughter]*

MSR: To digress a bit – here's one more story about Chandrasekharan. Apparently Bhabha had gone to the US to recruit people for the Tata Institute. He went to Princeton where he met Chandrasekharan and offered a Readership at TIFR to him and Chandrasekharan accepted. Then one day Bhabha, Yukawa and Einstein were taking a walk, and behind them Von Neumann and Chandrasekharan were also taking a walk. Apparently Von Neumann asked Chadrasekharan, *'So, I heard this man walking there has offered you a job; are you taking it up?'* Chandrasekharan replied that, *'Yes I think I am taking it up.'* Then Von Neumann told him, *'He is as good a physicist as any, don't let him intimidate you, stand up to him.'* [Laughter]

MSN: There was another story. Raghunathan along with Raghavan Narasimhan went to a tailor's shop in Princeton. The tailor asked them, *'Do you know von Neumann and Chandrasekharan?'* He said, *'Yes.'* And the tailor said they were the only two people who knew how to dress.

SW: He also always dressed in a suit.

MSR: You haven't seen Chandrasekharan. He always dressed immaculately - there's no question about it.

MSN: In any case, KC built up the school of mathematics in the Tata Institute in 15 years and changed the landscape of mathematics in India. Just remember that he was there only for 15 years.

CSS: He left in 1965.

RG: By then there was a large number of people?

MSN: I would say around 20ish. But very good people.

MSR: It was close to 30.

SW: Why did he leave?

MSN: I don't know. There are theories. There were some disagreements with Bhabha. The second thing was probably some personal reason. And sometimes he used to say things like, *'You guys can now take care of it.'* I mean not in as many words. I suppose that he meant that the school of mathematics can carry on without his guidance

MSR: As long as he was there, he ran the place. And ran it like an autocrat, no question about that. He did the right things, no doubt about that. But his views were always final. Of course, he used to listen to good advice as well. He didn't socialize much, only with very senior people in the institute. Bhabha and a few others and maybe Narasimhan and Seshadri. That's it.

RG: But TIFR mathematics did not grow in the directions that he might have wanted?

MSN: Yes, in fact that is the greatness.

MSR: Yes, he didn't push anybody towards any field, including his.

MSN: At that time France was the centre of mathematics. One of the US mathematicians said, *'France is our teacher.'* So Chandrasekharan somehow saw that that is where the future was. He made contact with French mathematicians and induced them to come to TIFR. Laurent Schwartz came and lectured for two months. Some other eminent French mathematicians also came and lectured. And that was the direction he set. The only person who worked in his field was Raghavan Narasimhan, (who also worked on several complex variables). There was complete freedom. I wish to say another thing – he had great humility as far as mathematics was concerned. He never thought he was a great mathematician. He really appreciated good mathematics.

CSS: I think Chandrasekharan's relationship with France began with Laurent Schwartz's visit to the institute. Schwartz actually told people that there was a very good place here and he himself visited Tata Institute many times. I was working on some number theory, some periodic functions, giving seminars on topological vectors, all sorts of things. And then Laurent Schwartz was so wonderful, that I thought this is what I want to do. Coming back to the point, I don't think it was decided from the initial days that Chandrasekharan will send the students or have such a close relationship with France. Probably, it was the result of the visit of Laurent Schwartz. It was him who convinced people to come to the Tata Institute and then Narasimhan and I went to France for three years.

MSR: In some unpublished reminiscences, Schwarz has referred to KC as a *'great mathematician'*.

MSN: The connection with France at this time was very important. There was a constant flow of French mathematicians to the institute. They gave high level lectures on topics of current interest. The notes were taken down by the young students. The students would then also come in direct contact with these French mathematicians. And it's amazing how just after one year after some of these lecturers very good work would be done by young mathematicians at TIFR. My work with Ramanan was because he had attended Koszul's lectures on differential geometry. Raghavan Narasimhan's famous work on several complex variables followed after a course given by Malgrange on this topic. And similarly from Germany there were many lecturers. Soon after a course of lectures by Siegel came Ramachandra's work. So you see how immediately this kind of contact worked.

The role of the French contacts was immense. And as Seshadri said Schwartz played an important role. He is supposed to have told his French colleagues: *'The students there are the salt of the earth, we should help them.'*

So some of us went to Paris and many famous French mathematicians visited TIFR. And I can give you at least five instances where there has been a correlation between some very good work done at TIFR and an interaction with these great mathematicians.

RG: So coming to the mid-60s and you mentioned a lot of students getting into this area, can you talk about how the subject and the different directions the school of mathematics evolved in?

MSN: There was a very good work being done on number theory, among others by KC, Raghavan Narasimhan and KGR. This was perhaps a direct influence of Siegel. So that was one direction. The othermajor field cultivated in TIFR was algebraic geometry. In this area, apart from the work of Seshadri, Ramanan, Ramanathan and myself on moduli,there were basic contributions by C.P. Ramanujam.

Seshadri was working also on another interesting direction – standard monomials, which is a very exclusive way of doing representation theory. Seshadri was very much interested in that. Why? You can ask him.

There was also a strong school of algebraists.

Now Raghunathan can talk about the development of some other fields in TIFR.

MSR: My thesis in a sense came out of the theory of deformations of complex structures which Narasimhan had taught me and he had proposed that I do something similar for connections. So my thesis was on *'Deformations of connections'* and it connected them with Deformations of Representations of discrete subgroups of Lie groups. I joined TIFR in August 1960. There had been in January that year an International Colloquium at TIFR. Where Atle Selberg had announced some very exciting results about Deformations of discrete groups of SL(n,R). That and a paper by Andre Weil on discrete subgroups of Lie groups had close connections with my thesis and led me to the theory of Discrete Subgroups of Lie groups which has been the mainstay of much of my research. I worked on the problem of arithmeticity of lattices and made considerable progress but was pipped to the post by the Fields Medalist G.A. Margulis. I had a series of students working on different aspects of the theory and we had by the seventies a fairly strong group of researchers at TIFR. But the group around Seshadri and Narasimhan was a lot stronger – Ramanan, RamanathanVikram Mehta.

MSN: I wanted to mention again one more name here – CP Ramanujam.

CSS: Yes, I was also thinking of that.

MSN: He was one of the most brilliant mathematicians I have known. Unfortunately, due to some problems, he committed suicide. But he was one of the few people, except perhaps Seshadri, who were completely at home with the work of Grothendieck in algebraic geometry. He has two famous works. One is his vanishing theorem which he proved for surfaces. Inspired and motivated by this result, it was generalised by Kawamata and Viehweg and plays a crucial role in the so-called minimal model programme in algebraic geometry. This theorem was known as the Ramanujam –Kawamata-Viehweg vanishing theorem, but in recent literature Ramanujam's name is missing!

CSS: There is a beautiful characterization of $\mathbb{C}2$.

MSN: That a smooth affine complex surface X, which is contractible and simply connected at infinity, is isomorphic to the complex plane $\mathbb{C}2$.

MSR: He also gave an example to show that the condition at infinity is important. Later, some people at Tata Institute went into problems closely related to this work of Ramanujam's. Gurjar was one person. Ramanujam proved two important theorems in Number Theory – one was about cubic forms over number fields which was proved simultaneously by Birch and Ramanujam. They proved that any cubic form with 54 variables has a non-trivial zero. He also made a contribution to the Waring problem. The Waring problem is the following question: given an integer K is there a positive integer $g(K)$ such that every integer is the sum of $g(K)$ of Kth powers of integers. This was solved by Hilbert. Then Siegel generalised it to number fields, but the formulation becomes a bit technical. Siegel also made a conjecture connected with this and Ramanujam proved it to be true. His elegant proof appeared in a short (14-page paper). This was his thesis – it was probably the shortest thesis in the history of the Tata Institute.

MSN: By the way, there was a problem on the fundamental group of a projective variety. What was it, Raghunathan?

MSR: Serre had asked the question whether the fundamental group of a projective variety is residually finite.

MSN: An important input is what Raghunathan did. Perhaps he didn't realise the connection. By the way, he never told me about this, I heard about it from someone else.

MSR: I did some work on torsion in co-compact lattices in Lie Groups which turned out to be a key ingredient in the construction of a projective variety with a fundamental group that is not residually finite. The construction was done by Domingo Toledo. Yes, I was not aware of this question of Serre's when Toledo announced his result in 1990 at the Tokyo Congress. In fact, as soon as we heard about it at TIFR, Madhav Nori came up with a construction (also using my result) which turned out to be lot more elegant than Toledo's.

PPD: You still give lectures on the subject year after year?

MSR: No.

SW: Carlos, do you have a perspective on how this theorem went forward?

Carlos Simpson: Yes, I was just curious about a historical question, which is Mumford came to visit Tata Institute, right? When did he come? Was it after your work?

MSN: When did Mumford come, Seshadri?

PPD: He came in 1970.

MSN: Our paper was in 1964-65.

CS: So one of the reasons he came to visit your institute was your paper, right.

MSN: Yes, I think so.

PPD: You know I think he came probably earlier for a short visit. But the year that he spent at the Tata Institute was 1970-71. Bhaskara was already built and we were all living there. He stayed there, his kids went to school in Bombay.

MSN: He said that his kids learnt how to spell English words properly when they went to school in Bombay.

RG: Carlos, we also wanted to talk about how the subject grew and its ramifications elsewhere. You said that for a while it was dormant?

CS: I don't know ,dormant to what extent…There was Mehta-Seshadri for example, dealing with the case of open curves. The Gieseker-Maruyama construction of the moduli space of bundles on a higher dimensional space was motivated by it.

I think the aspects of the relationship between analysis and algebraic geometry was maybe less explored. So when I got to graduate school, everybody was all excited about gauge theory, because of Donaldson and Donaldson invariants. One of the first papers [during that time] was Donaldson's new proof of the theorem of Narasimhan and Seshadri.

RG: So this new incarnation of your theorem through gauge theory – I believe that was the time when you started working with Ramadas.

MSN: This was because of the work I did with Ramanan on the existence of universal connections. I was aware in a way that gauge theory was theory of connections on principal bundles. The paper with Ramadas, was an attempt to understand this relationship better and to understand mathematically questions like gauge fixing, Coulomb gauge and the like.

Rukmini Dey: When did you get interested in conformal field theory?

MSN: I found that physicists were using my work with Ramanan on universal connections and my work with Seshadri. I was curious and tried to read some relevant papers. It was difficult for me to understand the language and physical motivation and relate to their work. But luckily Divakaran and I were friends. We used to play bridge together. We used to often discuss mathematics and physics, for example what is gauge theory from a physicist's point of view. For the first few months, communication was difficult but after sometime we could understand each other. (Incidentally Divakaran asked me to give some lectures on gauge theory and Ramadas was his student. Ramadas wrote up my lectures). Coming to the question regarding my interest in conformal field theory: Physicists working in a theory called Chern-Simons theory associated certain finite dimensional vector spaces to a compact Riemann surface. I could see that these spaces should be the space of sections of holomorphic line bundles on moduli spaces of vector bundles on the surface. But there were some serious technical problems, for instance due to the presence of singularities in the moduli spaces. So I started working with a young French mathematician, Drezet, and we made a thorough study of line bundles on these moduli spaces and one could define rigorously the vector spaces defined in this theory as sections of these line bundles. These sections are now called generalised theta functions.

On the other hand, the physicists, working in conformal field theory, defined something called conformal blocks, using Kac-Moody algebras. Generalised theta functions and conformal blocks were supposed to be the same. But there was no proof.

I sort of started learning these topics. In this context I wanted to find out whether there was a good version of the Borel-Weil theorem for Kac-Moody algebras. There were some versions and I was not happy with them. So one day I asked Shrawan Kumar, in a tea table conversation at TIFR, whether he knew a good version of Borel-Weil theorem for Kac-Moody. He said yes, pointed to himself, and said, *"I have proved it".* So that's how it started, sitting around at the tea table. Then, in collaboration with A. Ramanathan, we proved that generalized theta functions and conformal blocks are the same. It turned out that independently and at the same time two other set of people proved this result.

By the way, the referee's report on this paper of ours said, *"This paper provides a rigorous proof of a result that is very important for mathematics and mathematical physics. The physicists apparently consider this result as obvious."* So much for interaction between mathematicians and physicists!

(From left) M. S. Narasimhan, Pranav Pandit

Pranav Pandit: Were there joint seminars between physics and math at the Tata Institute?

PPD: You know there were two or three of us, you know the mathematicians are very difficult to move.

MSN: I think Spenta thanks me for some conversations.

PPD: No earlier, there was Ashok Raina and I - we had very close contact with especially Narasimhan. I had very special contact with Raghunathan and even with Seshadri. In fact, there is one paper of mine in which I thanked Narasimhan and Seshadri for entirely different kinds of help. I will tell you what they are – I was working on the question of how to construct a theory of gauge invariant things from gauge theory. The idea was that hadrons are described by these invariant things, so it should be a good theory. So I was talking about this on one of those walks along the seaside with Seshadri. And I asked Seshadri if he could help me with this. He told me to go read Procesi's paper Fundamental Theorems for Invariants in One Matrices. It was actually the key. The idea is that in the end you get a manifold, the invariants form a manifold, and it is functions from spacetime into that manifold. Then I said how do I understand this? What you get is the exact non-linear sigma model of hadrons. So I was in Japan after that. I wrote a letter to Narasimhan from there telling him that I was stuck with these problems. He told me that what you need is the homotopy exact sequence for this. He did it and sent it to me. And that is an appendix to our paper. Of course, Raghunathan has been incredibly helpful in everything I have done.

MSN: He has written an appendix to a paper of yours.

PPD: He has written the appendix to one of my papers, which is probably the only readable account of universal central extensions in the entire mathematics and physics literature.

MSN: It's also very popular among mathematicians.

PPD: Hyman Bass was visiting TIFR.Someone told me to talk to him, he might help. But Narasimhan said I should talk to Raghunathan. So I went and talked to him. It is the key result of the general characterization of universal central extensions and how they lead to projective representations. It is for me the key to understanding quantum theory.

MSN: Another thing that I told him was about Borel Density Theorem.

PPD: Yes, yes! Then I got him interested in discrete spacetimes. And of course at the West Canteen coffee table I asked, 'Do you know how does one deal with the discreteness of spacetime from the point of view of the invariance group -- Lorentz group and the Poincare group. He immediately said go and talk to Raghunathan. Then he took me to the Borel Density Theorem, which says something about how irreducible representations of certain noncompact groups behave. And that's a preprint – it's actually not really dead. I am still working on it.

SW: About this mathematics-physics interaction I gave a course on quantum mechanics for mathematicians.

PPD: Yes, I remember.

MSR: I just wanted to make one comment. There are two papers by Narasimhan that have made big impacts in two completely different fields. One is on differential equations – he has a joint paper with a Japanese mathematician Kotake. The second is on representation theory – again with a Japanese mathematician named Okamoto, which is about the realization of discrete fields. Both these areas he just went into it, proved some outstanding theorems, comes out and then does something else.

PPD: That's the discrete series?

MSN: Yes.

PPD: That's the one in which there is a story about Harish-Chandra?

MSN: *[Laughs]* There is a story but not in the paper! I was in Princeton and collaborating with K.Okamoto. Okamoto used to talk to Harish-Chandra during this period (An unpublished work of Harish-Chandra, which he communicated to us, was very useful in our work). Now Harish-Chandra somehow did not seem to like some modern notions like vector bundles. As I said Okamoto used to have discussions with Harish-Chandra. So I asked Okamoto how he managed to do it. He said that when he talked to Harish-Chandra he talked about induced representations and when he talked to me he talked about sections of vector bundles.

PPD: I have written on the very sleek introduction on the representations of Poincare group – I say use representations, then I add in brackets you can think of it as sections of vector bundles.

RG: Prof. Seshadri, I wanted to ask about your thoughts on this physics-mathematics interface that has grown – some of it from your work, some in other ways.

CS: My contribution or contact with physics is nil… I will say. But I had contact with representation theory, where my interest lay in the theory of standard monomials. Standard monomials means that you construct canonical bases of finite-dimensional representations. That is the subject. Therefore I have been involved in that. You can call it a generalization of classical invariant theory. That's probably how I suggested Procesi's paper to Divakaran. Standard monomials have many combinatorial applications. That was somewhat different in flavour from the algebraic geometry that I was doing.

Another thing in algebraic geometry that I was very much involved in was the problem of constructing quotient spaces, on which Simpson has also worked… I did, for example some general cases of computations of quotients … Of course, Simpson came up with a wonderful theorem that absorbs everything. Part of that slightly more complicated proof is in my first early work where the construction of the moduli space follows a certain pattern. That is to say, one proves the existence of some kind of Quot scheme. In classical language, this is called the universal family. Each object in the moduli problem can be represented by many points in the parameter space or versal family. From this parameter space, one has to go down to a minimal model by identifying equivalent points, often by passing to a quotient by a group action. One of the principles of moduli theory from the algebraic geometry point of view is to reduce any moduli problem to a problem of quotient spaces by algebraic group actions. That's where Mumford's theory comes in.

We were also fortunate that my first paper with Narasimhan was on unitary connections on vector bundles. Then what is the moduli space that has to be constructed? This came soon after, thanks to Mumford. And the importance of Mumford's work and from the historical point of view is the definition of stability. It is a very deep idea of Mumford's. Mumford sent me a paper on this which Narasimhan and I used in our paper on unitary connections.

In any case, what I wanted to say is that I had been considerably interested in constructing quotients. Now, of course, quotients are to be understood in a more general sense… Quotients also throw up also new objects. Andre Weil wanted to construct Jacobians in characteristic p. He could not construct it as a projective variety. Therefore, then, this led to the definition of an abstract algebraic variety. The moduli problem also suggests generalized objects of algebraic geometry. Therefore, quotient spaces throw up objects which are not part of "classical algebraic geometry", but are generalizations of objects thereof, and which are used in moduli problems. One of the things that arose from all this is Mumford's conjecture. I was very interested in this conjecture. I made some progress but it was solved by Haboush. And recently I found a proof which is longer than the short proof of Haboush, but done in a completely different way. I do not know whether physicists would be interested in this. Probably they would be interested in quotient spaces, because they define algebraic objects which are not defined by algebraic equations, but are defined in some functorial or categorical way. This has played a great role now, and unfortunately I have not been following the latest things.

Anyway, what I wanted to say is that I had been quite interested in constructing quotients. Quotients throw up objects which are not part of the classical theories of algebraic geometry.

CS: You have a paper in Advances in Math, right?

CSS: I was talking about the paper in the Ramanujan Mathematical Society which I wrote very recently. There I have a proof of Mumford's conjecture without representation theory, following my old approach in my work on quotient spaces for actions of reductive algebraic groups, where I proved Mumford's conjecture for stable points. I believe this might be of some interest to others.

CS: One of the courses scheduled for tomorrow and Friday by Daniel Halpern-Leistner is about that type of question.

(From left) P. P. Divakaran, Spenta Wadia, Rukmini Dey

CSS: Yes, I was interested in listening to him.

MSN: Let me make two comments on what Seshadri said. The Mumford conjecture – Seshadri could only partially solve it long ago. Then it was solved by Haboush. And I am sure that Seshadri was bothered all these years, 'C'mon, why am I not able to solve this!' [Laughter] No, I am serious. This shows his tenacity and also his way of thinking. Then after almost 30-40 years he does something and he finished it. I am sure he is very happy.

And the second thing I wanted to say is about the impact of our paper which I am not sure we realise. See, Mumford constructed only the space of stable bundles. You have to compactify this space. Our theorem suggests a natural compactification, namely the space of all unitary bundles. Seshadri introduced a structure of a projective variety on this space by introducing a equivalence relation, now called S-equivalence (Seshadri equivalence) on (semi-stable) vector bundles. This method of constructing compact moduli spaces using S-equivalence relation on sheaves is now the standard way of constructing projective moduli spaces of (semi-stable) coherent sheaves on higher dimensional varieties.

MSR: Also about Mumford conjecture Seshadri says he did something – he made the first dent in the problem. Afterwards it was solved by Haboush. But that first dent was crucial. Similarly, he made the first dent in another famous problem: Serre's conjecture about projective models. The year I joined it was big news at the Tata Institute – everyone was excited about it. It was known to be true for one variable for a long time, Seshadri gave the solution for two variables. Then it stayed like that for another 20 years before Quillen proved the full theorem.

MSN: Some people called it the Serre conjecture. He was not happy originally for it to be called a conjecture. Then Seshadri proved it for two variables, then somebody proved it for three. Then he didn't mind it being called Sere conjecture.

MSR: It also generated a lot of interest in the Tata Institute. Lots of people, including me, worked on it.

CSS: Let me also tell you that the three variable case was proved by Murthy, one of the Tata Institute people, and another person. That made the real difference. Up to that time it wasn't clear whether it was true for greater than two variables. Their paper used Abhyankar's work to show that over a polynomial ring in three variables, any projective module is free. And within a year Quillen and Suslin [independently] proved the general case. So there was this important contribution of the Tata Institute, but unfortunately, people don't refer to it at all. As far as mathematics references go, you give typically credit to the final work on a major problem, and probably sometimes the first one and final one, but if something happens in between that work is often not cited.

PP: What about mathematics today?

MSN: That's a whole big question for another day.

RD: Did you ever meet Grothendieck?

MSN: Yes. When I first met him I was not doing algebraic geometry. I was a student of Laurent Schwartz, and I had started reading Kodaira-Spencer. At that time someone told me that Grothendieck had new way of looking at the Kodaira-Spencer theory of deformations. I asked Grothendieck if he could explain to me his approach and he explained it to me in three hours.

PPD: Where was this, Narasimhan?

MSN: This was in Paris. But he was amazing. Here he was talking someone whom he didn't know and he was seriously talking for three hours explaining everything in detail. That was the first time I met him.

PP: Did he come to TIFR?

MSN: Yes, he did. He announced his famous "Standard Conjectures" in an International Colloquium at TIFR. Many famous results were published in the proceedings of international colloquia held in TIFR.

MSR: Probably the only written account on the Selberg Trace Formula by Selberg himself is his announcement in the Tata Institute Colloquium 1956 Proceedings.

MSN: There are two famous results by Selberg, the Selberg trace formula and his result on rigidity of discrete groups. Both are in Tata Institute publications.

MSR: Selberg published very little. Another famous result is on moduli of Hodge structures by Griffiths.

MSN: By the way, attending these famous lectures in these international colloquia formed a part of our mathematical culture.

MSR: There is absolutely no question about it. Learning and talking about problems happen simultaneously.

CSS: By the way, let me also say that the Satake compactification -- Satake came to the Tata Institute Colloquium -- was also at the back of my mind while working on in this compactification of the moduli of vector bundles. Usually in the Satake compactification you will have something first compactified topologically and then Bailey came and later added complex structures. But in Mumford's theory what was fascinating for any mathematician was that at one stroke you get everything.

MSN: Seshadri, you are giving it a bit too much credit. See, you gave a very good example – Satake had only topological structure and afterwards Bailey brought in the complex structure. It's exactly the same, there was the compact topological space of all unitary representations and you endowed this space with an algebraic structure.

CSS: No, no I was mentioning that in the moduli of vector bundles, you do not have to go through the procedure to construct an object, where first you pass to the completion and then prove it is projective ;everything comes immediately. Now it is familiar because everybody is used to it but Satake's compactification and Mumford's theory of compactification are very similar.

CS: This is crucial for the Hitchin fibration, to have a proper fibration, because the moduli space is not compact. □

PROGRAMS

Due to the unprecedented COVID-19 situation, a few programs had to be cancelled. However, we were also able to organize several programs online.

Fluctuations in Nonequilibrium Systems: Theory and Applications
9—19 March 2020 ✦ *Organisers* – Urna Basu and Anupam Kundu

Gravitational Wave Astrophysics (Online)
18—22 May 2020 ✦ *Organisers* – Parameswaran Ajith, K. G. Arun, Sukanta Bose, Bala R. Iyer, Resmi Lekshmi and B Sathyaprakash

Bangalore School on Statistical Physics – XI (Online)
29 June—10 July 2020 ✦ *Organisers* – Abhishek Dhar and Sanjib Sabhapandit

Recent Developments in S-Matrix Theory (Online)
20—31 July 2020 ✦ *Organisers* – Alok Laddha, Song He and Yu-tin Huang

Zariski-Dense Subgroups and Number-Theoretic Techniques in Lie Groups and Geometry (Online)
30 July 2020 ✦ *Organisers* – Gopal Prasad, Andrei Rapinchuk, B. Sury and Aleksy Tralle

DISCUSSION MEETINGS

Discussion Meeting on Zero Mean Curvature Surfaces (Online)
7—15 July 2020 ✦ *Organisers* — C.S. Aravinda and Rukmini Dey

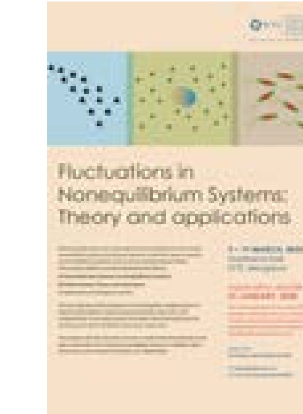
LECTURES

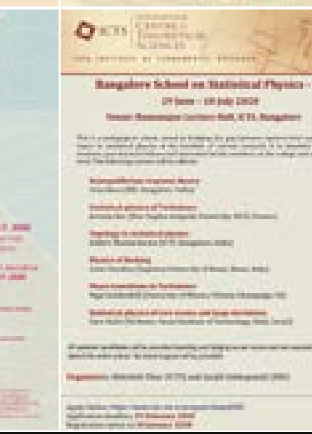
KAAPI WITH KURIOSITY

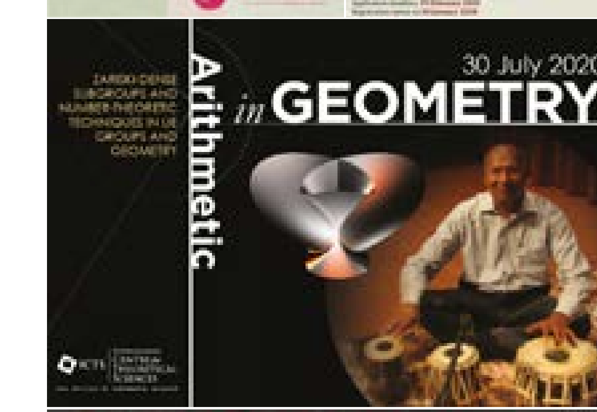
There have been two *Kaapi with Kuriosity* lectures (temporarily renamed *Kuriosity during Kuarantine*), livestreamed through the ICTS YouTube channel.

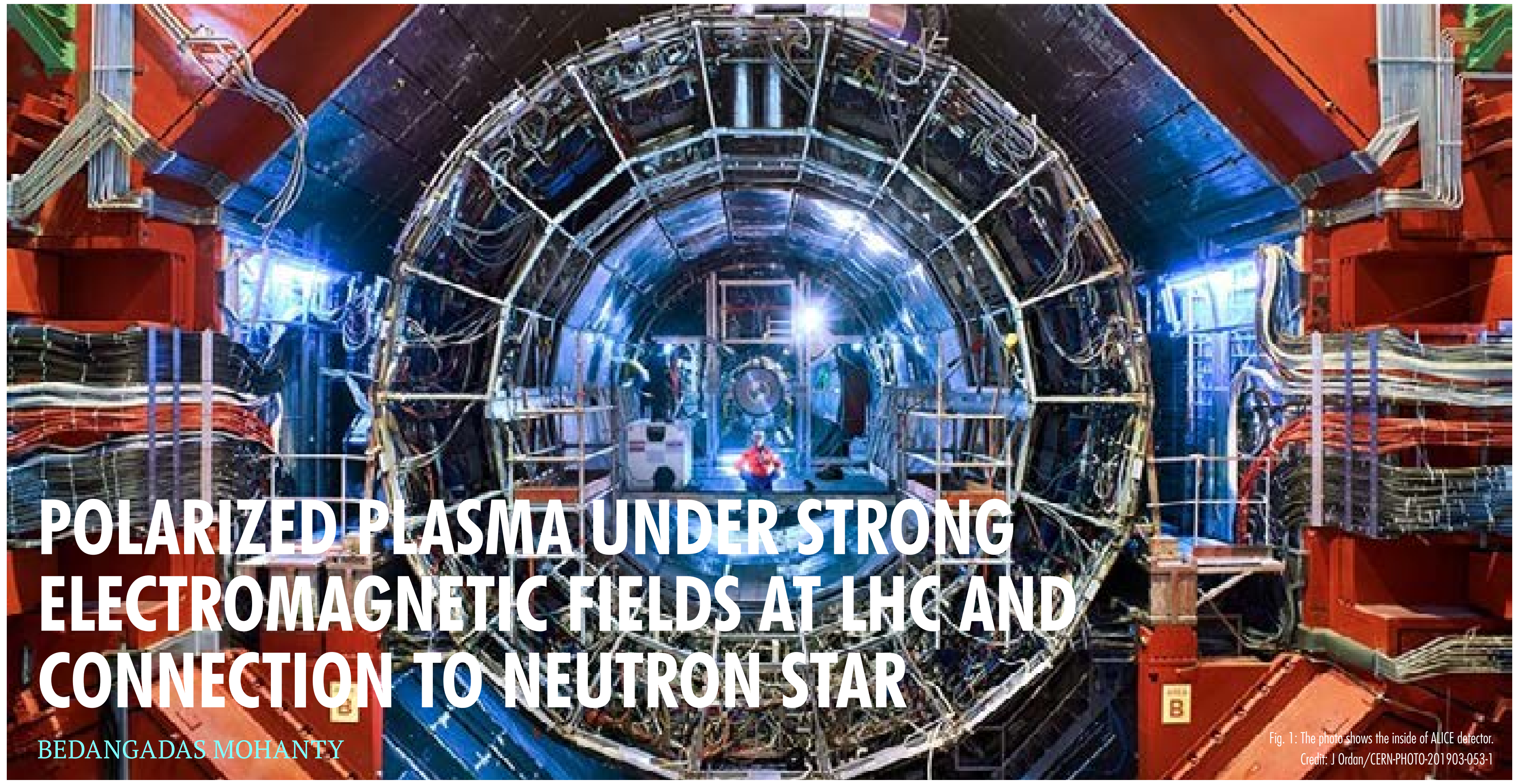
Soft and Squishy Materials and How to Think About Them
19 July 2020 ✦ *Speaker* — Gautam Menon *(Ashoka University and IMSc)*

Automating Mathematics?
17 May 2020 ✦ *Speaker* — Siddhartha Gadgil *(Indian Institute of Science, Bangalore)*









POLARIZED PLASMA UNDER STRONG ELECTROMAGNETIC FIELDS AT LHC AND CONNECTION TO NEUTRON STAR

BEDANGADAS MOHANTY

Fig. 1: The photo shows the inside of ALICE detector.
Credit: J Ordan/CERN-PHOTO-201903-053-1

ALICE (A Large Ion Collider Experiment) is a heavy-ion detector on the Large Hadron Collider (LHC) facility at the Conseil Européen pour la Recherche Nucléaire (CERN). The 10,000-tonne ALICE detector is 26m long, 16m high, and 16m wide.

The detector sits in a vast cavern 56m below ground in France. The collaboration consists of more than 1800 scientists from over 175 physics institutes in 41 countries. It is designed to study the physics of strongly interacting matter at extreme energy densities, where a phase of matter called *quark-gluon plasma* (QGP) forms [1]. Heavy-ion collisions in the LHC generate temperatures (T) of the order of 10^{12} – 10^{15} degrees Kelvin. Under these extreme conditions, protons and neutrons melt, freeing the quarks from their bonds with the gluons leading to QGP formation. Thus, recreating in the laboratory conditions similar to those during a few microseconds old Universe. Studying such a phase and its properties tests the theory of quantum chromodynamics (QCD), provides understanding of the phenomenon of confinement, and the physics of chiral-symmetry. While the confinement tells why we observe only colourless states in the particle spectrum in nature, the chiral symmetry on the other hand describes the presence of particles like pions and mass generation of nucleons, which then accounts for most of the mass of all the visible matter. The experiment in conjunction with the theory has measured several properties of the QGP medium like the shear viscosity to entropy density $(\eta/s) \sim (1\text{-}2)1/4\pi$, stopping power also called as opacity of the medium $\sim 2 - 10\ \mathrm{GeV^2/fm}$ and diffusion co-efficient times $2\pi T \sim 1\text{–}10$.

Out of the several properties of QGP listed above, the value of $\eta/s \sim 1/4\pi$ led to the discovery of the *emergent property* of QCD matter, namely the perfect fluidity [2]. Maxwell had realized that for a dilute gas, the shear viscosity is related to momentum transport by individual molecules and is given as $\eta = (1/3)$ npl. Here n is the density, p is the average momentum of the molecules, and l is the mean free path. As mean free path varies inversely with density, η to a good approximation is independent of density (one interesting consequence is that the damping of a pendulum caused by the surrounding air is independent of atmospheric pressure!). For a fixed density and temperature, as η is proportional to mean free path (l), it will become smaller with stronger interactions among the constituents of the fluids. So, is there a limit to how small a value η can take up? The shear viscosity is a measure of the ability of a fluid to transport momentum from one point to another, and Heisenberg's uncertainty in quantum mechanics puts limits the accuracy with which momentum and position can be simultaneously determined. This leads to $\mathrm{pl} \gtrsim \hbar$. Considering entropy density $(s) \sim k_B n$, the lower bound to $\eta/s \gtrsim \hbar/k_B$. However, this simple estimate is not the complete physics story, it turns out that a precise value on the bound on the viscosity can come from string theoretical calculations using the anti-de Sitter/Conformal Field Theory conjecture. It was shown by Kovtun, Son, and Starinets (also called KSS bound) $\eta/s \geq \hbar/(4\pi k_B)$ (modulo some corrections). The QGP fluid created at LHC is found to have a η/s value close to $1/4\pi$ (KSS bound in natural units, $\hbar = k_B = 1$), hence it is termed as a *perfect fluid.*

Recently, it was realized that the perfect fluid of QGP formed in ultra-relativistic heavy-ion collisions is subjected to two interesting initial conditions: (a) *Angular momentum* and (b) *Magnetic field.* Angular momentum of the order of $10^7\hbar$ is theorized to be imparted to the system through the torque generated when two nuclei collide at non-zero impact parameter with center of mass energies per nucleon of few TeV. This leads to a thermal vorticity of the order of 10^{21} per second for the QCD matter formed in the collisions. Further, when the two nuclei collide in the LHC, an extremely strong magnetic field of the order of 10^{15} Tesla is generated by the spectator protons, which pass by the collision zone without breaking apart in inelastic collisions. The effect of the angular momentum, a conserved quantity, is expected to be felt throughout the evolution of the system, the magnetic field on the other hand is transient in nature and stays for a few fm/c in time scale. Just to give an idea of the magnitude of these values, the highest angular momentum measured for nuclei ~ $70\ \hbar$ and the strongest magnetic field we have managed to produce in the laboratory is ~ 10^3 Tesla. Are there experimental signatures of these phenomenally large initial conditions? The complexity enters due to the femtoscopic nature of the system formed in the heavy-ion collisions, both in space and time scales. Nevertheless, the experiment has been able to address this challenging problem.

Establishing the large initial angular momentum
It is known that the spin-orbit coupling causes fine structure in atomic physics and shell structure in nuclear physics, and, is a key ingredient in the field of spintronics in materials sciences. It is also expected to affect the development of the rotating QGP created in collisions of lead nuclei at LHC energies. The extreme angular momenta are expected to lead to spin-orbit interactions that polarise the quarks in the plasma along the direction of the angular momentum of the

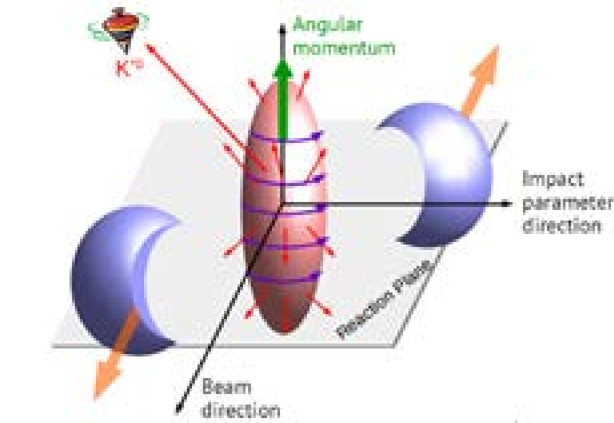


Fig. 2: Schematic representation of a frame of reference to calculate the spin alignment of vector mesons (spin-1 meson K*0 shown here). The purple hemispheres are the outgoing nuclei and the magenta ellipsoidal is the participant nuclear matter which has been imparted a large angular momentum.

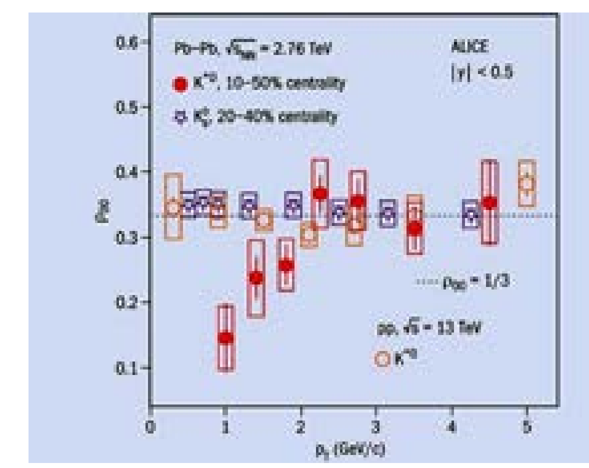


Fig. 3: The spin alignment of (spin-1) K*0 mesons (red circles) measured in terms of deviations from ρ00 = 1/3, which is shown here versus their transverse momenta, pT. The ρ00 was estimated for (spin-0) K mesons (magenta stars), and K*0 mesons produced in proton-proton collisions with negligible angular momentum (orange circles), as systematic tests.

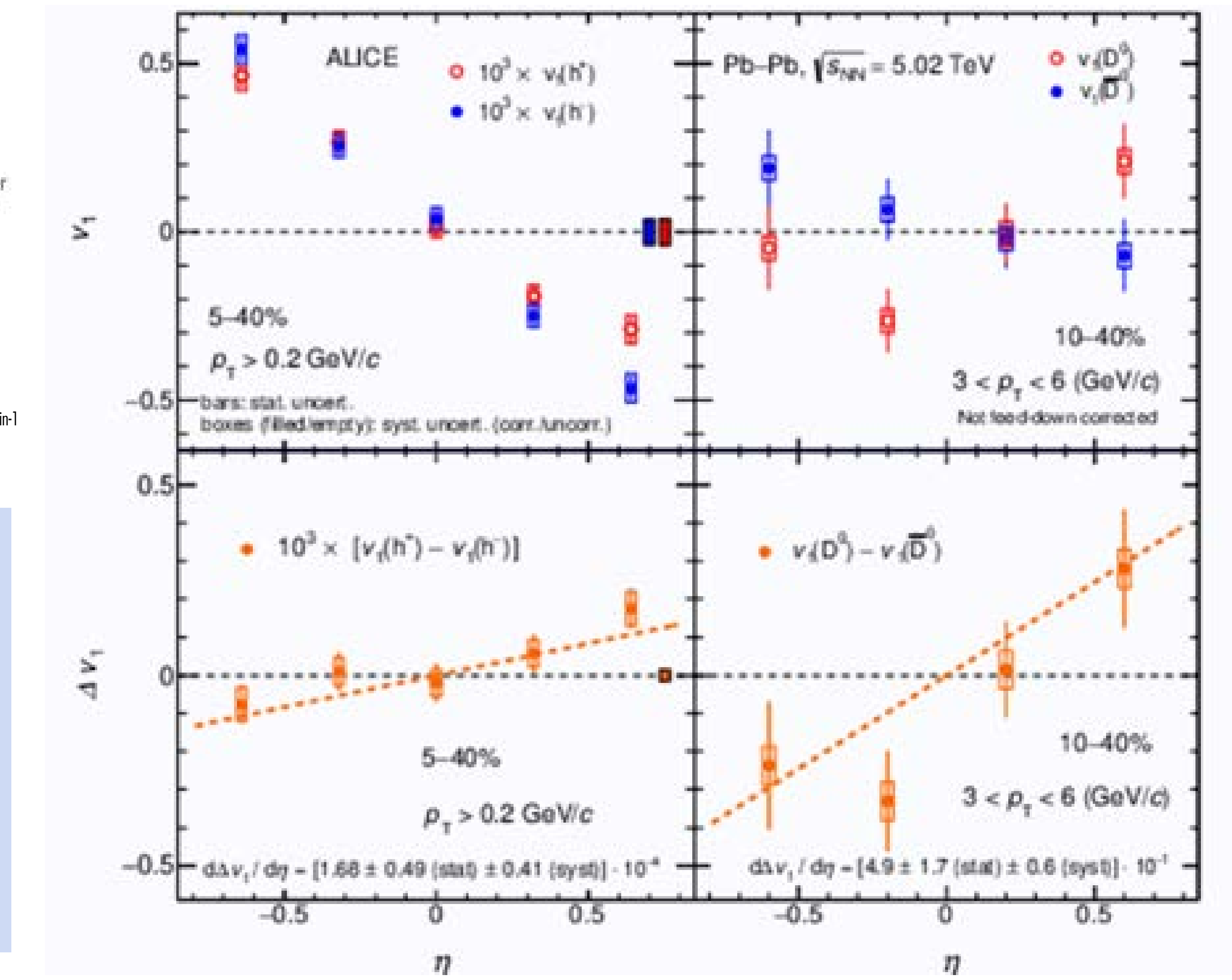


Fig. 4: The directed flow v1 of positively and negatively charged hadrons (left, scaled by a factor 10 for visibility) is three orders of magnitude smaller than for D and D mesons (right). In both cases, the difference ρ1 between the charge-conjugate species as a the function of pseudorapidity ρ(bottom panels).

plasma's rotation. This should in turn cause the spins of vector (spin =1) mesons (K^{*0} and ϕ) to align. Spin alignment can be studied by measuring the angular distribution of the decay products of the vector mesons. It is quantified by the probability ρ_{00} of finding a vector meson in a spin state 0 with respect to the direction of the angular momentum of the rotating QGP, which is approximately perpendicular to the plane of the beam direction and the impact parameter of the two colliding nuclei. In the absence of spin-alignment effects, the probability of finding a vector meson in any of the three spin states (−1, 0, 1) should be equal, with $\rho_{00} = 1/3$. The ALICE collaboration measured the ρ_{00} and found it to deviate from $1/3$ for low momentum at a level of 3σ (see Fig. 3). A number of systematic tests were carried out to verify these results. K^0_S mesons do indeed yield $\rho_{00} = 1/3$, indicating no spin alignment, as must be true for a spin-zero particle. For proton–proton collisions, the absence of initial angular momentum also leads to $\rho_{00} = 1/3$, consistent with the observed neutral K^* spin alignment being the result of spin-orbit coupling. The measurements are a step towards experimentally establishing spin-orbit interactions in the relativistic-QCD matter of the quark–gluon plasma and hence detection of the large initial angular momentum.

Establishing the strongest yet probed magnetic field: Left–right asymmetry in the production of negatively and positively charged particles relative to the collision reaction plane is one of the observables that are directly sensitive to electromagnetic fields. This asymmetry, called directed flow (v_1), is sensitive to two main competing effects: the Lorentz force experienced by charged particles (quarks) propagating in the magnetic field, and the Faraday effect – the quark current that is induced by the rapidly decreasing magnetic field. Charm quarks are produced in the early stages of heavy-ion collisions and are therefore more strongly affected by the electromagnetic fields than lighter quarks. The experimental signature for the large magnetic field would be a much larger v_1 for charm hadrons compared to light quark hadrons.

The ALICE collaboration has measured the directed flow, v_1, for charged hadrons and D0 mesons as a function of pseudorapidity ($\eta = -\ln\tan[\theta/2]$) in lead–lead collisions at $\sqrt{s_{NN}} = 5.02$ TeV [4]. The top-left panel of the Fig. 4 shows the η dependence of v_1 for charged hadrons. The difference Δv_1 between positively and negatively charged hadrons is shown in the bottom-left panel. The η slope is found to be $d\Delta v_1/d\eta = 1.68 \pm 0.49$ (stat) $\pm\ 0.41$ (syst). The right-hand panels show the same analysis for the neutral charmed mesons D and $\bar{D}$. The measured directed flows are found to be about three orders of magnitude larger than for the charged hadrons, reflecting the stronger fields experienced immediately after the collision when the charm quarks are created. The slope of the differences in the directed flows mesons for D is $d\Delta v_1/d\eta = 4.9 \pm 1.7$ (stat) $\pm\ 0.6$ (syst). These measurements provide an intriguing first sign of the effect of the large magnetic fields experienced in heavy-ion collisions on final-state particles.

Links to physics of neutron star: As discussed above, ALICE has created in laboratory a perfect fluid of quarks and gluons that is polarized and rotating. The few microseconds old Universe was composed of a matter of free quarks and gluons, similar to that observed at ALICE. However, are there any other such systems in nature which also has such intense magnetic field and rotations? Can our knowledge from experiments like ALICE at CERN help understand such systems?

An object in the Universe which has such rotational and magnetic field properties is a neutron star. They are one of the most exotic objects in the Universe, whose physics is controlled by the long-range gravitational and electromagnetic interactions, as well as the short-range weak and strong nuclear interactions. They exhibit extreme stellar properties in terms of the gravitational field, rotational frequency, magnetic field, surface temperature as well as extreme nuclear properties such as nucleon density, isospin asymmetry, bulk superfluidity, and superconductivity. Due to the steep gradients in pressure and density necessary to stabilize the star against gravity, one expects much larger central densities, leading to super-dense matter, nucleons compressed by gravity well beyond the average density of normal nuclei. The radial profile of neutron stars is determined by hydrostatic equilibrium under gravitational forces and by the equation of state of matter, namely its pressure and composition as a function of density, which calls for the presence of different exotic phases like hyperonic matter. However, a serious problem with hyperons being included in the equation of state is that the interaction cross-sections of these heavy strange baryons with nucleons are poorly constrained by accelerator experiments. The current accelerator-based experiments like ALICE at LHC is an ideal location for studying hyperon, hypernuclei, strange hadron interactions with nucleons. The physics learned at LHC can be used to understand properties of neutron star.

In this context, ALICE has carried out some interesting recent measurements.

The lifetime of hypernuclei: The hyperon-nucleon (Y-N) interaction is of fundamental interest because it introduces the strangeness quantum number in high density nuclear matter, such as a neutron star. The formation of hyperons softens the equation of state and reduces the possible maximum mass of the corresponding neutron star, which makes it extremely difficult to describe neutron stars exceeding two solar masses, several of which were observed recently. Among other explanations, alternative Y-N couplings have been suggested as possible solutions for the so-called "hyperon puzzle".

Hypernuclei are bound states of nucleons and hyperons, hence they are natural hyperon-baryon correlation systems. They can be used as an experimental probe to study the Y-N interaction. Studying their properties is one of the best ways to investigate the strengths of hyperon–nucleon interactions. The lifetime of a hypernucleus depends on the strength of the Y-N interactions. Therefore, a precise determination of the lifetime of hypernuclei provides direct information on the Y-N interaction strength. Further, the high collision energies of the heavy-ions at LHC (2.76 and 5.02 TeV) creates favourable conditions to produce hypernuclei in significant quantities. The lightest, the hypertriton, is a bound state of a proton, a neutron and a Λ.

Fig. 6 below shows the ALICE collaboration measurement of the hypertriton lifetime using Pb–Pb collisions at $\sqrt{s_{NN}}$ = 5.02 TeV, along with results from other experiments and theory calculations [5]. The lifetime of the (anti-)hypertriton is determined by reconstructing the two-body decay channel with a charged pion, namely ${}^{3}_{\Lambda}H \rightarrow {}^{3}He + \pi$. The measured lifetime is 242^{+34}_{-38} (stat) ± 17 (syst) picoseconds. The insight these measurements bring are the following: with Λ-separation energy of only ~130 keV, the average distance between the Λ and the deuteron core of the hypertriton is 10.6 fm. This relatively large separation implies only a small perturbation to the Λ wavefunctio inside the hypernucleus, and therefore a hypertriton lifetime close to that of a free Λ, 263.2 ± 2.0 ps. Most calculations predict the hypertriton lifetime to be in the range of 213 to 256 ps. Combining the ALICE result with previous measurements gives an average lifetime of hypernucleus to be ~ 206 ps, which is about 22% shorter than the lifetime of a free L, indicating possibil of a stronger hyperon-nucleon interaction in the hypernucleus system. This is a crucial input for models attempting to understand the physics of neutron star.

Interaction potentials: The ALICE collaboration is able to use the scattering between particles produced in collisions to constrain interaction potentials in a new way. So far, pK, pΛ, pΣ , pΞ and pΩ interactions have been investigated. Strong final-state interactions between pairs of particles make their momenta more parallel to each other in the case of an attractive interaction and increase the opening angle between them in the case of a repulsive interaction. The attractive potential of the p-Ξ interaction was observed by measuring the correlation of pairs of protons and Ξ particles as a function of their relative momentum (the correlation function) and comparing it with theoretical calculations based on different interaction potentials.

Data from proton–lead collisions at a centre-of-mass energy per nucleon pair of 5.02 TeV show that p-Ξ pairs are produced at very small distances (~1.4 fm); the measured correlation is therefore sensitive to the short-range strong interaction. The measured p-Ξ correlations were found to be stronger than theoretical correlation functions with only a Coulomb interaction, whereas the prediction obtained by including both the Coulomb and strong interactions (as calculated by the HAL-QCD collaboration) agrees with the data (Fig. 6) [6]. As a first step towards evaluating the impact of these results on models of neutron-star matter, the HAL-QCD interaction

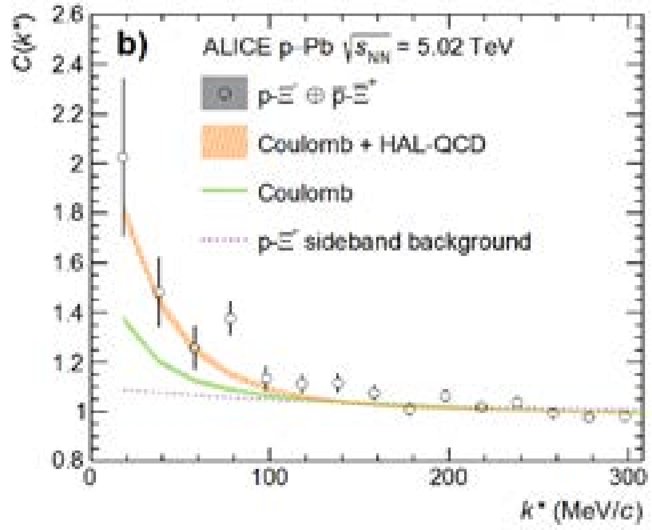


Fig. 6: TProton-Cascade correlation as a function of the relative momentum of the proton and cascade, and the expected background contribution. The theoretical results, including only the Coulomb interaction, and both the Coulomb and strong interaction, as computed within the lattice–QCD framework are also shown.

potential was used to compute the single-particle potential of Ξ within neutron-rich matter. A slightly repulsive interaction was inferred (of the order of 6 MeV, compared to the 1322 MeV mass of the Ξ), leading to better constraints on the equation of state for dense hadronic systems that contain Ξ particles.

ALICE has thus made some very exciting recent measurements related to QCD matter that indicates the first evidence of spin-orbital angular momentum interactions, presence of strong electromagnetic fields and findings which can be linked to determining the equation of state for dense and cold nuclear matter with strange hadrons, like a neutron star. □

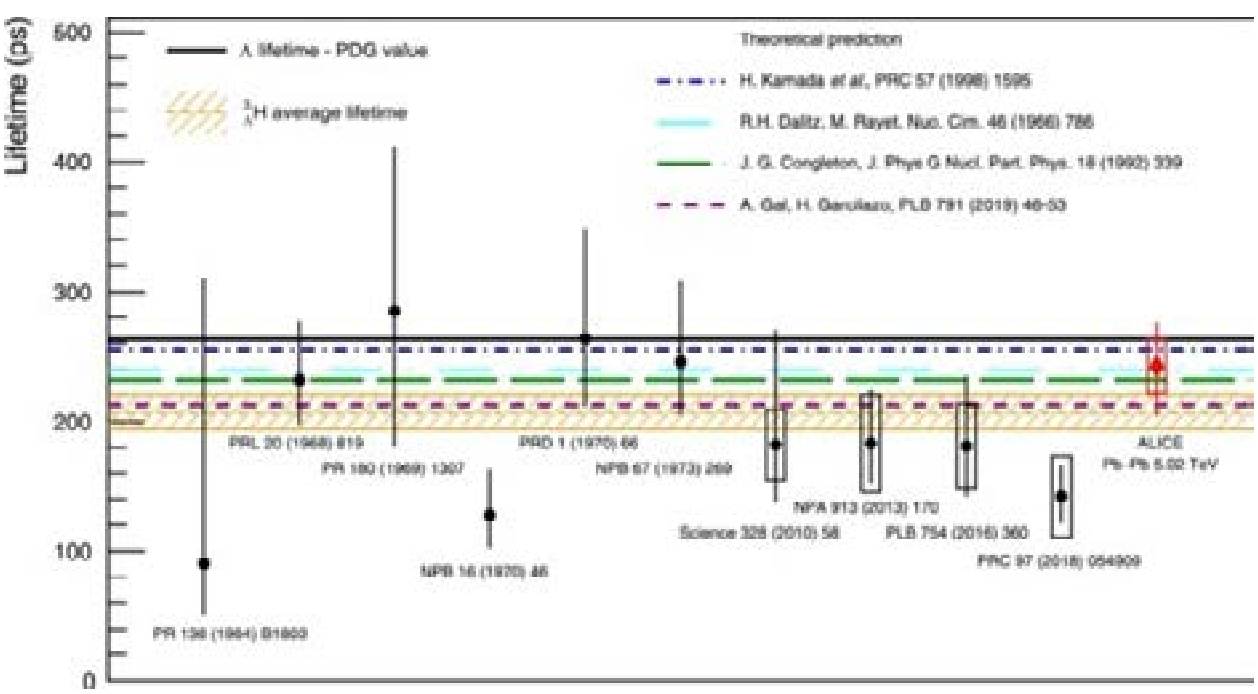


Fig. 5: Hypertriton lifetime measurements from various experiments, including the latest results from ALICE at LHC (red), The orange band is the average of the lifetime values and the dashed lines represent theoretical predictions

References

[1] P. Braun-Munzinger and J. Wambach, *The Phase Diagram of Strongly Interacting Matter*. Rev. Mod. Phys. 81, 1031-1050 (2009).

[2] B. Jacak and P. Steinberg, *Creating the Perfect Liquid in Heavy-Ion Collisions*. Phys. Today 63N5, 39-43 (2010).

[3] S. Acharya et al. [ALICE], *Evidence of Spin-Orbital Angular Momentum Interactions in Relativistic Heavy-Ion Collisions*. Phys. Rev. Lett. 125, 012301 (2020).

[4] S. Acharya et al. [ALICE], *Probing the Effects of Strong Electromagnetic Fields with Charge-Dependent Directed Flow in Pb-Pb Collisions at the LHC*. Phys. Rev. Lett. 125, 022301 (2020).

[5] S. Acharya et al. [ALICE], *3LH and (_Λ̄^3)H Lifetime Measurement in Pb-Pb Collisions at √sNN = 5.02 TeV Via Two-Body Decay*. Phys. Lett. B 797, 134905 (2019).

[6] S. Acharya et al. [ALICE], First Observation of an Attractive Interaction between a Proton and a Cascade Baryon. Phys. Rev. Lett. 123, no.11, 112002 (2019).

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