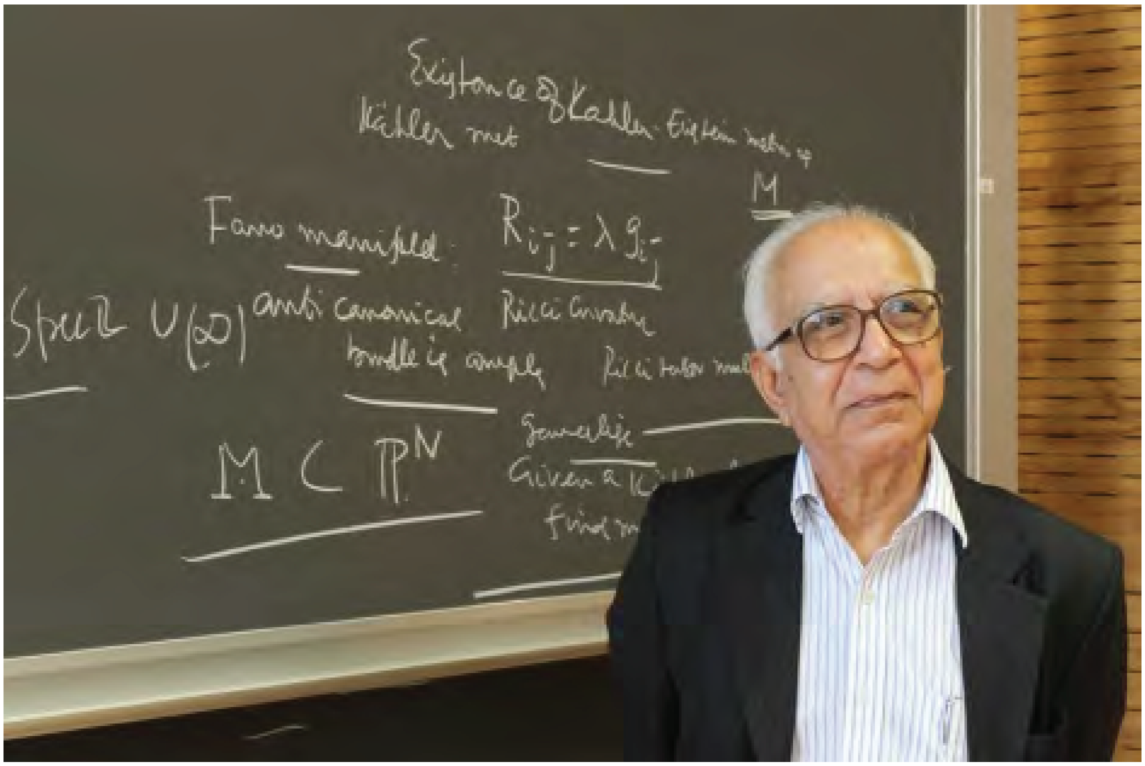
A Versatile Ace at Bridge Building

M.S. Narasimhan in Conversation with Sudhir Rao



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M.S. Narasimhan counts among the most impactful of Indian mathematicians to have worked in an era just after the passing away of Ramanujan. With a handful of hugely talented fellow mathematicians, he led from the front in establishing the School of Mathematics at TIFR, Bombay as a world-renowned centre for mathematical excellence, thus fulfilling the dream that both its founder H.J. Bhabha, and his own mentor K. Chandrasekharan, had nurtured for Indian mathematics. Narasimhan is known for his deep work that has profoundly shaped and impacted subjects both inside and outside of mathematics, and also for the expanse of his interests, with singular contributions to many highly arcane subjects. In an interview that bore witness to his erudition and wit, the sprightly octogenarian spoke at length on his life, his work and his heroes.

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TIFR Archives

MSN with S. Chandrasekhar, at TIFR Bombay.

Thank you for making time to speak to us, Prof. Narasimhan.

MSN: Thank you very much for inviting me.

Could you please tell us a little bit about your childhood and upbringing?

MSN: I was born on the 7th of June 1932, and I come from a small village called Thandarai, in Tamil Nadu. At that time, it had only an elementary school in the village, serving students from standards one to five. And there was only one teacher for all the classes. To go to middle school, one had to travel about five miles from our place. There were no buses then, and we had to find some other means of transportation for getting there.

As for my family background, I come from a family of agriculturalists, and we were, at the time I was young, quite well off. But afterwards, many things happened—my father passed away when I was still 12 or 13, and then we had some reduced circumstances. But still, my other family members encouraged me to do what I wanted.

Were you the eldest of the children?

MSN: Yes, I was the eldest of five children.

From your very early days, did mathematics appeal to you, or were you generally good at academics?

MSN: I was generally good at academics. But I was, for some reason, particularly interested in mathematics, even from a very young age of 13 or 14. I mean, it just interested me. It also happened that I came across some advanced textbooks in mathematics—textbooks, not research articles—which contained more information than what I studied in school. These books further increased my interest in mathematics.

There was another reason too. In school, you're in general taught or told certain things, and you're supposed to repeat them in the exams. "Don't ask questions"—that is the general attitude. But in maths, it was different. If one repeated what one was told, then one would get say, 40 percent of the marks; but if you wanted to do well, you had to solve on your own "riders" or problems. That meant that you had to think for yourself. This must have been one of the reasons for my strong interest in maths. It gave you some opportunity to exercise your brain. It's not just the nature of mathematics itself, but maybe this was the only subject which gave you an opportunity to think for yourself, and I got interested that way.

Since you mentioned some textbooks that got you interested, was there any particular book?

MSN: No, it's not like the Ramanujan story. [Laughs] Some of them may even have been well-known textbooks in geometry of those times, but I don't remember the names.

Since you brought up the name of Ramanujan, we note that you were born in an era that had just come in after the passing away of Ramanujan. By popular accounts, Ramanujan was a household name in those days in more ways than one. When was the first time that you actually heard about Ramanujan, or was his name generally in the air?

MSN: I think it was generally in the air, as you said. I cannot say at what age I first heard about him. By the way, his name was known not only in south India, but also in the whole of

India, which was then the undivided India. Even in distant Lahore, a young college student who later became a famous theoretical physicist, and a Nobel laureate too, wrote his first paper based on a work of Ramanujan. You probably know that Abdus Salam, when he was young, heard about Ramanujan for the very first time from his mother. His family lived in Lahore at the time. This means that Ramanujan's must have been a household name across India.

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Fr. Racine (on the right) at TIFR Bombay.

From the small village where you did your early education, you moved to Chennai, to Loyola College. Was this a conscious decision? Was Loyola College on the map for you?

MSN: I wanted to study mathematics. At that time, there were two colleges in Madras which were supposed to be very good for mathematics. I chose Loyola College. I didn't know at the time I joined though, as to why it was supposed to have been good for mathematics.

Which was the other college?

MSN: The Vivekananda College.

When you were enrolled in Loyola College, a very significant event happened in your life, which is the blossoming of a great friendship with C.S. Seshadri. How exactly did this happen?

MSN: After finishing school, I went to what used to be then called "Intermediate", which is for two years, and passing which you went into an undergraduate course. So this Intermediate course had a large number of students in mathematics, maybe 300 or so. It was divided into two sections, of 150 students each. Seshadri and I were in different sections. There was, at the end of the first term or second term, an exam in which I came first, and in the other section Seshadri stood first. But I didn't have too much contact with Seshadri during those Intermediate days. Of course, when I joined the mathematics honours course, it was a small class with 30 or 35 students, and naturally we got to know each other. But I think our real friendship started only later, after we both joined the Tata Institute [TIFR]. He had his own group of friends in college, and they were interested very much in Carnatic music and the like, while my interests were somewhat different.

Presumably when you moved to Loyola College for your Intermediate, your education was in the English medium. Before that, in school, was it in English?

MSN: Everything was in Tamil up to Intermediate. But the important thing was that English as a subject was taught quite well. So, this transition from Tamil to English medium, which happened when we joined the university, was not traumatic. In fact, I would even say that it was quite smooth. I don't know why people talk so much about what language you should do your early education in. Of course, it's obvious that in school it should be in your mother tongue. Everything, including mathematics, was taught in Tamil.

In Loyola College, for your honours course, you came across Fr. Racine, who played a mentoring role in your early career, as well as that of Prof. Seshadri's. Your thoughts on that.

MSN: Fr. Racine[*] was a mentor not only for the two of us, but also for almost all of that whole generation of mathematicians. He was a member of the Society of Jesus, or Jesuits as they are called, and Loyola College is run by them. He taught in Loyola College, and presumably earlier in Trichy too. And the amazing thing was that he was in touch with many of the modern developments in mathematics. He was also a close friend of many top French mathematicians of the day like Jean Leray. In fact, he was a PhD student of Élie Cartan himself.

At that time, if at all there was a subject that was taught well in mathematics in Indian universities, it was analysis. We had never even heard of modern algebra or anything else. Mostly, some bad textbooks in the British genre of applied mathematics were used. As a refreshing change, and for the first time, at least in south India, Racine introduced algebra—the so-called modern algebra of that time, and what you would now simply call algebra. And it was taught very well. The way it was taught was by insisting not only on computations, but also on concepts. This gave one a slightly different view of mathematics than what you would normally get. And also, somehow, he induced in motivated students, mainly by talking to them, a realization and understanding of what was deep mathematics and what was not. In a sense, he cultivated some kind of a mathematical taste in his students. It's all very difficult to explain in precise terms what mathematical taste is. Without yourself being aware of it, you were actually moving into a different level of mathematical appreciation and knowledge, thanks to his mentoring.

He taught only the honours class, and never used to teach in the first year. However, he still wanted to get in touch with his students before teaching them. So on Monday mornings, there used to be a catechism class in Loyola College, which was meant for Catholics. The other students had something called a moral instruction class. Racine used to take the moral instruction class, the idea being to get to know the students whom he

[*] "Fr." is an abbreviation for the title of "Father", commonly used to address Jesuit priests.

would teach mathematics. But the only thing he used to do was to come to the classroom and write letters, doing his own work, while the students were mostly just left alone. One day, I still remember, he came in with a serious face and said, "The Indian mathematician Pillai has died in an accident, and let us stand up in silence and pay our respects." Of course, we had never heard of S.S. Pillai at that stage.*

The honours class size was 30 or 35 students. It was sort of an open classroom setting, with French windows, and you could just walk outside, across the class. I actually used to walk out of his classes sometimes when he was not looking. At the end of the first term though, I did very well in my exam and I was noticed by him, and after that I could not walk out of the class! [Laughs]

I got to know him fairly well from then on. More seriously, I think that was the way in which I first got exposed to some kind of mathematics at a high level.

> "It's obvious that in school it should be in your mother tongue

How exactly would he do that? Would he tell you anecdotes of his own mathematical education in France? This inculcation of a higher mathematical taste, or aesthetics, how was it actually done?

MSN: I mentioned that algebra was introduced in our honours course curriculum. The book by van der Waerden, in my opinion the best book on the subject, was the book that was followed in our class. There was only a German edition of the book available then. Racine used to personally translate it into English, type it on his own like the French usually do, and distribute notes—because there was no good textbook in English at the time. Somehow, I came to know that there was an English edition that had just then become available in Higginbothams, the bookstore on Mount Road. I went there and ordered two volumes of the English translation, which was somewhat of a strain on our finances at the time. Anyway, I got them, and went to Racine and showed them to him.

"How did you get it?" he asked me. He didn't know there was an English translation because he didn't care. He had a German version and he could read German. In the first English edition, there were some wrong translations. So when I had some problems following it, I used to go and ask Racine. He would immediately consult his own copy of the book in German and would agree that I was right, and help me along too.

But coming back to your question more seriously, I wouldn't call him a very good teacher in the usual sense. I mean, in terms of teaching courses, he just used to give these notes, which were well-written; you just read it yourself. Importantly, if he found that some students were good at mathematics and interested in it, he used to meet them outside the class and talk to them personally, one on one, and that's how it really happened. Also, I don't think he ever told us about Élie Cartan, or his own education.

I must also say that there was a very good analysis teacher there. Again, the notes for analysis were prepared by Racine. His notes on algebra and analysis are still available with me. One of the things which he did for the analysis course—which he didn't teach but for which his notes were used—was to bring in some topological concepts very clearly. Ideas like open sets, closed sets in $\mathbb{R}^n$ [the n-dimensional space] and all that. So when we went on to learn set topology, it was an easy transition. Usually, following the old textbooks or even the book by [G.H.] Hardy, you would not know what an open set was. Some even used archaic terminology such as cluster points, resulting in confusion.

In other words, one of the main things Racine did was to bring in new ideas which either he himself taught, or which some other good lecturer taught. He introduced to us new material like algebra, and thereby inculcated a completely new and different way of thinking. Direct contact with him in conversations outside the class also helped immensely.

By the way, my first publication was in the *Mathematics Student*. In those days, the *Mathematics Student* used to pose problems, which sort of challenged students. If someone solved it, the solution along with the name of the person would appear in the next issue. I solved one of the problems and that's how my first "publication" happened.

Before I came into contact with Racine, I also did some piece of research on my own, and actually wrote down a paper on triangle geometry, which I then gave to my teacher. He in turn took it to Racine. Racine told my teacher, "This student looks clever, but he should know that this is neither the kind of mathematics we do nowadays, nor should he be doing."

If I were stupid, I would have got upset and discouraged. But somehow, I think I had the good sense to understand what he meant. And I could eventually make this transition when I came into contact with him. I believe that one should know, ideally at an early stage, what is the kind of mathematics one should study. The required clarity of thought in my own case actually came from my contact with Racine. Most of the time during our college education, all we knew was only Euclid. While a study of Euclid in school days is important and is what fascinates many students, even attracting many of them to mathematics, it is equally essential to know that geometry had indeed moved far beyond Euclid. Quite recently, this was the topic of one of the public lectures I gave, "Geometry beyond Euclid". Young students must know what exists beyond Euclid. That's the kind of role Racine used to play.

A kind of cultural moulding of young minds…?

MSN: Yes, and also telling them what

* For more about S.S. Pillai, see the article: R. Narasimhan. "The Coming of Age of Mathematics in India". *Bhāvanā*. Jan 2017. **1**(1): 37–51

they should and should not be doing. Because otherwise they would just waste their talents.

From a very encouraging mentor in Fr. Racine at Loyola, you moved on to the Tata Institute of Fundamental Research. Was this transition too mediated by Fr. Racine?

MSN: Yes, otherwise we would not even have known that there was a Tata Institute. As a matter of fact, it was then not very well-known either, as it was still very newly set up. Racine knew K. Chandrasekharan (KC) and K.G. Ramanathan (KGR), and knew that they were trying to do something potentially significant.

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> Young students must know what exists beyond Euclid

In Madras, there was around the same time, an active group of mathematicians: Racine, Ananda Rau, Vaidyanathaswamy, and then many young people like Minakshisundaram, KGR, KC. They used to have seminars and they were a very lively group of mathematicians, I would say. That's how Racine must have known them. M.H. Stone used to travel around the world, and he had once come to Chennai and perhaps very likely even met Racine. He must have suggested that young people should go outside India. But sadly, it's mostly forgotten that there was a lively group of mathematicians then in Madras.

When you first arrived as a student at TIFR in Bombay, KC was the head of the School of Mathematics there, and eventually went on to become your PhD thesis advisor. How exactly did this happen?

MSN: At the time there were only two people who were permanent members on the faculty, KC and KGR, and you had to choose one of them. [Laughs] I was probably more interested in geometry and analysis than in algebra and number theory at that stage. And KC also gave a course in zeta functions, which I followed quite well. So, it was natural that I gravitated towards KC, without having any particular problem in mind that I wanted to work with him on.

How was the student–mentor relationship between you and him, and how often did you meet him?

MSN: At that time, in the mid-1950s or so, it is important to see how mathematics was done in India. There were some first-rate people in India. But then they were all scattered around and isolated. They usually learnt only one subject, and did either good or competent work, but in that subject alone. As somebody back then used to say, "Oh, for my students, I first simply define what a group is, and immediately give them a problem." There was no idea or concept of a modern-day graduate school at all in India at that time. On the other hand, before you go into research today, you are taught some advanced topics in a graduate course, for one or two years, by first-rate people. You absorb that, and you have to also follow three or four other courses reasonably well so that you have some broad-based knowledge before starting research. Otherwise, you are just going into a small corner.

So, the new approach of having graduate school courses was one of the first things that was introduced in the Tata Institute. At that time, as I said before, there were only two senior people there, KC and KGR. KC used to lecture mostly on analysis, and KGR in algebra. KC would give courses in measure theory, real analysis, or maybe sometimes on zeta functions; KGR mostly in algebra and number theory. But for other topics, there was no expertise in India at all; especially in differential and algebraic geometry, Lie groups, or even for PDEs [partial differential equations]. So this was KC's great idea: to invite first-rate mathematicians to come to TIFR and give lectures for two or three months, all very advanced lectures. The students had to attend these lectures, take notes, write them up, and show them to the lecturer. The idea was that one cannot write notes without first understanding what was going on. [Laughs]

In other words, apart from KC and KGR, there was a kind of a floating faculty. Research students were given complete freedom to study what they wanted, as long as KC and KGR were convinced that the student was capable. I met KC maybe twice or thrice a month. At that time, I was more interested in what Laurent Schwartz was doing and KC knew about it, so he didn't bother very much. Still, we used to meet and talk in general about mathematics. I learned from him some number theory, some analysis. It was more of a general talk, and not always on what I was doing.

I will give you an idea of the courses we did those days. In the first year, I think KC was lecturing on analysis and KGR on algebra. Warren Ambrose, from MIT, was a visitor and taught a course. He assumed that we all knew topological spaces. And when we said that we didn't know what a topological space was, in three hours he covered all the basics of set topology. In the rest of the course over the next two to three months, he went on to prove the existence of the Haar measure; and then he proved the spectral theorem and used it to prove the Peter–Weyl Theorem, and so on.

I think he was from the south of USA. In the first one or two lectures, we didn't understand anything about what he was talking, because of his accent. [Laughs] But he used to write every single sentence—complete sentences—on the board, and after two or three lectures, we were fine.

That was in the first year. In the second year, it was Samuel Eilenberg who came visiting. He was one of the best lecturers I knew. At that time, the functorial point of view was just coming into the scene, which now every young person knows. He taught a course on algebraic topology—and by the way, I still have the notes, all of it, running up to 400 pages of my own handwritten notes. He taught everything in a functorial way, without ever mentioning the word "category" or a "functor". Consequently, when you read Grothendieck, or if somebody said "Let X be a representable functor", you already knew what it was about because it was sort of already embedded in your way of thinking. That way, an exposure to the more modern way of thinking was brought in.

> "He taught everything in a functorial way, without ever mentioning the word "category" or a "functor"

I still remember that once he wanted to use the tensor product. We said we didn't know what a tensor product was. So in the next lecture he defined tensor product as a certain representable functor, as we would say now. Of course, there was no such word at the time. I mean, this was all done at that level of rigour.

And then came Laurent Schwartz. I eventually developed an interest in almost all the topics he taught in his course on complex manifolds. I wrote up the notes for his course too.

This "cultural awakening" that the students were subjected to, was this a consciously thought out decision by KC and KGR?

MSN: I think it was sort of subconscious. They knew what they were doing. It's not that they said okay, we will get this person here, and he will teach our students the idea of functors. It was not like that. They used to get some really first-rate people who would lecture on very modern and topical mathematics, and that way they hoped that the students would learn. Luckily, there were also some very good students.

Do you count courses by Ambrose, Schwartz, Eilenberg and Carl Siegel as among the best that you have attended?

MSN: Yes. I did attend Siegel's course but I wasn't particularly interested in that field. The course was more in number theory and related aspects. Though I was following the contents, thanks to KGR, and sometimes did attend some lectures too, I was not particularly involved in that.

So starting from being mentored by Fr. Racine and developing a mathematical taste—your time at TIFR seems like a logical continuation of that.

MSN: I was lucky to be able to do that, to have the opportunities to do that. That is the amazing thing. It is also good to see how students were trained in the West in those days. Students were taught there by really first-rate people who were actually doing research. So just by attending these classes, you somehow absorbed these ideas—like mother's milk, as they say. That culture sadly was not there in India at the time. This invaluable glimpse into the way things were done outside India, in the best places, is what was made possible by these visiting professors. They used to stay only for two or three months though, but we had good student seminars too.

Of course, there also was the Bourbaki. This is the finest in French mathematics. Let's not get into what the Bourbaki really is. Nowadays there's no more a debate on what the Bourbaki is. And the great set of books they brought out taught you how first-rate mathematicians used to think even though you did not have direct contact with them. You may say it was abstract or difficult, but for a young student, this was a gateway to see how great mathematicians thought, and how mathematics was actually developed. It was important especially for young researchers in India, who mostly had no direct contact with renowned centres of mathematics.

Bourbaki's books used to appear continuously, and every year there would be a new book that we all used to wait for with excitement. At that time, and which in retrospect I used to joke about much later, we used to wait for every new book from Bourbaki just as children in recent years used to wait for Harry Potter books. [Laughs] The same kind of enthusiasm! Many of us even used to say that we were all Bourbakists. Of course, we had to learn French, but that was not so difficult.

Visitors to TIFR. (Left to right): Donald Spencer and Georges de Rham.

By the way, the importance of Bourbaki was told to us by both KC and KGR, though they were not originally themselves from that school.

The Bourbaki movement must have been a truly worldwide influence. KC studied with Ananda Rau, who himself was Hardy's student, and so firmly belonged to the British school. KC later went to IAS, Princeton in the US. But then when he came back, he saw that the French school was thriving, and best not ignored! Your impressions on this.

MSN: At IAS Princeton, KC came into contact with really great people, like Hermann Weyl. KC was an assistant of Weyl. I think both KC and KGR also understood how mathematics teaching and research was done, both in Germany and the USA. In *Mathematics Student* there is an article*

* H. Weyl. 1953. "Universities and Science in Germany". *Math. Student*. **21**. Volumes 1, 2: pp. 1–26

by Hermann Weyl, about how research was organized in German universities. So going to Princeton for both KC and KGR must have been a revelation. They [KC and KGR] didn't actually explicitly ask us to read the Bourbaki. But Bourbaki was the happening thing.

So the spirit of Bourbaki was already alive and available in TIFR. Further when you moved to Paris to work with Laurent Schwartz, you met, interacted with, and even learnt from them directly. Was that so?

MSN: Of course, I learnt from some of them like Schwartz directly, and also from some of his famous students.

Such as Alexander Grothendieck?

MSN: Contact with Grothendieck happened much later. I was at that time mostly interested in analysis. At that time the French, and particularly the Schwartz school, was really streamlining PDE with a systematic use of distributions, which in itself was then a very new input. This quick adoption of new ideas was also very much a Bourbaki tradition. Schwartz himself says that if he were not associated with the Bourbaki, he could not have come up with the theory of distributions. I used to meet Schwartz every week and we used to discuss things.

Now Bourbaki has become part of everybody's knowledge. There are no more people saying that Bourbaki is a bad influence. It seems even Hermann Weyl had once said that Bourbaki was too abstract. But after a few years he admitted that he was wrong. [Laughs] The starting point of whatever was happening in TIFR in those early days was this strong alignment to the French school through Bourbaki.

It kind of created a revolution?

MSN: Yes, and luckily, we were into it. We also didn't bother what people thought about Bourbaki, or even whether mathematics itself was the better or the worse for it. Look at Lars Hormander, who never bothered about these things. He used distributions and then pushed PDEs to a whole new level, compared to others who just kept debating in vain about Bourbaki and distributions and their bad influence.

To tell you a story: English translations of Bourbaki's works became available after some time. There was once a discussion in the TIFR library committee on whether we should buy them or not. I said no, because people had to learn French, and reading Bourbaki in the original was one way to do it. This applied not only for Bourbaki, but also for the works of many other mathematicians who wrote in French. Then we found that the students at TIFR were not reading Bourbaki at all. I then said that we shall buy the English versions so that they can read Bourbaki, in whatever language! [Laughs]

By general admission, Paris was the world headquarters of mathematics at that time, populated by the likes of Laurent Schwartz, Henri Cartan, Jean Pierre-Serre, and even Jean Leray, the eminent analyst.

MSN: Well, yes. Talking of Leray, yes, he was a very eminent analyst of course, but it is really his contributions to other areas of mathematics which is, in a concrete sense, the very basis of the revolution in French mathematics. He introduced sheaves, cohomology and direct images of sheaves, spectral sequences; all of which are a part of a mathematician's dictionary today, and used extensively in several parts of mathematics. When Leray wrote his papers, not many really understood them. Only later work by Henri Cartan, Jean-Louis Koszul and others made them accessible and well-known, or so the story goes.

I'm reminded of a story in this connection. It seems Racine gave lectures on sheaf theory in Madras (maybe in Madras University) when Leray's work first appeared. People wouldn't believe this when I used to say it before. Anyway, here it is. I once spoke about Racine's lectures on sheaf theory to Armand Borel, and he was not surprised. Apparently, when Borel was first reading Leray's papers, he consulted the review journal *Zentralblatt MATH*, to see if it carried reviews of these rather difficult to follow papers of Leray. In *Zentralblatt*, Borel found two or three reviews of the works of Leray, stating "By Racine, (Madras)".

> "It seems even Hermann Weyl had once said that Bourbaki was too abstract

And Armand Borel actually read those?

MSN: Yes! [Laughs] I think Leray was a part of the Bourbaki for some time and left it later. As I mentioned earlier, Leray had three big ideas in sheaves, cohomology and direct images of sheaves, and spectral sequences. All these things completely revolutionized topology and algebraic geometry.

I have heard that Leray came to this set of ideas by actually thinking about fluid flow and the Navier–Stokes equations.

MSN: The story is slightly different, though very interesting. I don't think it was the Navier–Stokes equations that led him to the above set of ideas. It is true though that he was interested in Navier–Stokes equations and in the applications of mathematics, nonlinear PDEs, the Leray–Schauder fixed-point theorem, and so on. He was actually a very well-recognised analyst.

But it was the time of the Second World War, when France was conquered by Germany. He was afraid that he would be forced to do some direct applications of his work in analysis. After all, these are all things that can be directly applied; the fixed-point theorem perhaps not as much, but the Navier–Stokes theorem certainly can be used for war purposes. I think he saw it somewhat in advance and said that he was no longer interested in PDEs, and that he was primarily interested in topology, at which, apparently, he arrived indirectly via the Leray–Schauder fixed-point theorem. So, he simply declared that he didn't do PDEs anymore. [Laughs]

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At the Algebraic Geometry Colloquium held at TIFR Bombay, 1968. From right to left: J.W.S. Cassels, MSN, Yuri Manin, KGR and Alexander Grothendieck.

Was that his way of escaping the obvious consequences of being associated with the German war effort?

MSN: Yes, otherwise he would have had to contribute, although he was French.

Anyway, while in Paris, I used to talk to Henri Cartan from time to time, and even used to know him socially too. I was mainly associated with the PDE group of Laurent Schwartz and his students. I also met a young Japanese mathematician there, Takeshi Kotake. Probably my first best work was with him.

This is how it happened: Schwartz used to come once a week to the university, give two lectures and then see his research students. The students used to sit on a bench outside, awaiting their turn to go and see him. Then one day a Japanese guy was sitting next to me, and we started talking. He asked me, "Do you know a mathematician called Narasimhan?" It turned out that he was referring to me. Then we talked and that's how we started collaborating. It's a kind of an accident that I met him.

But what made him ask you if you knew Narasimhan?

MSN: He could see that I was an Indian. He had actually seen one or two of my earlier papers, and so asked me if I knew this Indian mathematician. Those PDE years were very important to me when I turned later to read the work of Kunihiko Kodaira and Donald C. Spencer on deformations of complex structures, which eventually played a great role in my future work. Their main tool in this work was PDE, mainly elliptic PDE.

Was it continuous deformations that they were looking at?

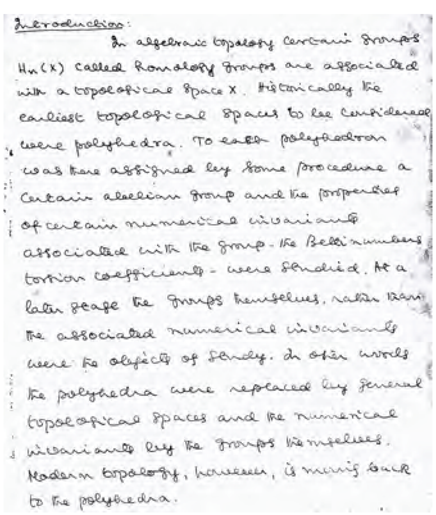
MSN: Yes, but they specifically used elliptic PDE. Since I knew elliptic PDE, it was quite easy for me to get into that. When I was reading Kodaira–Spencer, I also heard that Grothendieck had by then moved into algebraic geometry.

Oh!

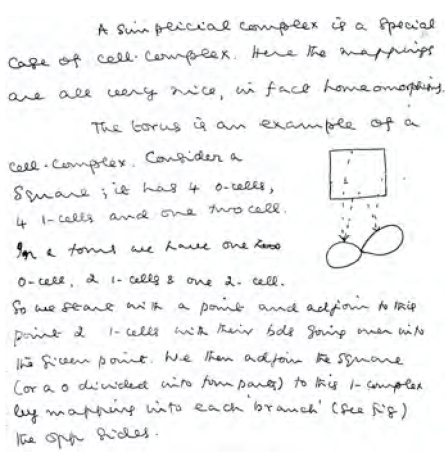
MSN: Yes. He had just then moved away from his early work on functional analysis. Someone then also told me that Grothendieck had a new way of looking at Kodaira–Spencer theory. I did not know Grothendieck then, and I just met him once in one of the corridors of the Institut Henri Poincaré.

Is this where Laurent Schwartz had his office?

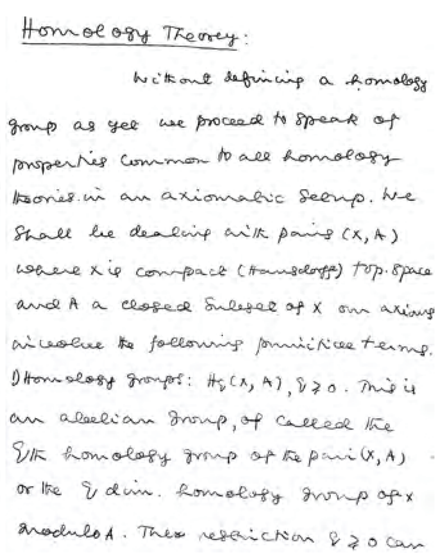
MSN: Everybody who mattered had their offices there. [Laughs] Henri Cartan once said, "Oh, I never use my office, and you can use mine." But I just didn't dare. Anyway, when I met Grothendieck, I asked him, "Could you please explain to me, your approach to Kodaira-Spencer?" He didn't really know who I was. Yet, amazingly, for three full hours, he explained to me everything in detail. Not that I understood everything. See, there are not too many mathematicians who are prepared to spend time explaining things in that detail to some unknown person.



Introduction:
In algebraic topology certain groups $H_n(X)$ called homology groups are associated with a topological space X. Historically the earliest topological spaces to be considered were polyhedra. To each polyhedron was there assigned by some procedure a certain abelian group and the properties of certain numerical invariants associated with the group - the Betti numbers torsion coefficients - were studied. At a later stage the groups themselves, rather than the associated numerical invariants were the objects of study. In other words the polyhedra were replaced by general topological spaces and the numerical invariants by the groups themselves. Modern topology, however, is moving back to the polyhedra.



A simplicial complex is a special case of cell complex. Here the mappings are all very nice, in fact homeomorphisms.
The torus is an example of a cell-complex. Consider a square; it has 4 0-cells, 4 1-cells and one two cell. In a torus we have one 0-cell, 2 1-cells & one 2-cell. So we start with a point and adjoin to this point 2 1-cells with their bdy going over into the given point. We then adjoin the square (or a o divided into four parts) to this 1-complex by mapping into each 'branch' (see fig) the opp sides.



Homology Theory:
Without defining a homology group as yet we proceed to speak of properties common to all homology theories in an axiomatic setup. We shall be dealing with pairs (X, A) where X is compact (Hausdorff) top. space and A a closed subset of X. Our axioms involve the following primitive terms.
1) Homology groups: $H_q(X, A)$, $q \geq 0$. This is an abelian group, & called the qth homology group of the pair (X, A) or the q dim. homology group of X modulo A. The restriction $q \geq 0$ can

M.S. Narasimhan

MSN's handwritten notes of Samuel Eilenberg's course on algebraic topology, given at TIFR Bombay, around 1955.

Did he explain it on the blackboard? Or with just pen and paper?

MSN: Pen and paper sometimes, but mostly as in a conversation.

Many mathematicians, especially very eminent ones, will not waste time talking to somebody who they are not sure will understand what they are saying. Some people, on the other hand, will immediately tell you the answer offhand if they know it themselves. But most people are not prepared to think for some time and then answer your questions, which for example, Armand Borel would do.

I should also say that, after many years, I dared to ask Laurent Schwartz a question. He used to come only once a week to the university, used to give lectures and meet his students. So I asked him if this arrangement was fair. What is very interesting is that he actually became very apologetic. He said, "In the old days, French professors had no offices." He meant the earlier generation like Élie Cartan and so on. They all used to work at home, and that became a kind of habit, and Schwartz too would mostly work at home, go to the Institute only to give his lectures and meet students, and then go back home.

Coming back to your interaction with Grothendieck, did you have many more such conversations?

MSN: No, not many. Of course, after I came back to Bombay I read up much of Grothendieck's work. I learned his work not by direct conversation, but by reading his work.

You mentioned the work of Kodaira and Spencer that played a significant role in your later work with Seshadri. We have heard that you took ill when you went to Paris, and that you came across the work of Kodaira and Spencer when you were not well.

MSN: That's true. I fell ill, and in fact, it was tuberculosis. Schwartz made special efforts to see that I received the best medical attention. Schwartz came to the hospital and also showed my x-ray to one of the best experts on the disease who told him: "If he (i.e. me) spends six months in a sanatorium, he will be okay. Do not worry." It was very early stages, and it was also the time when streptomycin was already available. Apparently streptomycin had some ill-effects, so they found something else to add with streptomycin to make it safer.

So you were away from action for six months?

MSN: I was away from Paris, recuperating in the Alps for six months, but I was not necessarily away from action! [Laughs]

At that time, Schwartz had just then received a huge preprint of a paper by Kodaira and Spencer. You may be surprised on how long it was—it actually appeared much later in two or three different issues of the *Annals of Mathematics*. Schwartz asked me "Are you interested in this?". I said yes, and just took it with me. It was indeed a very perceptive gift from Schwartz, in view of the enormous influence of this work on my future research.

But I must say one more thing before that, about why it was so much easier for me to get into Kodaira and Spencer. It was all thanks to Schwartz's own lectures I had heard earlier in Bombay. Also, the time period that I got into algebraic geometry was also somewhat fortuitous for me. The timing was in a sense, perfect. If I had read the work of famous algebraic geometers, which was in fact written in a very complicated language, any earlier than I actually did, I surely couldn't have found resonance with their works. But luckily for me, what happened was that, just by using the modern ideas of sheaf theory and harmonic forms, Kodaira and Spencer had published short notes in *PNAS*, totaling 30 to 40 pages. And these, by the way, were even before their deformation papers. In these short notes, the two used modern techniques of the day to prove some fantastic old theorems in algebraic geometry, and even some new theorems too. So you had to know what a harmonic integral was, and also a bit of sheaf theory. Thanks to Schwartz, I knew what a harmonic integral was, and we had had seminars on sheaf theory too. So, I could directly get into some high-level algebraic geometry without going through ideas such as valuations, complicated field extensions and so forth. This was a lucky break. At a later stage though, delving deeply into Grothendieckean algebraic geometry was absolutely essential to make progress in research in algebraic geometry.

> "Yet, amazingly, for three full hours, Grothendieck explained to me everything in detail

Coming back to Kodaira–Spencer: I knew some of the notions in those preprints. I had read it during the six months I was at the sanatorium, cover to cover. As I told you, it used PDE techniques mainly, and since I was familiar with elliptic PDEs thanks to my interactions with Schwartz, it was quite easy to read.

I must mention one amusing thing that happened during this time. There was one paper which was mentioned by Kodaira and Spencer which I didn't have access to. When I mentioned this to the people at the sanatorium, they got in touch with Grenoble University, and got me a photocopy of that paper. [Laughs] In some sense, the Kodaira–Spencer paper started off on the idea in this paper where it was proved that if the first cohomology of the tangent sheaf is zero, then there's no small deformation.

In fact, I also realized at that time that the idea they were using was that the direct image of a coherent analytic sheaf by a proper holomorphic map is coherent. I felt that this must be behind their work. So, after I came back to Paris from the sanatorium, I asked Seshadri: "Do you know if such theorems are true?". He said, "Have you not heard that the algebraic case was proved a year back by Grothendieck, and now Hans Grauert has just proved it in the analytic case?"

TIFR Archives

Armand Borel with C.S. Seshadri at the 1968 Algebraic Geometry Colloquium, TIFR.

I was very happy, to have at least discovered the theorem for myself, though I am sure I could not have proved it in the analytic case. I was really involved in this paper of Kodaira–Spencer. It gave you a way of constructing some manifolds, their deformations, which was all very useful in later work. That was a crucial input in my work.

So was this the beginning of your later work with Seshadri?

MSN: I didn't know then that it was going to be useful.

The main thing during my time in France was that I learned a lot from the already mentioned Kodaira–Spencer preprints. As far as my own work was concerned, it was the work with Kotake, which is quite well-known even today, which was the highlight. Among other things, we proved that some operators were indeed pseudo-differential operators, even before the terminology was there. I think the deeper result was a result which generalizes in a difficult way the following well-known fact: Any solution of an elliptic PDE with analytic coefficients is automatically analytic. Kotake and I proved a theorem characterizing real analytic functions via Cauchy-type inequalities satisfied with respect to powers of a linear elliptic operator with analytic coefficients. This is a somewhat hard theorem. I also wrote one or two other papers, such as one on deformations of complex structures on open Riemann surfaces; not a big deal, but at least I learned how to use Kodaira–Spencer techniques in a particular case.

Coming into contact with Kotake, and finishing one or two ideas I had (before leaving Bombay to Paris) in something to do with Riemann surfaces; and of course, getting familiar with the work of Kodaira–Spencer and deformations, were the big takeaways from my time in France.

While there, apart from mathematics, because I was in this sanatorium, I was exposed to people from different classes. From working-class people to high bourgeoisie, everybody there was sick. [Laughs] There was one working-class person and he was appearing for one of the Grandes Écoles examinations, just like our own IIT entrance exam. Once you go there, your career is made. I used to tutor him.

Was your French pretty good then?

MSN: I learnt it there. I took some classes initially, and in one or two months it was perfect. I used to give lessons to this young person appearing for an exam. And then happily, he passed that exam. For somebody from that class, to go to a completely different level of life, was not easy even in France. Soon afterwards he got married, and even asked me to be the best man in his wedding. [Laughs] That was his way of showing his gratitude.

I also learned to play bridge there. The sixteenth arrondissement in Paris is the habitat of high bourgeoisie class. I used to go and play bridge on holidays with pretty girls in the sixteenth arrondissement.

Anyway, here is where I understood the real problems of people from different classes. No matter what the individual backgrounds of people may have been, they all had their own problems to face. There was somebody who had a very aristocratic name, with a "de – something" in his name. He told me: "I'm actually a very ordinary and poor person. But because of my name which denotes aristocratic ancestry, people think that I'm very rich and don't want to deal with me."

For me all of this was very important. Going to Bombay from Madras already opens your eyes a bit, but from Bombay to France, well, the scales literally fall from your eyes, and you get a much bigger view of the world. When I got back to Bombay and immersed myself in research, I had the same set of mathematicians or physicists to interact with, and there was nothing different or varied. You tend to lose contact with the real world, in some sense. I feel that, because I fell sick, this opening up to different realities became possible. That was for me another big input in my overall development. Of course, I learned French too. I can now lecture very well in French.

You then moved back to India, to TIFR, and took up a professorship there.

MSN: Maybe a Readership. [Laughs]

Were you keen on taking students immediately, or did it happen over a period of time?

MSN: I was not keen. It just happened.

It's interesting that though the French influence on the School of Maths, and on KC too, was strong, the nomenclature of these positions was still Fellow, Reader, and so on. Very British!

MSN: Thanks to Bhabha! [Laughs] Nehru gave a lot of support to build up science, particularly to two people, Homi J. Bhabha and P.C. Mahalanobis. Of course, these were very good choices. But remember that Nehru was a Cambridge product, and the other two were also from Cambridge. [Laughs]

After you moved back to TIFR, your friendship with Seshadri took on

a more serious role in terms of the mathematics you jointly did, and your work with him resulted in the now famous and eponymous Narasimhan–Seshadri theorem. How did this joint work materialize, and could you please provide a precursor to this work?

MSN: In the early 1960s, both in topology and differential geometry, fiber bundles were very well understood, as explained in Norman Steenrod's classic book—which was incidentally presented to me by Balagangadharan. Then, in a small way, fiber bundles arrived in algebraic geometry too. Grothendieck proved a theorem on vector bundles on the projective line, which apparently goes back to George David Birkhoff. Seshadri also proved it independently, and André Weil had some remarks on vector bundles in algebraic geometry. Serre and Atiyah also had something on related aspects. But all of these were, I would say, then somewhat in a nascent stage still. It looked then as though the time was just ripe for one to go and develop the algebraic geometry of vector bundles. There is something else here too that is important. And here again we were very lucky, because in our student days in TIFR Bombay, even before going to Paris, K.G. Ramanathan had told us about the work of André Weil on the generalizations of Abelian functions. KGR had heard about that work from Carl Siegel. And then, we had a seminar on that. Seshadri gave some lectures on the paper. In this paper, Weil never mentions "vector bundles", but essentially he was trying to develop a theory of vector bundles, and moduli for vector bundles on compact Riemann surfaces.

Also, Weil himself says later that he was trying some method to construct it, but which was not fully correct. However, he makes two key remarks there. Here is the first: Suppose you have vector bundles that come from unitary representations of the fundamental group. Any two such bundles coming from unitary representations are isomorphic if and only if the representations are equivalent. It's not a very difficult result, although the proof that Weil had in mind was very complicated. The second remark he makes is that unitary representations should play an important role. I mean, it's just an off-the-cuff remark, a remark made "en passant".

After reading Kodaira and Spencer, we started thinking about it after our return to Bombay from Paris. Seshadri was already working then in algebraic geometry, and I was familiar with Kodaira–Spencer. But what we realized even before we proved anything was that a unitary representation was a very transcendental object. So you had to understand algebraic-geometrically what these bundles which came from unitary representations were. Unitary representations depend on the fundamental group. When you go to the fundamental group, you go to the universal covering, and you lose your algebraic geometry. And that's also the way one can construct moduli. So, we were looking for some algebraic characterization of unitary bundles. This was very clear in our mind. Also, we were sure that if only one could guess what it was, perhaps, it can even be proved, by the so-called continuity method of Henri Poincaré and Felix Klein. This is a method Poincaré and Klein had envisaged to prove the uniformization theorem for compact Riemann surfaces, which was later proved by some other methods.

So what does this method consist of? You have on the one hand unitary representations. On the other hand, you have these new objects called stable bundles. I should say that in 1962, in the ICM [International Congress of Mathematicians] held at Stockholm, David Mumford gave the definition of a stable vector bundle, and announced that he could construct the moduli space of such bundles as a quasi-projective variety. All that was needed for us, as it turned out, was the definition of stable bundles! [Laughs]

It's not very hard to prove that unitary representations (of the fundamental group) give a (semi-) stable bundle. Irreducible representations give stable bundles. Suppose you have two manifolds, where one is the space of (irreducible) unitary representations of a given degree, and the other is the space of stable bundles of degree zero. Suppose that the manifolds have the same dimension, and also that the second manifold is connected. There's a mapping from the first manifold to second, which is one-to-one and continuous. Our problem is to prove that this mapping is bijective. You can prove that the image is open, say by using Brouwer's theorem on invariance of the domain. But then, it is enough to show that the image is closed, because the second manifold is connected. For example, if the first is compact, clearly the image will be closed. But, here the trouble is that it's not compact—but this can be overcome.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David Mumford and André Weil at the 1968 Algebraic Geometry Colloquium, TIFR.

So, this was our general idea and approach. In one of our earlier papers, Seshadri and I had constructed the manifold of unitary representations, before we were even aware of the notion of stable bundles. Now, we had to construct a manifold of stable bundles. That is where Kodaira–Spencer theory helped us. Nowadays you can do it in other ways, but Kodaira–Spencer theory allowed us to construct it. But still, you have to have manifolds first.

Nithyanand Rao

A friendship that helped build bridges between disciplines—MSN with Divakaran, at MSN's home in Bengaluru.

And then Weil also made a remark on the unitary representations, right?

MSN: Yes, he said unitary representations should form an important role. As I said earlier it was sort of an "off-the-cuff" remark, a remark made "en passant". I don't think he was clear about the precise nature of its important role, because there was no suitable definition available on the other side.

I always say that you should be in touch with the highest level of mathematics which is going on everywhere, but you should also be a bit away from very big centres. When we ourselves started working on this, it was felt by some very influential and knowledgeable people that indecomposable bundles would form the correct moduli space, which turned out not to be true. In general, it helps to be aware of what is happening at the major centres of the world, but it is also important to maintain some distance, so as not to be adversely impacted by the dominant personalities and approaches of these leaders. A young person may be even dissuaded from pursuing a promising line of approach, only to discover later that it would have yielded rich dividends, if only one had ignored such advice and forged ahead. I cite this instance also as an illustration of a working philosophy that people, especially young researchers, must be aware of.

So, in some sense, the fact that we were in the Tata Institute, in touch with what was going on in the big centres, yet a little away from very strong personalities, was very useful I think. On the other hand, you have to know what's going on, and cannot isolate yourself, as very few people can work in complete isolation.

It's a balance between the two.

This work that you did with Prof. Seshadri went on to impact subjects like string theory which were not even on the radar in the 1960s. String theory came into its own much later, thanks to the work of people like Michael Green and John Schwarz, among others. But today the mathematics of string theory is a big thriving subject. Could you please elaborate on this unexpectedly fertile connection—one that perhaps you were also not aware of then, but which became apparent only later?

MSN: To be more precise, this is related to gauge theory and conformal field theory. Gauge theory is essentially the theory of connections on fiber bundles. Incidentally, my first work with Sundararaman Ramanan was on universal connections. When I say unitary bundles, you can replace it by a flat unitary connection. So, you can forget the fundamental group representations and so on, and it becomes a flat connection with $\mathrm{U}(n)$ gauge group. So we're taking this differential geometric object, and then interpreting it as a purely algebraic geometric object. It's a result which intertwines algebraic geometry and differential geometry in a non-trivial way. Each has its advantages. For example, with unitary representations, the moduli space is clearly compact—which by the way is not so easy to see from the algebraic viewpoint. On the other hand, if you started off from the algebraic viewpoint, some other properties of the moduli spaces can be proved. Ideas from conformal field theory are also related to that, but is a little more involved to explain. It is related to some linear systems on the moduli spaces of vector bundles.

As you said, I didn't know anything about these things at the time. But I found out that some very good physicists were using some of the results that I had proved with Seshadri and Ramanan. So I got curious to know.

MSN: It turned out lucky for us.

T.R. Ramadas

(From right to left) T.R. Ramadas, MSN and David Mumford

When exactly did you first find out?

MSN: Maybe around the time gauge theory became popular, around early 1970s maybe.

It was initially very difficult to get into that, for me. And luckily, I was friendly with the theoretical physicist P.P. Divakaran, as we all used to play bridge together. Slowly, I started asking him what gauge theory was, from the physicist's point of view. Communication between a mathematician and a physicist was very hard, for the first few months. Slowly, we somehow gained some understanding.

Bridge helped you bridge the gap! [Laughs]

MSN: The bridge is certainly there, but it's not completely solid. I just hope we don't tumble down.

This is a corollary to your work done with Prof. Seshadri, in 1965. In the late 1980s and the early '90s, Nigel Hitchin and Carlos Simpson built on your ideas from 1965, and formalized what are today called Higgs fields and Higgs bundles,* respectively. In 2013, nearly half a century after your joint work with Seshadri, Sir Peter Higgs was awarded the 2013 Nobel Prize in Physics for his contributions to the idea of the Higgs boson. An idea that originated from the purest of mathematical traditions, half a century later, turned out to be a bridge that connects the world of mathematics to the Standard Model of particle physics. Your thoughts on this.

MSN: I am really no expert at all on the physics, and on the link between the Standard Model of particle physics and the ideas of Higgs. But still, my own feeling is that Hitchin really came to this idea, as far as I can see, not from the ideas of Higgs which were on the mechanism by which particles acquire mass, but by actually looking at the Yang–Mills equations in four dimensions. He did what is called a dimension reduction on the Yang–Mills, reducing its dimensions from four to two.

Furthermore, Hitchin observed that this reduced equation, or set of equations, although living on the plane, was in addition even conformally invariant; and therefore also made sense on Riemann surfaces. Thanks to this reduction, Hitchin obtained a bundle; plus, another object that resembled the so-called Higgs field, though not exactly so. The challenge was to interpret these new objects algebraic-geometrically. My own work with Seshadri in 1965 was on unitary representations, but Hitchin did it here for any GL(n). Just as Seshadri and I had earlier used the definition of the stability of a bundle à la Mumford, an analogous notion of stability had to be understood here too, as there was also the presence of an extra field, the Higgs field.

Actually, the physicists define a Higgs field slightly differently, and it involves the idea of the section of a certain bundle; but by and large, this is how the idea evolved. In essence, as far as I know, the work of Hitchin was not influenced by the work of Peter Higgs. My own feeling is that he looked at a particular object that he had obtained from the cornucopia available after the dimension reduction of the four-dimensional Yang–Mills, and since it looked like a Higgs field, he simply called it one. At the same time, I am simply unaware of how these purely mathematical developments have had any bearing on our understanding of the actual physics of the generation of mass as seen in real experiments; which is anyway for a physicist to explain.

But speaking of related developments within mathematics itself, the above set of ideas have had some surprisingly unexpected consequences. Ngo Bao Chau, the 2010 Fields Medalist, used some of these ideas in number theory while working within the ambit of the so-called Fundamental Lemma of the Langlands program. It was important to construct a certain moduli space there too, and this in particular was first originally done by Nitin Nitsure; and was later generalized by Carlos Simpson. So, in essence, the main influence of Hitchin's work, as far as I can see, has been in the above set of ideas in mathematics that I just mentioned.

This connection with the Langlands program is seen to occur again, but this time via your work on spectral curves and theta functions. Do you see these occurrences as another example of what Michael Atiyah points to, as the "unity of mathematics"?

MSN: I do not know [laughs], as these are really very big words. I am wary of using this phrase which is often used without adequate justification.

A system of Indian philosophy believes that there is a unique metaphysical "reality", a "universal principle," a single binding unity behind all the observed diversity. Similarly, some mathematicians too seem to believe that there is a "mathematical unity" from which all mathematical facts emerge and flow out. I do not subscribe to this view. At the same time, I should say that the emergence of hidden and unexpected connections is a very important thing, and I am not at all, even for a moment, underestimating its importance.

In fact, personally for me, it is this very inter-connectedness that is most

* Steven B. Bradlow, et al. "What is a Higgs Bundle?". *Notices of the AMS*. Sep 2007. **54**(8): 980–981

interesting, because I am really not interested in proving some theorems scattered randomly here and there. The fact that this inter-connectedness makes our understanding of themes and ideas better than if they were viewed in isolation, is what makes these links and conceptual bridges invaluable. I have personally seen that methods that apply in one field of study are surprisingly seen to apply and work in wholly new and unexpected areas, forging conceptual links between what previously were thought to be separate areas of study; and thus, potentially enriching both.

Mohan Narasimhan

Reminiscing on an old friendship, over a game of Bridge.

So, in summary, while I think that "unity" is perhaps too strong a word, I also deeply appreciate the inter-connectedness of ideas. The examples you stated aptly illustrate this inter-connectedness.

In 1970, three Indian mathematicians, all from TIFR, were invited speakers at the International Congress of Mathematicians held at Nice, France. It was a one-of-a-kind achievement for Indian mathematics. How did it feel to be part of that group?

MSN: Nothing special, honestly. We were happy of course that we were invited. Maybe the full import didn't really dawn on us at the time. Of course, if you see it from another point of view, there was nothing like this before, and all of a sudden this happens.

At that time, our country didn't have much financial resources. It was very difficult to get travel funds for three people, and find money for hotel accommodation. [Laughs] For travel, I think we found some money, and then we wrote to Jean Dieudonné, who was in charge of the organization, requesting him to give us some place to stay. [Laughs] Now we routinely send 30–40 people to ICM. It's good, and I'm not complaining!

You've had many students and many of them have gone on to become internationally renowned mathematicians—M.S. Raghunathan, S. Ramanan, V.K. Patodi, R. Parthasarathy, S. Kumaresan, T.R. Ramadas, to name a few. But surely the story of Vijay Kumar Patodi stands out from the rest. One, because of how tragically short his life was, and also because how productive it was in such a short time. Unfortunately, not many people are aware of the nature of the work that he did, much less your role in it. Since it is generally acknowledged in mathematical circles that the Index theorem is one of the high points of 20th century mathematics, and since Patodi himself had a rather central role to play in this field, we would like to know from you what it is that Patodi achieved, and how it all started off.

MSN: For example, let's look at the special case of the Gauss–Bonnet theorem giving the Euler characteristic, which comes from the de Rham complex. Suppose that you'd like to prove the Gauss–Bonnet formula, integrating the Pfaffian of the curvature of the Riemannian manifold. There are many proofs available to do this. One way would be to use the heat equation, and find a formula for the so-called "index". But this formula actually gives n derivatives of the metric, while curvature involves only two derivatives. So, you now hope that somehow miraculously the other terms, that is, the higher-order terms, cancel out. This was conjectured by Henry McKean and Isadore Singer. Patodi first proved this conjecture.

The problem was that at that time, when this work of Patodi was sent for publication, the referees said that while it looked correct, they were also not completely convinced of the method. Patodi clearly had a proof, but it was so complicated that it was not easy to follow. It certainly had to be fixed. So, Ramanan and I asked Patodi to give us some lectures on his approach, so that we could find out whether the proof was okay or not; and if it was not, to see how to go about modifying it. Patodi gave us the first lecture, and we just didn't understand anything. In the second lecture, he put things in a different way, but it still did not help clarify things.

Then, some kind of a pleasant accident happened. Ramanan and I used to work very hard those days. And we used to go for lunch to a restaurant outside the TIFR campus, in Bombay. One day, we were having lunch and I said to Ramanan, "What is Patodi really saying?". Then, we both had an idea of what it should mean. We then formulated and proved some purely algebraic lemmas, which are called "supersymmetric lemmas" nowadays. We gave this set of lemmas to Patodi, and told him that this is what must be behind his calculations. Of course, he understood it, and in just a few days from then he wrote up the paper and it was published. But, mind you, this was only for a very special case.

The next obvious thing that one would want to prove, with the same method, was the Hirzebruch–Riemann–Roch Theorem. Naturally, he looked at it, but then the problem was that he was using a general Hermitian metric on the complex manifold, and in this case the cancellations just didn't seem to occur. So, he came one day to me and discussed things with me. He didn't know the definition of a Kähler manifold at that time. I just gave him the definition of a Kähler metric and said, "Why don't you look at the case of a Kähler manifold? Probably in this case it may actually work." After one week, I still remember vividly, he came grinning—"It cancels, sir!"

By the way, all of this is recorded in the *Collected Papers of V.K. Patodi*, edited by Atiyah and myself. And we have also written a small preface there. I have written about the things that I just mentioned, and Atiyah has written about Patodi's time when he went to work with him. This clearly brings out that it's not one of those stories which even today some people bring up. They somehow seem to suggest that this was another Ramanujan who was never discovered in India, and had no one to talk to here. But it's simply not quite so.

TIFR Archives

(From right to left): MSN, B.V. Sreekantan and Michael Atiyah

The great capacity of Patodi is not so much the analysis. When it came to hugely complicated expressions which you had to deal with, and discern a certain hidden pattern in them and extract deep consequences, well, that's a different kind of talent altogether. That's what he had. Later, I felt that he should go and work with Atiyah. But Atiyah was just then moving to IAS, Princeton. Then I met Borel in 1970, at the Nice conference. Patodi's paper had still not been published at that time, but he had already written up the correct version. I told Borel, "I heard Atiyah is coming to IAS. It will be very good for this young person to go and work with him." Borel said, "Send me his papers." In a short time, Borel himself communicated to me, "Yes, he can come to IAS now."

Among your other students, Raghunathan went on to work in algebraic groups, and Ramanan in differential geometry. Ramadas who was trained as an engineer in IIT Kanpur first joined the School of Physics in TIFR, but then switched over and worked with you in the School of Math. All of this seems to reflect the highly versatile range of your own interests, of being open to new things, and also being able to identify problems where you could contribute quickly and at depth, even in subjects very new to you.

MSN: For some years, after my work with Seshadri, I didn't even realize that I was working in algebraic geometry, although everybody thought it was so. I was interested in different things. Whatever interested me, I used to work on it. By the way, the work of Ramanan's thesis, and M.S. Raghunathan's too, was indirectly suggested by my own work with Seshadri. So indeed, was the subject of R. Parthasarathy's thesis on discrete series and Dirac operators, which occurred to me on the basis of my work with Kiyosato Okamoto.

Ramanan has written an article about me, where I think he has more or less captured the correct picture. He says that I always tell people that one must work "off the top". That is, one should learn any subject from as sophisticated a point of view as one is capable of; so that in some sense, from then on, you can move down into the details. Secondly, Ramanan remarks that: "He would quickly get a working idea of the problem and could think creatively without worrying about the foundational aspects at first. One filled that knowledge later." But this approach works well only if you're simultaneously doing different fields; and for me personally, this is what is most essentially interesting in mathematics.

When you say view from the top, do you mean seeing connections between different fields?

MSN: No, I don't even mean that. I don't prescribe going canonically from definitions, theorems and so on to start with. You have to eventually do it anyway, at some stage. But then the problem is that you can get lost in the minutiae without any idea of what it is all adding up to. As Eilenberg once said, everybody in mathematics has a "natural boundary". What he meant was that up to a certain level, you can understand things. Beyond that, it becomes very difficult to do so, even in a particular field. When you have to cross this boundary, how do you do it? Well, you could just get discouraged and simply give up, which is not at all what I am talking about. You could also say "I'll go to some classic textbook and read linearly word by word, the definitions, and so on." On the other hand, as in a metaphor, suppose that you can see and identify a few small holes in a large and otherwise mostly opaque structure. And through which some scattered bits of understanding are filtering through. This affords you some view into the world beyond, though only slightly and incrementally better than before. Now, you try to get ahead by using these little toeholds of incremental understanding that has filtered through the small openings. It is essentially that sort of a thing. For this, you clearly need some sophistication. Also, even here, you can proceed like this only up to a certain extent, decided by whatever sophistication you're individually capable of. If you try to do this exercise in as many different and creative ways as you are capable of, then hopefully you can get a bigger and a more concrete idea; and eventually understand more than what you initially started off with. Finally, of course, you have to sit down and get all the minute details thrashed out. No escaping that.

Wonderfully put. Moving on, you have mentioned before that your mathematical hero in some sense is Hermann Weyl. Can you tell us why?

MSN: I should add one more thing, which I do not mention very often. My other mathematical hero is Kodaira. It is Kodaira's work, after Schwartz's lectures, that gave me an entry into this world of ideas. Beautiful, very deep, original, not too difficult, with one of his great papers stretching for not more than just 20 pages. Absolutely deep work. That's my other hero, Kunihiko Kodaira.

And, of course, Henri Poincaré!

But coming back to your question, I think there are two or three reasons.

The first book I read with Seshadri when we had seminars together as students, was Weyl's book on Riemann surfaces. There was no English translation then, and we had to read it in German. That's the first real book there is on differential analysis. It is the first place where a manifold is defined, a complex manifold is defined, a differential equation is defined, differential equations on manifolds are solved, and complex geometry is studied. And it was not even his main work, as all that he wanted to do was to give a course on the topic. He was just a 25-year-old lad, and then he publishes it. It's a great book, *The Concept of a Riemann Surface*.

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The "fab four", in 2004. From left to right: M.S. Narasimhan, C.S. Seshadri, S. Ramanan, M.S. Raghunathan.

I read his papers too. He had three or four papers on representation theory of semi-simple Lie algebras. Again, at that time I read it only in German. There are many other interesting things that you find when you look at his collected papers. You can turn almost any page and you find something interesting, in different aspects of mathematics. At that time I didn't realize it, but he had such a deep understanding of physics. He wrote books on quantum mechanics and relativity. I don't think there's anybody now who knows both mathematics and physics so deeply, and who can write with such authority on both. I would say that most of his work is inspirational. I did use his representation theory later, and when you read it you know you are…

…in the presence of a master?

MSN: Not just a master. In the presence of great things, too.

People working in Diophantine approximation tried to generalize a particular idea. But Weyl has just a 15-page paper on it, where everything becomes clear. I just love his idea of using Fourier series to say something significant about the so-called characteristic function. I mean, this is the kind of simplified idea which gives very deep results. Though I am not particularly interested in that field, it's a pleasure to read that paper. I have with me Weyl's collected volumes, all four of them, presented to me by Anandaswarup Gadde.

Were stories of Hermann Weyl[*] told to you by KC?

MSN: Yeah, I'll tell you one or two. Minakshisundaram was once talking to me and Seshadri, and he said, "You people should have met Hermann Weyl."

KC was Weyl's research assistant in Princeton. So he had very close contact with him. There was a very interesting story that KC once told me. Once when KC was arranging Weyl's reprints, he found a set of papers by Weyl in reprint form. It occurred to him too that these had never been published. So he asked Weyl: "I don't seem to have seen this paper published."

And the story is the following. Weyl had written this paper and sent it for publication, perhaps to either *Mathematische Annalen*, or some other journal that I do not now remember precisely. Then he found out that a young person called Alfred Haar had written a paper on the same topic and ideas, and had even sent him a pre-print. Yes, Alfred Haar, before he became famous for his measure. Weyl wanted to be nice to this young researcher, and wanted to withdraw his paper so that the young man could publish his. But that's not the entire story. This still does not explain the presence of the reprints. Apparently, Weyl had already sent his paper to the said journal, and he wrote to them saying that he would like to withdraw his paper. The journal's editors agreed that Weyl could indeed withdraw the paper, except that since they had already printed many copies of the paper, he had to pay for the already printed copies. Weyl paid for them and the journal sent him all the reprints! This is one of the stories which KC told me. Not many people may know this.

See, some of the great mathematicians become bitter a little later, when they find the younger generation doing their own things. Somehow they cannot take it. There's a Fields Medal elocution of ICM 1954. Weyl was the chairman of the medal committee when both Kodaira and Serre won the Fields in 1954. And he says that the younger generation imposes more and more concepts on his generation, and that it was all very difficult for them. And immediately, within just half-a-page, he actually goes on to explain beautifully what a sheaf is. You can see the broad-mindedness and generosity of that gesture. Here are his exact words.

"If I omitted essential parts or misrepresented others, I ask for your pardon, Dr. Serre and Dr. Kodaira; it is not easy for an older man to follow your striding paces. Dear Kodaira: Your work has more than one connection with what I tried to do in my younger years; but you reached heights of which I never dreamt. Since you came to Princeton in 1949 it has been one of the greatest joys of my life to watch your mathematical development. I have no such close personal relation to you, Dr. Serre, and your research; but let me say this that never before have I witnessed such a brilliant ascension of a star in the mathematical sky as yours. The mathematical community is proud of the work you both have done. It shows that the old gnarled tree of mathematics is still full of sap and life. Carry on as you began!"

Beautiful!

MSN: His whole personality, along with his mathematics, is probably why I see him as a hero.

We want to briefly touch upon here a debate that happened in the 1990s. This was just after Edward Witten was awarded the Fields Medal in 1990. The mathematical community was, in some sense,

[*] See the article in this issue of *Bhāvanā*: "Hermann Weyl: Itinerary of a Scientific and Intellectual Career" by Christophe Eckes.

in two opposing camps; one of which was broadly represented by Atiyah; and other by the logician William Quine, the mathematical physicist Arthur Jaffe, and even Saunders MacLane. People in both the camps admitted that the very nature of mathematical proof itself had changed, courtesy the work of Edward Witten. And, MacLane is supposed to have said that despite the physicists, mathematicians still need a proof. Your impressions on this event.

MSN: I suppose you are also alluding to the old joke, that in Kyoto, there were three quantum Fields Medals and one classical Fields Medal. This episode was, in my own view, some kind of a storm in a teacup. I mean, it disappeared soon. In my view, the point is that the physicists came with some insights into topics that were of interest to mathematicians. These insights also let the mathematicians make progress; and consequent mathematical insights were also later fed back into physics. I myself understood them, but I took some time. But then it's anyway the job of the mathematicians to prove theorems. So what's the big deal? The real trouble was: What is a proof? If somebody says something new, should we take it for granted? That was the real issue there. Also, how are you then going to be sure of the proof? The physicist may say "We have the intuition." Okay, that's fine. I'd go further and say that what the mathematician should try to understand are those inputs from the physics school of thought that originally gave rise to these interesting things. It is very hard, and I have really tried to learn these things myself. Quantum field theory especially is not easy for mathematicians to get. Physicists write some Feynman integral, and pull out a cornucopia, and so on. It is apt here to recall André Weil, who once said, "Rigor is to mathematics, what morality is for humankind." [Laughs].

In my own case, physicists have used my results. I, too, through my own papers, have tried to understand what they have discovered, and I have tried to prove those results rigorously. On one such paper, the referee remarked, "This paper provides a rigorous proof of a result that is very important for mathematics and mathematical physics. The physicists apparently consider the result as obvious!" [Laughs] We had to write so much on something that was simply and plainly obvious to the physics community! The real problem, as people say, is the fight between proofs with a small "p" and proofs with a capital "P". I also think that the best comment on this aspect of proofs was apparently in a lecture by Gerd Faltings, where he said, "There will be no proofs with small 'p' in my lecture. They will always be with the capital 'P'". I think that more or less summarizes the issue.

What is meant by a proof with a small "p"?

MSN: Some idea may or may not be fully rigorous, not even partially so. I've seen many people use the phrase "we argue". It's even become standard terminology nowadays, and is a kind of argument that's neither completely empty, nor foolproof. Certainly not fully rigorous. These things qualify to be called small "p". Capital "P" on the other hand refers to proofs where everything is fine. Everybody knows that in mathematics—and Weyl also says this—it's not the proof that's the sole point of focus; "You should discover". This is said by all great mathematicians.

But the same people will also say that you have to prove it. Some mathematicians would have probably said that one had to prove things not just once, but twice; and both times, as rigorously as possible. [Laughs] There are good examples of how a physicist's intuition, unless backed by a rigorous analysis and proof, may actually be wrong.

So, in summary, I think this is not a big issue. After all, it is true that at a certain stage, some of the inputs from physics have been useful. But they were not consistently the greatest ideas or inputs into the body of mathematical knowledge. After all, the internal dynamics of the mathematical community, say the proofs of the Poincaré conjecture, or Fermat's theorem, have nothing at all to do with physics and have contributed to great progress in mathematics. In the same breath, I'm not at all undervaluing some inputs from physics.

> "There will be no proofs with small 'p' in my lecture

When Harish-Chandra mentioned this to Dirac, Dirac supposedly told him that "I'm not interested in proofs, but only in what nature does." In knot theory, in what are called Tait's conjectures, a collaborator of the physicist Lord Kelvin by the name of Peter Guthrie Tait made some observations that eventually came to bear his name; and here Tait was then primarily responding to Kelvin's flawed idea that atoms were knotted vortex tubes of ether.

MSN: Yes, ether of course turned out to be wrong!

Yes. But Tait, like a mathematician, made observations that came to be known much later as Tait's conjectures, and which eventually become a mathematically more rigorous area. It contributed to the body of mathematical knowledge in a completely unexpected and unintended way.

MSN: It was not useful in physics in its original formulation, but evolved to become a theory in mathematics. Of course, Witten had a very nice way of looking at the Jones polynomial. The amazing thing to me really is how Witten could even think of that. But what is also equally important here is why physicists still cannot provide or construct a rigorous proof. That's the question.

So in summary, while you may

not still be completely convinced about an idea owing to lack of rigorous arguments, it also sometimes looks like it's okay. Its merits can be, as I said before, "argued".

Therefore, as an answer to your question, I don't think one should bother too much about this. If there's something you can understand from the physicists, and if it turns out to be interesting, and, further, if they haven't proved it, just go ahead and prove it, if you can.

Moving on to areas slightly removed from mathematics, but things that you have done over the past many decades. These are the administrative roles that you have shouldered. You were the dean of the School of Mathematics at TIFR and then you were the very first chairman of the National Board for Higher Mathematics. Later you moved on to head the mathematical division at the International Centre for Theoretical Physics. Then for a few years you were also at SISSA [The International School for Advanced Studies]. While doing all these things, your research output never diminished in any sense. How did you manage these things?

MSN: Before I answer the question, I should mention that KC was a fantastic person, who changed the mathematical landscape of India by his role in creating the School of Mathematics in TIFR, Bombay. Whatever administrative capacity I may have, I learnt it all from KC. It is not that he taught me directly how things should be done. I just picked it from him, simply by observing him.

Something like proof by example.

MSN: Yes. His stress on attention to details, and on how to deal with human behaviour. How to say a polite "No", while ensuring that you do not hurt the other person, is something that I learnt from him. Another thing which he told me which I still remember very well is the following. When I was the acting head for the first time he told me that when one does these things first, one is very enthusiastic. "You want to be efficient and to have a 'clean' table. However, especially if you have to write an important letter or say something in an official capacity, don't be in a hurry, and just wait for a day. Nothing great is going to happen in a day's time anyway." [Laughs]

I never tried to separate research and administrative work. If you have good secretaries, the job is even easier, and I had very good secretaries. Maybe if you have to raise funds as the president of a university, it's a different problem. But for me, it was never taxing.

You have been awarded many medals and prizes. It started with the Bhatnagar award, and then the high point was the Padma Bhushan. There are other equally prestigious awards too. You're a Fellow of the Royal Society, and have been awarded the King Faisal Prize, the Third World Academy of Sciences award, among many others. Among all these, what is the one that gave you the greatest satisfaction?

MSN: First of all, the honour that you receive from your country. The Bhatnagar award, to start with, and the Padma Bhushan are very important because you feel you are recognized and valued by your own community. Of course you're also pleased, there's no doubt. As for the King Faisal award, the remarkable thing here are the other people who have also received this award. They're all great mathematicians. So you feel humbled to be in that company.

Since you mentioned that an award from one's own country makes you feel a part of the community, that brings to mind something related. You once mentioned in a message to students that there's brain drain and you asked them to come back to India. Do you mean to say one has to give back to one's community?

MSN: Some people may say that these are all old-fashioned ideas. When we came to the mathematical scene in India, I entered it immediately after our Independence. The material resources were nothing. And luckily, in addition to some enlightened politicians and scientists, surprisingly, some wonderful IAS officers whose names you may not know also felt that science had to be supported, and actually did something concrete about it. Why I mention administrators here is that, for example, when I formed the National Board for Higher Mathematics, the person with whom I had to deal with in the Department of Atomic Energy was a joint secretary whose own background was in the humanities. But he got so involved in this new venture, and gave me such good advice, that it took off very well. In fact, even the name of NBHM comes from him.


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MSN felicitating Lalgudi Jayaraman

Let me in turn ask you this question. Why did people, who made a name for themselves while being a part of the Tata Institute, and who were reputed professors there, go abroad? Tata Institute could have gone much further ahead if they had stayed back and contributed.

One thought that I've had, and of course, it's not an answer, is that in the US academicians can retire whenever they want. Here in India, since there's a retirement age, that maybe a problem.

MSN: When I was young, I never bothered about retirement. I didn't even know there was a pension.

So, is a sense of idealism a necessary ingredient to build institutions?

MSN: Idealism is a word which is not so apt. At least, some of us felt that this poor country of ours has given us so much, and nothing at all to regret about. Also, I'm sure I could not have done any better work anywhere else in the world. Maybe I could have bought a few more books if I were in the US, which I could not afford here! So much is given to us, at least we should do something in return. I think it's a serious problem. If we want to reach the top, we have to do something about it.

Apart from mathematics, among what could be called your avocations, we hear that you're a big fan of detective novels. And that you're also fond of, perhaps due to your time in France, Western classical music and Carnatic music. Any favourites among authors and musicians?

MSN: No particular favourites. I like many of them. I'm quite conversant with Tamil literature. Some of our local Indian language literature is as good, if not much better, than the so-called big things you learn from English translations, let alone Indians who write in English. Personally, I feel some of the greatest writers I know are Tamil writers. I like to read them, and I can cite names, but that's not the point. I also read some French, too.

As for detective novels, you perhaps start in your younger days with Sherlock Holmes. And then maybe, according to some Indians, you graduate into Scandinavian novels. Another thing that's good in detective novels, and of course, it's true of other novels too, is that you get an ambience of different historical times, people and cultures. Some Agatha Christie novels give you an idea of upper class society. On the other hand, if you read Judge Dee novels, set in a not-very-ancient China, you get some idea of what the nature of Chinese society was at that time. Incidentally, this protagonist Judge Dee was created by Robert van Gulik, a Chinese scholar. Apparently, these are very old Chinese stories, and he kept telling writers that they should be translated to English. Nobody took him seriously, and so he himself adapted them into English, and became very rich. [Laughs]

You mentioned Tamil literature. Is there anything particular that you recall?

MSN: Well, there's a very good book on Ramanujan, fictionalized in Tamil, by Ragami. It's completely fictionalized, but what's interesting in it is that the language in which people speak in his book is exactly what Ramanujan would have actually spoken at his home during his time; which, by the way, is very hard to accurately get a measure of, especially in an English translation.

A rare glimpse into a particular aspect of an era, which could definitely get lost in translation!

MSN: Yes. You can almost imagine, reading Ragami, how they would have spoken. His mother apparently on the surface did not want him to go abroad, and she went to ask the deity on what needed to be done. But the whole point here is that she wanted him to go, and realized how important it was for his mathematical advancement.

Here is a strictly personal opinion. I don't think Ramanujan ever said that he got his ideas from the goddess of Namakkal. But everybody says he did. Anyway, it is nevertheless very interesting to hear what he would have actually said in Tamil.

For those who are contemplating a career in research, or are at the cusp of a major decision—or perhaps even for people newly into research—from your vast experience and insights, what words do you have?

MSN: First of all, you have to go through the usual mill, in the sense of reading something. But then that is at a certain lower level. When you want to go to a higher level, you should try to have a more global picture to start with, and then come back, and get all the nitty-gritty right. As I said, get yourself as sophisticated a point of view as you can muster. And of course, you should be lucky enough to have contact with good people. Here, I don't only mean professors. Your co-students and peers from whom you can learn by talking to. That's how you really learn. You cannot learn everything from books—you can, but it will take you ages. And of course, work hard. Be involved passionately, that's all I can say.

Prof. Narasimhan, thank you for your time. We wish you many more years of good health and continued mathematical creativity.

MSN: Thank you very much. ■