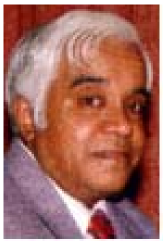
A conversation with Professor R N Iyengar



Professor R Narayana Iyengar has been with IISc since 1963 when he joined the Institute as an MSc (Engg) student. He is presently the KSIIDC Chair Professor in the Department of Civil Engineering and also a Professor in Centre for Atmospheric and Oceanic Sciences. He obtained his MSc (1966) and PhD (1970) from IISc. His PhD thesis on stochastic modeling of earthquake loads was the first doctoral thesis in the country in the area of random vibrations. He joined IISc as a faculty member in 1969 and became a Professor in 1986. His fields of expertise have encompassed the areas of structural dynamics, random vibrations, earthquake engineering, disaster mitigation, railway track dynamics, rainfall modeling and history of science. He has inspired several generations of students: he has guided 11 PhDs, 9 MSc (Engg) students and 13 ME projects. He has published nearly 90 papers in leading refereed journals. He is a Fellow of Indian Academy of Sciences, Indian National Academy of Engineering, and National Academy of Sciences. He was awarded the Alexander Von Humbolt Fellowship (Senior, 1978-80, 1992, 1997). He is the recipient of the M Visvesvaraya award for senior scientists for life time contributions to science and technology (Government of Karnataka, 1996). He has been a plenary speaker in the GAMM conference on applied mechanics -1992 and invited sectional lecturer during the ICTAM, 2000. He has been the director of CBRI, Roorkee (1994-2000). He has held visiting positions at the Purdue University, Brooklyn Polytechnic, New York and Distinguished Schimdt Visiting Chair, Florida Atlantic University (1995). He has lead field investigations after the Khilari (1993), Chamoli (1999) and Kutch (2001) earthquakes. He has been a consultant to various industries and R & D organizations in India including NPC, IGCAR, DRDO labs, RDSO, BHEL and Kerala State Government.

The following are the excerpts from a conversation that Professors B K Raghu Prasad, C S Manohar and D Roy had with Professor R N Iyengar on 4th June 2005. He talks of books that impressed him, the people who made impression on him, of grand problems in the study of earthquakes, how he views analytical tools *vis-à-vis* the numerical ones, on nature of risks involved in large engineering projects, and of ingredients for being recognized in research community.

To start with, tell us something about your early days: how did you get into research? You joined PWD after BE. How was higher education in civil engineering perceived at that time? What influences you had: individuals, books or any specific events?

I don't know where to start as my early days! I studied BE at NIE, Mysore. I disliked machinery and hence chose civil branch, more so due to the subject of mechanics, which was considered to be in the domain of civil engineering. I was selected, while in final year, to Neyveli Lignite Corporation and to HEC, Ranchi. A gazetted officer post in HEC was considered a prime posting for a fresh graduate but I was not interested. My teachers at NIE were all uniformly sincere and good but none was interested in any type of research. If someone went abroad, it was mostly due to the attraction of USA and not for research. But, Mysore city was full of scholars of various types and acquisition of knowledge was considered a goal by itself. One person at NIE who influenced me at that time was Ramamrutham. He did not teach me any course; but he stayed close to my house and that gave me an opportunity to see him once in a while. During these meetings he would ask: "do you know influence functions?" or some such things. He showed me how to find beam deflections under arbitrary loads by knowing the deflection under a concentrated load. This gave me a feeling that there are cleverer ways of approaching problems other than what I was exposed to in the class. Actually engineering research was unheard of except for the Mysore Engineering Research Station at KRS, which, during its heydays was a leading institution of its kind, but alas, not any more! I worked there for some time on daily wages, before getting a regular PWD posting as a

Junior Engineer in South Canara. Higher education in those days, particularly at IISc, was the monopoly of rank students and I was not one. So research was a distant dream for me; but I would not easily give up! While in college I came across the book "Theory of Structures" by Timoshenko and Young in the library. I felt attracted to it so much that I wanted to buy it! Fortunately, it was available in Mysore and my father bought the book for me paying 25 rupees- a huge sum in those days for an imported book. I found the methods described in the book, like Fourier series and reciprocal theorem, simply beautiful. Timoshenko has shown how the reciprocal theorem could be used for solving problems of continuous beams. I extended this to continuous arches and succeeded in impressing Prof Govinda Rao by getting my work published in the Journal of Institution of Engineers. This earned me admission to MSc (Engg) at IISc.

What prompted you to take up modeling of earthquake signals through stochastic processes as a subject of research during your Ph.D.? It appears nobody else was working on such aspects in India during those days (not even your research supervisor).

I cannot find any particular reason or inspiration. For Masters' my work was on vibration of beam and slab bridges modeled as orthotropic plates. My guide Prof K T S Iyengar suggested this problem to me. Those days were perhaps the initial days in structural dynamics research and focus was on understanding behavior of elastic systems. By then KTS had already done important work in the area of elasticity while he was in Germany. He had extensively used Fourier series in his work. Other kind of orthogonal functions come up naturally in vibration problems: the beam mode shapes for instance that satisfy different boundary conditions. Some of this I studied in my MSc thesis. For my PhD I did not want to continue with plate vibration. I dabbled with vibrations of continuous plates for some time. KTS also suggested me to work on vibration of curved bars and I formulated the problem using calculus of variations. This took about a week for me and I did not know how to proceed further. It was not at all exciting. Around this time Prof Jai Krishna visited IISc and delivered a few lectures on earthquake engineering. He had started a new school at Roorkee on earthquake engineering. I vaguely recollect, KTS was one of the founding members of Indian Society of Earthquake Technology (ISET) and had invited Jai Krishna to IISc. I remember that he talked about earthquake response of shear building models. I recollect someone asking a question on the divergence of the series for the base shear. This set me thinking. It seemed earthquake engineering was after all an extension of structural dynamics. I started working on earthquake response of structures like beams, arches and portal frames. I formulated the problem of a portal frame subjected to time varying boundary conditions and I could easily solve the problem. I could have gone on to do the numerical work and conduct parametric studies. But this looked quite uninspiring to me. Then I happened to read papers by Housner on random process models for earthquake loads and got interested into the subject. I also had access to the book on random vibration by Crandall. Sometime then I listened to a lecture in the aerospace department on the use of random process models in fluid mechanics problems and heard about Brownian motion etc. I started looking into random process models and it appealed to me. Davenport had just then published his paper on application of random vibrations to wind engineering problems. So, in dealing with earthquakes, questions of nonstationarity, peak statistics and response spectrum etc came up naturally. Once I started working on this there was no going back. My first paper on random process models for earthquake accelerograms appeared in 1969 in the Bulletin of Seismological Society of America and the ASCE Engineering Mechanics paper followed this.

When did the Koyna earthquake happen? How was this experience especially given that you were doing PhD on earthquake modeling?

It was in December 1967; a very interesting experience. Our director at that time was Professor Satish Dhawan. He was a very versatile man. During those days he used to meet every research student when the student joined the Institute and also subsequently he would visit the Departments and talk to every student individually. My first meeting with him in his office when I joined the Institute (in 1963) as a research student left a very strong impression on my mind. He had then asked me why I wished to do research? Could I do it at all (given especially that I was not a first class student in BE)? I had to tell him of my work on arches and my reading of Timoshenko's book and also that I had published a paper! He appeared to have been impressed. Later, (in 1967) during his visit to the Department he saw me manually digitizing recorded earthquake time histories from Housner's Caltech reports. I was using a lens and had drawn a set of straight lines and trying to read off the acceleration values. He commented caustically on the accuracy of such an approach and suggested to KTS to write to Housner and get the digitized data from him. Dhawan had studied at Caltech and probably the fact that the report that was on my table was from Caltech had caught his eye. He also asked me why I was working on earthquake problems. Was South India prone to earthquakes, – was it not a stable region?. He appeared to have been not so impressed. Shortly after this meeting, the Koyna earthquake occurred. I happened to run into him near the tower building and he called me by name and asked if I had looked into the details of Koyna earthquake. He was smiling. He appeared impressed that some work was getting done at IISc on earthquakes. My knowledge on seismology at that time was meager and I could only give a vague reply! I was preoccupied more with the wiggles of earthquake time histories that I was simulating. However, his question sent me thinking. In those days our computer work had to be carried out at TIFR, Bombay. On one of these trips I visited CWPRS, Pune, to collect the strong motion record of the event. I could simulate the sample using my model, once the rate of zero crossings was increased to a high value of about 25-30.

You are widely traveled. You visited leading Universities in the West after your PhD. Did you ever think of a career in these Universities? What motivated you to work at IISc?

In fact I was not planning to join IISc at all! It was a hesitant application for the lecturer's post that pushed me into the Institute. Satish Dhawan, the then Director, was expanding the Institute. He was somehow impressed with my work and me. I was offered a visiting position at Purdue University at the Center for Applied Stochastics, in 1969 based on a copy of my thesis, which had not yet been awarded the degree. The Department Chairman asked me to resign and in fact I had given him in writing my intention to resign from the Lecturer's position! But, Dhawan, who heard about this, called me and advised me not to quit without having seen USA. He definitely encouraged me to visit USA, but suggested that I should keep my option open by availing extraordinary leave. Well, he was from Caltech, he knew USA very well and he was working in India. After spending a year in USA, I decided to return to India and I have not regretted my decision.

You have seen the growth of the subject of random vibrations almost from its infancy. How do you feel about the way the subject has shaped up? What do you see as success stories? Has it succeeded in the field of earthquake engineering? The subject has not caught up with the industry-certainly not to the extent of being commensurate with the research efforts that have gone into this field. Curiously there has been never a journal of random vibrations so to speak. Was the later growth of the subject consistent with your expectations when you started with the subject?

I had no expectation with the subject of random vibration when I started with it. It was the challenges of the subject that I liked. The books that inspired me at that time were from electrical communication and general books on random processes. I have to become a bit philosophical! I think life is not deterministic; nature is not deterministic; that you can see the same things in different ways. This only means life is random. This in fact is the catch phrase in the book on Markov processes by Bharucha Reid. He uses the Sanskrit quotation from the Jain philosophy called *Anekantavada*. The quote reads "*syadasti naasti ca avaktavyasca*" (may be, it is, it is not and also indeterminate). According to this philosophy, anything in this world can exist in six different modes. These modes are not like the normal modes of vibration but like the concept of states used in probability. And, the other book that impressed me deeply was the book on statistical theory of communication by Middleton. It is a huge book that is wonderfully well written. Middleton in this book quotes from Bhgavadgita:

नष्टो मोहः स्मृतिर्लब्धा त्वत्प्रसादान्मयाच्युत ॥
स्थितोऽस्मि गतसन्देहः करिष्ये वचनं तव ॥ ७६ ॥

(My stupor is gone. I have recovered my faculties through your grace.
Devoid of all doubts I shall carry out your words)

Maybe the author derived inspiration from Bhagvadgita and wrote that big book. This impressed me beyond measure that there are people who look at things in unique ways and just for the heck of it they are going to do it throughout their lives! He took a cue from a book that is so important for me and for my culture. The same spirit guided me: whatever happens, I am going to work with the subject! That was the end of the story- I had no expectations. Also, I received encouragement from Prof C V Joga Rao to continue with random vibrations. My first PhD student P K Dash, was from aero-space department with CVJ as the co-guide. Joga Rao would also attend my lectures on RV and ask inconvenient questions! His support was a morale booster for me. As you may know I had one more student V J Sundaram from AE Department with Prof A K Rao as the co-guide. Sundaram came with a well-posed problem of life estimation of solid propellant grains under transportation. This was an interesting problem of random vibration of a visco-elastic system.

You are asking about success of random vibrations in earthquake engineering. I have always seen random vibrations as a part of Mechanics, wherein the parameters are uncertain and can even be random processes. Scope of stochastic mechanics is wide and vast. Why ask about random vibration and its success in earthquake engineering? There are academics who still swear by static analysis as being sufficient for successful earthquake engineering. These are well meaning persons whose vision of the subject does not go beyond design codes and thumb rules. Even now papers get published in which equivalent static analysis is done for earthquake response analysis. Do you call it as success? I look at your question in a slightly different way. Has the concept of probability caught up in earthquake engineering? The answer is a definite yes. No hazard map is worthwhile unless the return period or probability of exceedance in a time window is given. IBC-2000 has adopted this approach for finding design forces on structures. If you see the concept here, it is same as the first passage probability in random vibration. It is another matter that writers of Indian code (IS 1893) are backward in their thinking and follow the Americans after a time lag!

Your interpretation of this hazard map etc. is on specifying earthquakes. But quite a bit of work has been done on response analysis, nonlinear structural response analysis, hysteretic system, on which you have worked too. But at the end of all that people still seem to use response spectrum based method, even for multi-support excitation. Even for non-linear problems, the response spectrum is somehow modified and one manages to live with that. The whole subject of random vibration is now well developed and readily available on a platter. For instance simulation-based methods are now widely available. Profession is not accepting them or not even willing to consider using these tools. This was what the tone behind our question was.

No! My response is different. If you look at profession as only IS 1893, that may not be correct. You have to see the subject, particularly when we think of ourselves not just as teachers but as research workers, from an international perspective. At least I would do so. For example, simulation is suggested, as you yourself very well know, for nuclear reactors, wherever it is needed. Spectrum compatible accelerograms have to be used. That is a way of random process simulation. Then, ICOLD recommends two levels of earthquakes for large dams. These may not be in terms of our language of probability, but instead in terms of risk levels. For higher-level earthquakes simulation is indeed recommended. On building analysis and design there is a perception that one may not like to spend much time and maybe codal provisions are sufficient. Even here the

International standards (for example, DOE-STD-1020-2002, USA) do specify different exceedance probability levels.

Again on the same thing. After the recent 9/11 WTC failure, and Columbia shuttle failure, there are questions on if this kind of failures can be modeled.. Do you see a role or any conflict in usage of probabilistic models, in this kind of situations?

My answer is that probability is a way of envisioning what may happen: therefore, it is obviously limited by the power of envisioning that one has. In nuclear power plant codes one talk of 10000 years and questions on how to define active faults over this period are raised. How one should determine the SSE level earthquake? That is envisioning. Why only 10000 years and why not more? That becomes a social question. There is a scale involved when it comes to probability. For residential structures (beams, lintels, slabs etc.,) one can be content with a factor of safety approach. For large-scale structures one has to handle things in a different way. For instance there were discussions in nuclear engineering community, long back during the 1985 Brussels SMiRT, on safety of NPP structures against aircraft impact: whether that should be included or not in design. In fact a few years back, I had a discussion with Mr V Ramachandran of NPC on pattern of air-traffic at Mumbai airport and safety of NPP structures due to aircraft impact. They wanted to know, if, say by accident, an aircraft lands on a reactor what would happen. Obviously someone had envisioned this possibility. Therefore I would think, if you can envision possibilities, the theory of probability would be a powerful method to arrive at meaningful conclusions, further leading to enlightened decision making.

What you think of other aspects of modeling like the one based on fuzzy logic, interval algebra, convex model etc?

I did not work in the area of fuzzy logic. I read a little bit on this topic but somehow the subject did not impress me. The theory of probability, as I had studied and as I had understood, had a great hold on me and it seemed sufficient to answer so many questions.

There would be problems with application of probability as the dimension of the problem increases. Say, the size of the integral to be evaluated increases. Do you see this as a problem?

Yes, that is a problem. Multidimensional integrals are where we often grapple with when it comes to probabilities. Then, how do you overcome that? You change the problem formulation and you can solve some problems. But I don't think you have solved the basic problems.

As far as the use of probability for these kinds of things is concerned, you mean?

No! See, it is like this: suppose I have to handle an N-dimensional problem, which I believe to be probabilistic, and it cannot be modeled in any other manner. I have to simply say it is a multidimensional integral over X_1, X_2 .. etc.. You struggle with it, simplify it or approximate it; this is one approach. Another approach is: you change the formulation somewhere and use it through, maybe a fault tree analysis, or something else, where this kind of difficulties does not arise. To me it seems it is equivalent to making some assumptions on my probability theory (may be somewhere assumptions of independence or decomposability or something similar). It may not lead to exact answers but credible answers could be obtained. It seems to me that here the same probability theory would be coming in another garb. I must say, I haven't looked into it philosophically or mathematically in great detail.

There would be another problem that one may have to reckon with even if the problem of multi-dimensional integration is overcome. This has to do with lack of data needed to construct meaningful multi-dimensional non-Gaussian probability densities.

How do you overcome that in other methods? This is a basic problem that would still remain.

You seem to have a flair for inverse problems: you have worked on critical excitation modeling, *system identification, earthquake source modeling using observed strong motion displacements, and, dating of Mahabharath war based on reported occurrence of eclipses etc. What fascinates you about these problems? There seem to be a detective lurking in your person!*

I don't know if I could answer this question! I have been interested in inverse problems for rather peculiar reasons. I will give an outlandish example. People do magic or chant mantras expecting that it would thereby rain. I have thought about these things deeply because these are part and parcel of our culture. I have analyzed them in some other manner. The theory behind this goes back to a subject that I studied in my Sanskrit college days, namely, the subject of *Tarka* or logic. In logic, you start with a cause and you have an effect. If you start with an effect, the cause could be non-unique. This is discussed in *Tarka* in great detail. I was imbibed in my younger days with this kind of arguments. In that sense when I look at some of the religious practices, what they try to do is to induce some of the effect assuming that thereby the cause is strengthened. The hope is that any small action we take (like pouring water on an idol) might lead to small perturbations and that may lead to further effects. This may be wishful thinking but, this concept has impressed me. This is something that has some vague connection with my interest in inverse problems. But the dating of Mahabahrath I studied for altogether different reasons. As you know I have collected information about earthquakes in ancient India. Mahabharath mentions earthquakes in a few places. How to date these? This was the motivation for studying the epic and analyzing the consistency of episodes and a few planetary positions.

I found it appealing that inverse problems like critical excitations can be handled and solved mathematically. The question here is: from effects can we go to the cause? Because, many a time we may not know the external forces or they may be known only approximately. In such situations it gives satisfaction to get an idea of the cause. The solutions are however non-unique: there could be several forces leading to the same response criterion. But still, I have pinned down the causes to a smaller region – this gives some satisfaction.

Your work on the analysis of nonlinear and random dynamical systems bears out your enthusiasm and faith in the powers of purely analytical tools. Following the recent digital revolution, which has made computation cheap and accurate, and the enormous success of the finite element method, what, according to you, would be the future role of these analytical tools vis-à-vis research in structural dynamics and random vibration?

My enthusiasm and faith in powers of analytical tools is of course very firm and it will continue the same way. I have nothing against the numerical methods. To me both are wonderful allies to each other. I am going to use them together.

An engineer is supposed to do experimental work, be practical, observe and record: at least these are the things that we teach in undergraduate civil engineering classes. A theory however is always needed to interpret and understand experiments. Are such analytical tools always well developed particularly when large data comes up? These data could be coming from laboratory experiments, computer simulations or by observing nature (like rain fall and earthquake records). We need analytical tools to understand them.

The future of structural dynamics is quite bright. Random vibration in my view is included in structural dynamics. For concepts like real time substructure testing that you are now trying to develop, you need to be very strong in theory to succeed. I am of the opinion that to be an excellent researcher in experimental work is more difficult than being an excellent theoretician.

What would you identify as the two most important problems in random vibration and nonlinear dynamics of engineering systems?

We will pass this question. You people are better qualified to answer this question than myself! What is in your mind when you ask this question?

In many fields there will be a few unsolved problems - big problems - that hold the key to real *progress. There would be some stumbling block that needs to be overcome. It is in that sense that we are asking this question.*

I have not thought of random vibrations and nonlinear dynamical systems as separate problems of my work. I have broadly looked at applications of random processes. For instance, I have investigated forecasting monsoon rainfall through statistical methods and random process techniques. Even though we are doing something, these are still open problems and lot more needs to be done. I can't think of random processes separately from nonlinear engineering dynamical systems. At least I have not so far thought on these lines.

Your first paper on chaos appears to be dated back to 1989. Did you take the subject up purely out of a research interest or did you visualize any specific applications of this research in the context of civil engineering?

During 1980's I had a student by name Meera who was working on vibration of railway vehicles. She was studying the response of a moving railway wagon due to guide way induced vibrations. The wagon had hysteretic springs and she derived a 8-dof model. Initially we considered sinusoidal track profiles. She solved the problem using the averaging method. She could handle the multi-dimensional nature of the problem and derived simplified equations. The system had non-proportional damping. It led to several interesting questions. We tried to find the response amplitudes with reference to velocity of vehicle and wavelength of the track. For some parameters the simplified equations using averaging led to difficulties. I suggested her to integrate the equations of motion directly using Runge-Kutta method especially in parameter regions where there were difficulties with approximate solutions. The results showed that at some places the results were nearly periodic; at some other places they were totally erratic. Changing step size also did not help. I had heard that nonlinear systems could behave peculiarly and I was wondering what may be happening but I had no idea. But, as coincidence would have it, I had a meeting with Professor Roddam Narasimha at that time. He was just then back from an IUTAM symposium in Europe on nonlinear dynamics. He said the meet was about chaos and I was curious to know more. He is a very interesting person. He is one man who has impresses you by his sharp intellect and depth of knowledge in a variety of subjects, including Sanskrit, philosophy and history of science. He went to the black board and he explained discrete maps and strange attractors. I asked if a Duffing oscillator with harmonic forcing could become chaotic. He answered yes and mentioned about Duffing-Holmes oscillator. This excited me and I thought that I could do something. That discussion had showed me the following: if chaotic motions are steady state phenomena and they are not going to be periodic in the way I had understood with small perturbation and averaging methods, it could be random, and it had to be ergodic in some sense. But RN impressed upon me more the infinite periodicity of the signal and how it comes about. This is vaguely what I can recollect now. Then I decided to study this further particularly because of the experience I had with railway vehicles. Around that time there was an International school on chaos at CAS. This helped me further. We did quite a few things – even experiments with beams with G V Rao and yourself (Manohar). Stochastic characterization was an offshoot of this. Debasish Roy, as you know, developed this further in his thesis.

Research efforts in random vibration and nonlinear/chaotic dynamical systems and possibilities of their practical applications appear to have been plagued by the curse of dimensionality. Do you foresee any route other than advanced computational means to resolve this?

Bigger problems always will have difficult questions. Engineering problems are always very complicated; but many a times we may be satisfied with some simple answers. But if you want elegant answers, you have to have some systems that are simple also. These problems are there not only in nonlinear dynamics, chaotic dynamics, random vibrations, but also in other studies involving large scale systems.

Do you see any advanced computational methods suited for this?

Computation is perhaps the only way that you can cross over this but I have not worked on computational approaches to solve such problems. I know you have been working on some of these problems. Approaches like stochastic finite elements, did not impress me; not because they are not applicable or not useful: but because one quickly comes to some dead end somewhere and one has to struggle with other kinds of approximations.

Do you feel that the component of research on mechanics and physics of solids in on the decline in Civil Engineering? If so, what would be the reasons? Is it a problem or do we at all need to rectify it?

If you are thinking of this in the Indian context, your question is right. In the international context, is it reducing? I don't think so! In the Indian context, we all have a responsibility to rectify this. There are new areas like poroelasticity, magneto-elasticity, mciro-mechanics - they all have links with structural mechanics. I don't think we can afford any slide or decline in the quality of work in this area. But it is a fact that theoretical and applied mechanics are neglected fields of research in this country.

Has the lure of big research grants played spoilsport with the independent thinking process of researchers in India?

Yes, I think so. But let me hasten to add, this is a very complex issue. Large money may be required to carry out some problems that are important to the country. That itself should not be a worry. For example, strong motion instrumentation or the much-advertised topic of seismic microzonation of Indian cities. Naturally, government agencies like to encourage more persons to carry out research in these areas. But, what I find is that huge grants have been given to groups with very little proven research background in these areas. Perhaps there are other areas wherein also this is happening. It appears administrative compulsion to spend large sums of money on some areas of social relevance has taken precedence over merit in recent years. Civil engineering research linked with disaster mitigation is unmistakably an area of immense societal importance. It is not sufficient if funding agencies, which after all dole out public funds, support academics. They should seem to promote high quality research also. Otherwise researchers who value their independence more (than huge sums of money) tend to concentrate on publishable theoretical research instead of applicable research of national relevance. In the bargain the policy of bringing the fruits of S&T to the common man suffers from research of doubtful quality. But then there are no simple answers to some of these issues. As the country decides to increase S&T funding for non-strategic research, I visualize the size of the cake would increase and hence many of the current imbalances would get corrected.

Do you think there are enough interdisciplinary interactions between researchers in Structural Engineering and other disciplines in India? Are such interactions important?

At present there are very few interactions even within our department, not to speak of between institutions. But the fact is that it is not totally absent. I interacted with Athreya, who is a mathematician. I interacted with RDSO, DRDO, BARC, etc. Manohar gave a set of lectures at BARC on reliability, this is interaction. Similarly if Sudhir Jain is bringing IIT's and IISc under NPEEE, it is interaction. Maybe these should increase or improve- that is a different matter. At institutional level, if it has to happen, then it depends upon the vision of the people at the forefront. Recently I was a reviewer for a DST project that SERC Madras and IIT Madras had jointly carried out - I was impressed by the way they had interacted. While I was at CBRI, we had interacted with IITK to support MTech students at IITK whereas their project could be done at CSIR labs.

How about inter-disciplinary interactions?

Inter–disciplinary interaction can also happen and it should. At one time when biomechanics was still an emerging field, many structural engineers interacted with doctors. Now such interactions are quite routine. That is, looking at the human body as a mechanical contraption- like an engineering system- is happening in a big way. I think structural engineers can contribute enormously in this field. If people in this department take some initiative, they could be sucked into this area.

You have been outspoken about the issues related to safety of large engineering structures in the country and also about issues related to policies in the codes of structural engineering practice. Do you feel the need for any shifts in the way these issues are handled in the country?

Absolutely. Every designer worth his name has been trained to make a safe design. So long we talked about lintels, roofs and column footings, the factor of safety approach of our undergraduate classes was sufficient. The consequences of failure are limited to the owner or occupant of the building who may be willing to take risk voluntarily. But this is certainly not true when you talk of a chemical factory or a large dam. Thousands of people living downstream of a dam are subjected to involuntary risk. In such instances, the consequences of failure have to be taken care of in the design process. The consequences of failure have to be envisioned from a societal angle. If these consequences are going to be catastrophic to the whole society, we have to follow different methods of characterizing and qualifying safety. Unfortunately this is not happening in the country. That is why I am outspoken and critical. This worries me and I feel that more and more structural engineers should debate this issue. Open discussion on involuntary risk is needed. Whenever involuntary risk is involved, engineers cannot just rest by saying that their structure is safe for M=7 or 8 earthquake. They should come out openly and talk of the risks involved. If language of probability is needed for this, it should be used. So definitely a shift is needed in the way we are discussing the concept of structural safety.

Can you elaborate on what is the nature of this shift?

Well, in any sector in the country, be it petroleum, food, education or power, about 40% of money goes into infrastructure development. This money is more or less handled by civil engineers and that is why civil engineers get a good or bad name depending upon how they handle their responsibility. Right now the treatment of safety in the country is a soft concept. There is need for a paradigm shift in the perspective. Recently I read about a 12-year research program conducted in the USA on the fonts to be used on the highway signboards. They have been using for a long time the highway Gothic script. Now they are changing that because at the speed at which they are going on the roads, with the new fonts, drivers will get another 1-2 seconds of time before which they can see what is written on the boards. The idea is that using the new fonts on signboards can prevent many of road accidents, particularly involving aged people. This is an example of safety consciousness. This kind of forethought or vision, particularly when it comes to large dams, is very much needed. We must recognize that it is not just the economic life for which these structures have to be safe. What happens after the economic life is over? In nuclear industry there is concept of decommissioning a NPP. It is understood that, if need be, the structure can be dismantled and the site is brought back very near to its original condition. Have you heard of a similar concept in the case of large dams? As far as I know, our policy makers have thought of recouping the investment in about 100 years and then hope the reservoir would be used for irrigation. They expect, without saying so, the structure to exist perpetually. But the question is whether our structural engineers endorse this conscientiously. In a high level committee meeting on Tehri Dam, held in Delhi in 2000, I raised the question how the safety of the dam deteriorates with time and what is the perception of the authorities on what may be the structural integrity of the dam after say 500 years. A senior adviser of the CWC, present there, was aghast at my question and sarcastically commented that Bahuguna-boys are against development! It is disturbing to see some agencies brushing aside very relevant questions by extraneous arguments, instead of arguing through intellectual discourse. This is where a shift in the mindset in the way the Country is quantifying safety of large-scale structures is needed. Social life for a structure in some cases could be equal to the economic life but not always. When it comes to private buildings things may be topsy-turvy: buildings that can last for another thirty years are pulled down because it is no longer profitable to have them the way they are.

The subjects of structural dynamics and random vibrations seem to be suffering from an elitist image especially amongst the structural engineering community in this country. How you feel about this?

It is unfortunate, if this is true. Is this true? I want to ask that first.

Well, this at least is the perception among consultants and college teachers that these subjects are highly mathematical and difficult.

If you ask me how I feel about this, I can only say that I do not feel anything- it is all right! This kind of feeling exists in other walks of life! Unless you clarify the thrust of your question, I cannot answer this.

A discussion of nature of subject of structural dynamics came up in one of the NPEEE meetings on model curriculum development for undergraduate education. Many people there seemed happy with teaching equivalent static analysis. It was said that if students are asked to write down differential equations and solve the problem, it will be beyond the students' ability and nobody will understand. In fact someone likened structural dynamics to open-heart surgery- something that should not be taught to under grads. It can be a wrong weapon in wrong hands.

I disagree with this perception. But, we may not be able to teach structural dynamics at undergraduate level for other reasons. We have to have priorities. The subject need not be taught as a compulsory course- but as an elective- certainly yes. This is nothing to do with the subject being elitist. If I were to teach an elective of say 3 hours per week on earthquake engineering, I will certainly teach some amount of structural dynamics. This could include dynamics of single degree systems, concept of natural frequency, damping and response spectra. This much is certainly needed.

You have now worked in many fields: Disaster Mitigation, Structural Dynamics, Earthquake Engineering, Railway Track-dynamics, Rainfall Modeling, and History of Science. How you feel about your contributions? Which work has given you the most satisfaction?

I can't answer this question even to my own satisfaction. How good are my contributions - it is for you to say. All my work has given me satisfaction- but I wish I had done better.

What you see as the grand problems of structural engineering research? What problems would you have liked to work on if you were to start all over again?

If one thinks of exact sciences there can be grand problems (like the Fermat's last theorem). I don't think civil engineering research is of that kind. If you restrict your question to earthquake engineering, in my personal perception, there are a few problems that can be envisioned on a big scale. I can tell you about two or three grand problems in earthquake engineering that I have in front of me. One is, developing an experimental facility for fault rupture and demonstrating how structures on the surface can vibrate. This is not simple but can be done. It is something equivalent to what people conceived about ground motion simulation. In Yugoslovia (I think) they built up a huge system, which is of the size of a building or a room, it seems (I have not seen it) with chains and pulleys and all that so that low frequencies can be simulated. But they immediately found out this does not work. Then Housner simulated earthquakes using actuators with a cam that was cut to the profile of prescribed displacements. Jagadish and Rama Prasad tried out a similar thing here, in our department. When the first shake table came up in Berkeley, it was a big achievement in those days. It has taken some 30 years or so to reach this stage. The facility for fault rupture simulation that I am talking about is something comparable to this and I would love to see IISc or somebody in India do it. I don't know whether anyone has conceived that a fault rupture can be simulated in a laboratory. The force required may be enormous. But you have to try these things. Geologists won't do this experiment. Geophysicsts are not working along these lines. This is an engineer's problem- that is researchers who are on borderline between science & engineering - they have to do it. Initially it is going to be educational, but something new may come out of it.

And, the second big problem is developing an elasto-dynamic model for the Indian plate and study its collision with the Tibetan plate. This is a solid mechanics problem. This requires large computer power, which is readily available. Initially one can begin with a static problem: say approximate the Indian plate as a visco-elastic triangular plate on an elastic foundation. Then build up all the known major faults, internal stress regimes and annual strain increments into the model. This can be initially used to understand a few observations, such as, for instance, the presence of a trench in front of the Himalayas that is about 10 km deep. Why did it bend like the way it is? What is the characteristic wavelength here? Once such a model is calibrated, it can be used to see if small perturbations in the plate body can lead to earthquakes or not. IISc Structures group is well suited to handle this problem. This is a teamwork, that may take 5-10 years but it will bring laurels to the Department if it can be properly executed. I tried doing something on these lines while I was at CBRI. I worked on modeling of Himalayas. My interest has been to look at nature through mathematics. Mathematical modeling is the language that helps me to go to the next step- it is not an end by itself- but is the means; it is a very powerful tool. Now, what does modeling Himalayas mean? I have collected the data on the surface topography. It is actually a random filed. Geologists have talked about episodes of mountain building activity. They have come to specific conclusions on lower Himalayas, the Shivaliks, the upper Himalayas, which got formed during different episodes. This provides a very broad picture. But when you look at small scales and ask about landslides, safety of cities like Shimla, Srinagar etc., broad models are not powerful. Using the elasto-dynamic model some questions can be answered. One can harness the power of the finite element method for this problem. To start with a plane strain model along the North-South section of Himalayas could be used to analyze the problem under body forces. The surface can be modeled as a stochastic boundary: we have here a stochastic boundary value problem on hand! Ordinarily you would not get such problems in structural engineering. The top surface would be stress free; but there could be tension at say 10 m below the surface. One can fine tune the model and draw contours of principal stresses. One can look for places where failures could happen. This way one can arrive at relative safety estimates for some cities. This can be extended to a 3D model and with suitable calibration the relation between the complex terrain and the strain build up at the plate level leading to landslides could be better understood. There cannot be anything grander than mathematical modeling of Himalayas!

The third big problem is mapping of seismic hazard of the country on probabilistic basis.

You took up the position of the Director of CBRI while you were a productive academic researcher. In retrospect, how did the stay at CBRI jell with your aspirations as a researcher in an academic Institute?

I was not doing all of what I wanted to do here at IISc. It is just that! Many people in middle age may have that feeling but may continue with what they are doing for various reasons. But at that time some things happened: Dr Valluri talked to me; and, he has a way of talking! His way of talking somehow touched some aspects of my ambitions. One of the things that I had in my mind was strong motion instrumentation. I have had difficulty while at IISc in getting strong motion data for Indian earthquakes. I could not easily get data on Dharmashala earthquake, Uttarkashi earthquake. I always had the feeling that we must have a strong motion instrumentation program. I did not get much support here at IISc. I could immediately see that I could do this if I were to go to CBRI and never be able to do it if I continued to be at IISc. While at CBRI, I did not leave my academic career. I did the strong motion instrumentation at Delhi. Secondly, while at CBRI we worked on reduction of pollution in brick kilns, bringing it down to statutory levels. There are about 50000 kilns in North India, which contribute to pollution enormously. We developed a method that did not use electric power but involved a novel design of the settling chamber. We made a laboratory model and showed by actual measurement that the dust coming out is within prescribed limits. Our model was accepted by Government agencies and the kiln owners. They could implement this technology themselves. This work gave me lot of satisfaction because it was a large-scale problem that got solved. Incidentally this work won the NRDC-Technology Day Award in 2001. So in answer to your question I would say, yes, my stay at CBRI indeed jells - at least partly I could satisfy my academic aspirations.

This question has something to do with the structures group at IISc. The style of functioning in our group seems to be somewhat individualistic and this is at variance with the more prevalent international practice of doing collaborative research. How do you see this: as our strength or weakness?

I think it is a weakness; we have to interact, there is absolutely no doubt on this! Personally I have always interacted with other researchers. In my opinion we should compliment our strengths. We would be able to do lot more things without sacrificing our individuality. We must collaborate with others.

What qualities you think make a good researcher in engineering? We seem to be caught between demands of being socially relevant on one hand, and doing publishable research on the other. Researchers in mathematics and basic sciences do not seem to face this dilemma as much as we do. How you feel about this?

I have no idea as to what is the dilemma they face. My feeling is that you may be imagining things! For instance, if a researcher in the area of Chemistry develops nano-materials, is it not socially relevant? Therefore, when we talk of social relevance we must be careful. The spheres of social relevance could be different, the patterns could be different. Social relevance must be seen in proper perspective.

A good researcher in engineering or in any area I would say has an envisioning capacity- a capacity for imagination and daydreaming, thinking about model making, drawing analogies, seeing how developments in other fields could be applied in his field. The finite element method for instance was developed originally by civil engineers and it went into other areas like aerospace and mechanical engineering. It got developed lot more! These other people envisioned immediately that what works for dams and bridges should work also for their structures. Nowadays a surgeon's work also perhaps involves finite element models. Similarly, now civil engineers talk of fibre reinforcement in concrete. This idea originated in aerospace community. Someone in civil engineering community envisioned that idea might work in civil engineering too and pushed the idea to work.

The question of social relevance is still unclear to me.

We can ask a related question. There is a gap between structural engineering research and what designers and construction engineers practice. There is a perception that the work that tries to bridge this gap is more socially relevant.

Yes. I see researchers as random variables with multi modal distributions! People have different strengths. Left to itself, nature always differentiates itself with peaks and valleys and that is how structures emerge. This is a philosophical statement. If you leave an academic system on its own, peaks and valleys emerge. Social relevance - I am afraid, if you think in terms of low cost housing and masonry etc., no! That is not the only socially relevant work. That is also socially relevant, I don't deny. If someone says "I am working on low cost housing or rural housing- a very socially relevant work, therefore, I am a better researcher in engineering" -that is wrong. Such work need not be better research. There is place for every kind of research. Work on safety of a large dam or nuclear power plant that requires a very high level of engineering research is also socially relevant. Social relevance is a vague concept, because in this country this has not been discussed *vis-à-vis* civil engineering research. There are some who think civil engineering is only codes and construction and hence no serious research is needed. Once a senior physicist of IISc, who is no more, asked me what type of work I do.

His reaction was “Oh! Are stochastic processes relevant to civil engineering?”

Is construction the only worthwhile activity of civil engineering profession? What about protection of heritage structures? How many leading academics are carrying out research in this area? Is this not socially relevant? Similarly research in railway engineering is highly relevant. So also is fire-structural engineering. Hardly any front-end research is being pursued in the above areas in India.

Amongst the multifaceted role as educator, researcher, consultant, and administrator, which role challenged you the most? Which gave you the most satisfaction?

The challenging role, I can definitely say, was that of being an administrator at CBRI. When it comes to satisfaction, I always had it as a researcher. I cannot separate research and education. They go together. I would say I derived satisfaction as an academic. I have not been a big time consultant. But even with my few consultancies I derived satisfaction with analytical projects such as uplift of nuclear power plants, dynamics stability of missiles and nonlinear analysis of control rod drop during SSE.

You say that you found administration challenging but this was not that gave you most satisfaction!

But I do not know your definition of challenging role!

Yes, there seem to be a dichotomy about it.

Administration of R&D indeed gave me satisfaction. But even there, I derived more satisfaction doing research or in seeing others do good research. You know that Shailesh Agarwal did his PhD with me during my tenure with CBRI.. Always I have derived maximum satisfaction from research!

The question can be rephrased somewhat. Was being an administrator more difficult for you or being a researcher was more difficult?

This is hard to answer! I am trying to answer in terms of some quantifiables. Being an administrator was more taxing: one has to work with lots of constraints; you need to progress within the limits of these constraints; you are not a master of your time! Naturally it was stressful. In this sense it was challenging. In research no such constraints exist! In any case, CSIR gave me a chance to come across many problems that I could not have imagined from here. The mathematical modeling of Himalayan terrain, for instance, was one. In fact, some problems that I am working on since my return to IISc from CBRI are connected with my experience there. My thoughts on the previous grand problems in earthquake engineering came to me while I was at CBRI.

What would be your advice to a young and enthusiastic researcher in Structural Engineering? What would be the most important ingredient for being recognized in the research community?

Just do good work - that is all. Be original and individualistic. If you have spark in you, you would be recognized. If you imitate others and produce lots of work, people may applaud you but you would not earn recognition. In IISc I have seen people doing both kinds of work- putting flesh to a skeleton kind of work and also something original. This is all right. But if you want recognition, you must be original and produce novel ideas. Keep a part of your time for collaborative research with others. Originality is the hallmark of a good researcher.

Thank you, Professor Iyengar for your time. It has been wonderful talking to you.

Thank you.