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*Silver Jubilee Article*

# In pursuit of science

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## 1. A prologue

When Dr. Vinod Krishan invited me to contribute an article to the Silver Jubilee volume of the Bulletin of the Astronomical Society of India as a kind of my scientific autobiography, I was quite hesitant to accept. The reason was that I did not have great credentials as either an astronomer or as an astrophysicist, even though I had done some work in some limited areas of astrophysics. I was, however tried to be persuaded by the argument that as a Director of Physical Research Laboratory I had an important role to play in the development of the astronomy and astrophysics programme. Only partially convinced, I accepted the invitation nevertheless, for some other reasons as well. Throughout my life, childhood onwards-, as a student, as a researcher and as a Director I had experiences, which I thought I could share with others, and which might provide some useful hints. In particular, my account may also furnish some historical perspective of the scientific scene obtained at TIFR in the fifties and sixties, and the evolution thereof. This may be of some value to the readers. I also have certain views to express based on my observations of the Indian scientific scene which I thought may be of some use.

It is possible that in the process of writing all these accounts I may have become somewhat personal at some places. However, without expressing myself so uninhibitedly I would not have been able to convey what I want to. Rather than describe my experience in an "abridged" fashion, I have tried to express what I have lived through at various times and occasions: excitement, agonies and ecstasies, trials and tribulations. I hope that some readers may find here some echo of their own experiences. Younger readers may also find a lesson or two in these experiences of mine.

I would like to thank Dr. Vinod Krishan, the Editor for giving me the opportunity of writing this article.

## 2. Motivations

The desire for the pursuit of science as a career comes as a somewhat natural extension of the *Child's Curiosity* of one's childhood days, unless that curiosity has been crushed. It is the pleasure and the childlike delight in unearthing some new facet of nature and adding it to the existing body of knowledge, that motivates most to a love for science and to a career in it. Somewhere underneath, however, also lurks a desire for a 'name' and a 'fame' - a rather humanish weakness - resulting from such a new finding. In fact it would be no exaggeration to say that this latter sentiment plays a dominant role in motivating scientists to undertake difficult challenges and register great achievements.

Being human as I am, I was probably also motivated to pursue science because of the same sentiment; but I believe, the excitement of learning and possibly discovering some thing new was more at work at the (young) age that I committed myself to the pursuit of science because the consciousness of the 'name' and 'fame' appears at a much later age. I found it a great pleasure in asking questions to myself and to others regarding the natural phenomena occuring around me.

## 3. Childhood and school days

The earliest such questions that I remember to have asked was to my grandfather when I was about seven years old. Sitting on the bank of Ganga canal (which flows beside my home town) beside him, I was throwing pebbles into the water one after another, watching them sink. I chose successively smaller and smaller ones to see if that made any difference. It didn't; they all sank. I recalled that big logs of wood would float by merrily, which could be about a million times heavier than the lightest grain that I had experimented with; I immediately asked my grandfather, "How come?". However with his little knowledge of formal science, but vast experiencial wisdom he merely said, the pebble was 'heavier' while the wood was 'lighter' which was why it was floating - a substantially correct answer, though not a precise one.

It was really my grandfather who inspired me to take up science as a subject of my study through all the wonderous things that he would describe to me involving, changes of colours in chemical reactions, for example. I wondered where he could have seen these reactions. It was through those daily morning walks and conversations with him, my constantly harrassing him with my questions about nature that unravelled before me daily, that I learnt many things and imbibed from him his sense of values. Those morning walks through the gardens lined on both sides of the road to the canal are imprinted as the happiest memories of my childhood days (sadly there are no more of those gardens). I believe those laid the foundations of what I am today - a being with an abundant love of nature and desire to understand its wonderous ways. The love of nature also makes one a sensitive being which allows one to notice a myriad little things in life and derive joy therefrom.

The facination with science became so overpowering that I insisted, after std VII, on moving out of my school in my hometown (Sardhana) where there were meagre facilities for science education, to a school in Delhi (where my uncle lived). I assumed tacitly that the facilities would be superior there. Luckily, the school that I did join in Delhi (the ASVJ Higher Secondary School, Daryaganj) as a std VIII student, did indeed have better facilities. While all the teachers there (physics, chemistry, mathematics, english etc.) were indeed very good, the physics teacher, Sh. Tej Pratap Singh was exceptional. It is to his devotion and dedication to teaching, his uncompromising attitude to honesty in scientific recording and reporting, his insistence on preciseness of expression and above all, to the systematization of knowledge that he presented, that I owe my attraction to physics and my career in it. His manner of teaching provoked questioning and he always ended a topic with a comment as to what lies beyond as one goes to higher levels of study in a particular topic. I may mention here that the "sunspots" were first shown to us (the higher secondary class) by this teacher through a telescope which he had made himself. Even in those times such teachers were rare. Now they may be regarded as an extinct species.

## 4. Fascination with physics

A spirit of inquiry is almost synonymous with science. Questioning makes the learning process proactive and hence more delightful. It can also lead to a faster learning, arriving at times, at an advanced knowledge in answering the posed questions. An example of this arose from a rather "curious" (at least I regarded it as curious!) fact revealed from a table of latent heat of vaporization (as measured by Regnault) at different temperatures that I came across as a student of Intermediate Science (in a rather old text book of heat by R. Wallace Stewart (Oxford University Press, 1910) which I owned, having bought from the *kabari* bazaar around the Jamma Masjid in Delhi). The "curious" fact that surprised me somewhat at that time was that latent heat of vaporization decreased with the temperature, Why should it be so? A little thought and a small calculation yielded the answer which was substantially correct, and which turned out to be a part of the Clausius equation which I was to study a year later in my thermodynamics course in B.Sc.

One other fact that struck me as quite strange and fascinating as a student of B.Sc was that the saturated vapour pressure of a liquid was a unique function of temperature (as against the perfect gas relations). I wanted to obtain a theoretical relationship which describes this behaviour. After reading a lot of material, I was led to the Clausius-Clapeyron equation. But this equation involves both the latent heat and specific volumes, which depend on temperature. It could not be integrated exactly to determine pressure as a function of temperature. This represents another example of a proactive learning process which led from a particular inquiry to a learning about the Clausius-Clapeyron equation. In the process one learns many other things which one would not be able to do in what one may call as a passive learning process.

After my B.Sc.(from DAV college Muzaffarnagar, affiliated to Agra University) I was not able to join M.Sc. due to financial constraints. I worked as a "demonstrator" in physics during the year 1953-54 in the S.D. College, Muzaffarnagar (the college where I did my Intermediate from) on a salary of Rs. 120/- per month. However, it was not just as a demonstrator in physics that I served. I also taught intermediate classes in physics. And it is not that I was being exploited by the College authorities, I did it for the enjoyment of teaching. I enjoyed innovating ways of introducing a subject and letting the student learn and feel the inner excitement of physics, and the innate order of the facts of nature that it represents. At the age of 18, I was no more than a couple of years older than most of the students, but I was overjoyed by the regard that I received from them.

Following some marginal improvement in my father's financial position, and some savings from my demonstrator's job, I joined M.Sc. at Lucknow University. It is another story that Delhi University refused me admission on the ground that as a B.Sc. from Agra University, I was not good enough for admission to M.Sc. in Delhi University. Even Meerut College (which was affiliated to Agra University itself) would not admit me because, according to Mr. Mathur, the then Head of the Physics Department, I may have become "rusted" after the break, working as a demonstrator. It did not matter to him that I had a first division in B.Sc., and I had carried a letter of recommendation from the principal of my college (D.A.V). Lucknow University thus gave me a break, which I ought to be thankful for.

## 5. The TIFR days

After my M.Sc. I was selected for the position of a Research Assistant at the Tata Institute of Fundamental Research (TIFR) in its theoretical physics group. I joined the Institute on August 20,1956. The interview by the committee of which Bhabha was the chairman was a unique experience. I do not remember how many questions I answered correctly, but Bhabha whose very presence was a little too overwhelming for a young person like me, however, put me completely at ease with his charming manners and gentle questioning.

I had the impression after the interview that I had not done very well. So, I was quite pleasantly surprised at being told of my selection, the very next day, and was obviously quite exhiliarated to have gotten entry into TIFR which for me was a dream come true.

I found TIFR, which was then housed in the Old Yacht Club building, to be a place with a sizzling academic ambience. There were Wednesday's colloquia and Thursday's theoretical physics seminars, both of which Bhabha attended unfailingly (whenever he was in town), even if he came about twenty minutes late. Both of these academic activities were taken quite seriously, with the colloquia being attended by all the scientists of the institute and the seminars by all the members of the particular group. The presence of Bhabha added further to the seriousness of the business. Questions from Bhabha were sharp and he expected precise answers. This made the speakers come well prepared and kept them constantly on their toes.

Relationships among the academic community were rather informal with a conspicuous absence of hierarchical behaviour. Bhabha was, however, aristrocratically aloof. The scientific discussions were intense with a great deal of interaction among the members of a group as well as across groups.

The library was open for all the 24 hours, with the result that it was not unusual to find the members of the mathematics and theoretical physics groups to be working late into the night until 1 or 2 am. Same was the case with some experimental groups. One would inevitably sense the seriousness of purpose all around.

The totality of this ambience was in sharp contrast to what existed in the University that I came from, or if I may say so, in most universities at that time. I must say that my attitude to work and research, and the scientific ethos and values were deeply affected and moulded by these early years at the TIFR.

One of the most important academic traditions which was initiated and practised at the TIFR during that period was to invite eminent physicists from around the world to spend a period of a few weeks to a few months at the Institute, who would give a series of lectures for the benefit of especially the young scientists. Thus there was Gregor Wentzel during the fall of 1956 who gave courses on 'quantum mechanics' and 'classical therory of fields'. Later in the years, we had such luminaries as T.H.R Skyrme, A.M. Lane, Willis Lamb Jr., Phillip Morrison, R. Serber and Hannes Alfvén. It was from Alfvén that in 1958 I had my first course on "Magnetohydrodynamics" and a series of lectures on "Cosmic Rays". My first paper on 'Acceleration of Cosmic Radiation' (which was jointly written with G.S.Murty) arose out of this course. This may, at the same time, be regarded as my first paper on astrophysics published in 1958 which to my surprise has been found to have been quoted as late as 1981.

## 6. Initiation into plasma physics

My initiation into the brand new field of plasma physics was motivated by the then head of the theory group at TIFR, Dr. K.S. Singwi, I believe, at the instance of Bhabha. Having "ventured to predict that fusion energy would be available within the next two decades", Bhabha, as the chairman of the first Geneva Conference on the Peaceful Uses of Atomic energy in 1955, was anxious that some work on the controlled fusion should be initiated at the TIFR. A theoretical group was thus formed around 1957 to study plasma physics, with myself, G.S. Murty and S. Nagarajan as its members.

P.M.S. Blackett who was on a visit to TIFR, and who had somehow acquired a clout as a scientific advisor of sorts to the Indian scientific establishment, was invited to a meeting called by Bhabha to discuss the course and plan of action to pursue the goal of "controlled fusion". Both Murty and I were invited to that meeting which was attended, among others , by Bernard Peters, K.S. Singwi and D.Y. Phadke. Zero Energy Thermonuclear Assembly (ZETA) at the Imperial College, London, with which Blackett was associated, was in big news those days because of some neutron yield observed in that experiment (It was later found that they were not of thermal origin). Blackett was of the view (wrong in my opinon then and grossly so in retrospect now) that to make any worthwhile progress in the direction of 'controlled fusion' one must go in for big machine type high temperature plasmas. Peters and Singwi, on the other hand, opined that one should first learn about plasmas through small scale experimentation. Quite obviously, the latter view turned out to be the sensible one.

Nothing, however, happened subsequently, either on small scale or on large scale. Singwi left TIFR to join the Argonne National Lab, following differences with Bhabha. Peters also left soon thereafter to become later the Director of Danish Space Research Institute. In fact, there was a virtual exodus from TIFR of many young physicists to look for greener pasteurs in the U.S. as a result of the tremendous job market for physicists there in the post-sputnik scenario.

That was a severe jolt to TIFR. It was also a blow to the nascent plasma physics group which found itself in doldrums. No professional guidance was available to this group since there were no plasma physicists there. There was not even a conceptual or a policy guidance available to it from the top. We were left to fend for ourselves.

However, I continued to work in plasma physics all alone undaunted (though somewhat foolishly I later realized; I should have also left early to work towards a Ph.D. degree). Spitzer's book on "Physics of Fully Ionized Gases" was the only book available then from which I studied plasma physics. In fact, during this period, with a view to understanding the nature of Spitzer's resistivity I was led to study a host of papers including those of Chandrasekhar, Chandrasekhar and Von Neumann on the theory of stellar encounters to understand, in particular, the origin of "dynamical friction" which I wanted to relate to the electrical resistivity. These considerations, in turn, led me to look at the kinetic equations for plasmas, which were then an area of a lot of activity. I was able to write a paper on the subject based on a formalism of Prigogine and Balescu.

Even though this paper was subsequently published, it became clear to me that working in isolation, as I was then doing, was leading me nowhere, and I was wasting a lot of my very valuable time. So I decided to go for a Ph.D. A letter to Marshall Rosenbluth along with the manuscript of the paper that I had written brought forth a positive response and I joined him as a Ph.D. student in September 1961, at the University of California, La Jolla. Since joining TIFR in 1956, I had lost a very valuable five younger years of my life, before wisdom dawned on me that I should take care of myself. I must however record here my appreciation of the then Deputy Director Prof. M.G.K Menon who helped me a great deal in my efforts to gain admission at La Jolla.

## 7. The La Jolla years

The Department of Physics at the University of California, La Jolla which I joined as a graduate student, was a first rate one, with a highly distinguished faculty: Keith Brückner, Maria G. Mayer, Walter Kohn, Walter Elssäser, Harry Suhl, Carl Eckart, Oreste Piccioni and Marshall Rosenbluth, who was only half time with the department. Norman Kroll was to join later.

The Physics department of UCLJ (later UC San Diego) was constituted only two years ago and I belonged to the second batch of Graduate School there. The teaching at the department was superb. There was tremendous enthusiasm among the faculty, and there was keenness on the part of the students to learn. There were social gatherings involving faculty members and students. Foreign students were taken care of specially and lovingly. There were tensions of examinations, specially of the qualifying exam. But there were also fun times - what with the golden beaches and the La Jolla's naturally airconditioned climate. There were the joys of learning mingled with pleasures of the campus environment, the folk songs and the folk dances. The La Jolla years are perhaps the happiest days of my adult life.

Rosenbluth was a very good guide and a teacher, and I learnt a great deal from him : his manner of approaching a problem and getting to its solution; the very understanding of the plasma physical concepts which was his very own. I imbibed from him the manner of his approach and concepts. This enabled me later in my life to see through a problem at least qualitatively without going through the tedium of a lot of algebra. Unfortunately, at times it worked to a kind of a "disadvantage", because if I could see through a problem, I lost interest in it and failed to translate it into a paper. On the positive side, it allowed me more time to concentrate on the more exciting and challenging problems.

I was indeed very fortunate to have such a distinguished faculty as my teachers. I learnt a lot of things through the course work which I would not have done otherwise: Group theory and quantum mechanics, quantum electrodynamics and field theory. I also had a superb course on nuclear physics from J.H.D Jenson, who was visiting the Department in the year 1962-63. This broad-based training played an important part in my research career later on. It enabled me to understand and appreciate fields other than plasma physics, and to actually work in some of them. My interest in the foundations of quantum mechanics is an example, thereof.

While the TIFR years gave me a great sense of self-confidence and independence as a researcher, as well as a measure of wider academic background because of the extensive studies that I carried out there, the La Jolla years filled up some important gaps in my training and brought me into contact with the leading physicists of the world, and thereby let me breathe in the ambience of the highest level of academic excellence.

I submitted my Ph.D. thesis in August 1965, and joined NASA-Langley Research Center, as a NAS-NRC Resident Research Associate for a year until October 1966. For the five years that I was away from the TIFR I was on study leave which could not be granted beyond October 1966. So I returned to TIFR in November 1966. Being the institute wherefrom I started my research career, I had developed an almost emotional relationship with TIFR and I looked forward to returning to it in great anticipation of developing a plasma physics activity there and contributing to the original objective of evolving a plasma physics programme. So, I almost disdainfully spurned a rather lucrative offer of a long term appointment at NASA - Langley in its plasma physics division-a rather foolish act as I was to discover in retrospect.

I was excited to be back, not knowing that I was to be in for a rude shock. By the time I returned, plasma physics seemed to have lost favour with TIFR. I was asked by the then head of the theory division to switch to astrophysics. Not having any basic training in astrophysics, I saw myself doubly disadvantaged. First, I would not be able to pursue my work in plasma physics which I had so enthusiastically planned - a great setback to my morale. Second, I would have to learn an entirely new field, namely, astrophysics, for which I had developed no great enthusiasm. Moreover, it was not clear to me what kind of astrophysics, I was supposed to do. There was no guidance to that effect.

After a lot of uncertainty lasting over an year and a half, the four member plasma physics group at TIFR was finally wound up.

## 8. Into PRL

As a consequence, I landed up at the Physical Research Laboratory because it turned out that its Director, Dr. Vikram Sarabhai, who was also the Chairman, Atomic Energy Commission at that time, was looking for plasma physicists, partly to provide a plasma physics base to the space science activity there and partly (I was given to understand) to launch, at a future date, a fusion-oriented plasma physics activity. The estrangement from the TIFR, leading to the eventual "separation" was perhaps the most agonizing period in my life. But that need not be mentioned here anymore. Again it was Prof. Menon (the then Director of TIFR) who gave me a lot of encouragement and eventually arranged my transfer to PRL.

I joined PRL on July 3, 1968, as the first plasma physicist. PRL was very much different from TIFR, as Ahmedabad was from Bombay. The pace felt much slower here, and the number of disciplines much fewer. The theory group itself was small, and the possibilities of discussions with other theorist were thereby much rarer. Being the lone plasma physicist and with little plasma physics literature in the library to read, life suddenly looked very handicapped. Nevertheless, if PRL were to be my new home, I had to accept the challenge of making plasma physics as one of the central disciplines here. The advantage here was that plasma physics was welcomed here as against the attitude towards it at the TIFR.

An attempt was made to get more plasma physicists. The favoured bias at that time was space physics, however. Dr. A.C. Das, who had the space physics background joined the following year in 1969. By about March 1969, I had spent about two and a half years since my return from the U.S., most of it struggling at the TIFR and rest in academic isolation at PRL, with hardly any scientific output to speak of. I got a lot of education at PRL about the ionosphere, the magnetosphere, the solar wind and the magnetic storms; but for lack of a suitable collaborator in these new fields, there was no publication possible. Frustration started to creep in. I decided that I needed a change for a year or so to a plasma active place where I could get into the act of active research again. It was my *gurubhai,* Wendell Horton who came to my rescue and offered me a position with him at the University of Texas at Austin. So I proceeded on leave from PRL to work there.

## 9. My most favourite work

Fortunately, all was not lost on the physics front during the three years of turmoil that I went through. Already in 1967, I had started on a rather interesting but difficult problem relating to the nonadiabatic loss of particles from adiabatic magnetic traps. It had been known experimentally (both numerical and real) that particles trapped adiabatically, eventually leak out with a characteristic leakage time which increased with the magnetic field strength. Can one find out an expression for the life time as a function of the various parameters: the field strength, the pitch angle of injection, the energy and the form of the adiabatic potential? This problem on which I spent many years of my life later, posed a stiff challenge to its solution. To hit upon such a problem is both a boon and a curse. It is a boon because of the excitement and the challenge that it offers; and a curse because it tends to occupy one's mind (and time) in an addictive manner. The problem belongs to the non-perturbative class, since the nonadiabatic effects that need to be extracted are non-expandible in the smallness parameter of the problem.

A clue to a possible approach to the problem came to me in a rather intuitive manner, through an analogy of this problem with the tunnelling process in quantum mechanics. The leakage of particles due to quantum tunnelling from classical potential wells is compared here with the leakage of particles from adiabatic potential wells due to nonadiabatic effects (which manifest themselves when the small adiabaticity parameter $\varepsilon$, is not so small). There is also a mathematical similarity between the two problems. The relationship of classical mechanics to quantum mechanics is similar to that of the "adiabatic theory" to the exact charged particle dynamics. Both the relationships are asymptotic with the smallness of the parameters formally being $h$ and $\varepsilon$ respectively. [In the case of quantum mechanics, it is the WKB expansion, while in the case of charged particles dynamics there is a similar expansion].

Perhaps, I should mention that I was led to draw upon such an analogy because I was simultaneously occupied with the question of understanding the nature of quantum mechanics itself, and had been studying the book of Feynman and Hibbs. If the analogy is to be followed further one could ask what should be the counterpart of $\hbar$ (an action) which should replace $\varepsilon$, as the smallness parameter in the present problem. The answer has to be the gyroaction $\mu$. Thus if a Schrödinger-like equation were to be written down to describe the process of non-adiabatic leakage as analogous to the quantum tunnelling, it had to be

$$i\mu \frac{\partial \psi}{\partial t} = -\frac{\mu^2}{2m}\frac{\partial^2 \psi}{\partial x^2} + V_A \psi,$$

which I wrote down, pulling it literally out of my hat. $V_A$ here is the adiabatic potential, $V_A = \mu\,\Omega$, $\Omega = eB/mc$, (the gyrofrequency), and $x$ is the coordinate along the magnetic field line, and where $\mu$ enacts the role of

It is amusing to write down such an equation, but it cannot be taken seriously unless backed by sound basis. Alternatively, one may begin by atleast checking whether this described any existing experimental results to any degree of satisfaction. No such experimental results were available then (during 1967-68). On the other hand, all attempts by me to derive this equation from known starting equations were unsuccessful until the end of 1969. It was not until November of the following year (1970) that a heuristic derivation could be constructed which yielded the above equation as only one of an infinite set of equations differing in the presence of $(\mu/n)$, $n=1,2,3...$in place of $\mu$ with $\Psi(n)$ being the corresponding "wave functions".

By 1969 some good experimental results were also published which gave leakage life times as a function of the magnetic field. It was a pleasant surprise to find that the magnetic field dependence of the experimental life times agreed reasonably well with the theoretical expression obtained from the equation for $\Psi(1)$ using a modelled form of the experimental magnetic field configuration. This theory paper was promptly published in February 1971 in Physical Review Letters on the strength of the good comparison with the experiments.

While this was very satisfying indeed, the exciting thing, however, was that the other equations of the set predicted the existence of additional life times in the leakage of particles corresponding to n $\neq$1, which could also be looked for. No previous theory had even attempted to describe the experimental life times, not to mention any further predictions. Experiments were conducted at PRL subsequently (1979-1982) to look for these additional life times. Lo and behold! They were there upto n=3 with all the predicted characteristics. This was a triumph of a theory which started from an intuition, suggesting a rather heretical equation, a relentless search for whose derivation, finally led to something more. In the meantime the theory was given a sounder basis in 1985 by obtaining the same set of equations from the Liouville equation for the system under consideration as a Hilbert space representation thereof. This, however, further opened a pandora's box of questions as to why should a quantum-like formalism like this work in a classical mechanical parameter domain. And is there something deeper that one should be looking for? There was, however, another, even more spectacular prediction of these equations which is related, as in quantum mechanics, to the probability amplitude nature of the function $\Psi(n)$. Accordingly, charged particles moving along the magnetic field should exhibit one-dimensional interference effects, in the classical mechanical parameter domain. This was again a rather heretical prediction. Experiments carried out at PRL during 1988-1993 have indeed revealed, rather enigmatically, the existence of interference effects. It has not been possible to understand these experimental results in terms of the standard theory.

To summarise, I have been able to obtain a Schrödinger like description for a classical mechanical system (charged particle in a magnetic field). This is considered to be conceptually quite heretical because classical mechanical systems are not known to exhibit wave-like behaviour for single particle ensembles. The amazing thing however, is that the wave-like predictions of this formalism such as the interference effects, have indeed been experimentally verified. Furthermore, these astonishing results seem to defy explanation in terms of the standard equation of motion - initial value paradigm. These are entirely new results of a fundamental nature; and I consider them as my most important contributions.

## 10. Back to PRL

I had returned back to PRL in August 1971 after spending about two years at Austin where apart from the above mentioned theoretical work, I had also worked on *tokamak* plasmas with particular reference to their instabilities and transport processes.

Earlier when I had returned to India in November 1966 I had not given any thought to the question of staying over in the U.S.Returning to India and to my institution to which I owed unquestioned allegiance, was for me a matter of principle motivated by a sense of idealism. But the trauma that I went through at TIFR after my return had eroded some of that idealism though did not quite erase it. Therefore, this time around when I returned I was led to debate somewhat on the question of my return. Unfortunately, the residual sense of idealism again got the better of myself, but in a slightly different manner this time. First of all, my roots continued to beckon me back. Secondly, I argued that whatever I would do while in the U.S., teaching and research, would amount to a very tiny drop in the ocean of work being carried out there. The same amount of work carried out in India could, I hoped, be of more value to the Indian scientific scene, including the Ph.D. students that I may be able to guide.

Within barely four months since my return Sarabhai passed away, casting a pall of gloom over PRL and other organizations that he was associated with. An air of uncertainty hung also over the fate of the nascent plasma physics activity. We did not know what view the new director would take of the plasma physics.

Plasma physics, however, did continue after Devendra Lal joined as the new director, but not according to Sarabhai's plans, because nobody knew what they were. If it was to develop along fusion oriented course, under the umbrella of Department of Atomic Energy, then such a course for it was ruled out with the creation of the Department of Space, of which PRL became a part. At any rate, the plasma physics group began to evolve appropriately to the ambience, namely the space sciences, that it was placed in. Theoretical activity however explored all the three aspects, namely basic plasma physics - waves and instabilities, space, astrophysical and fusion related problems, while experimental work, which was initiated soon thereafter, was carried out to study instabilities of partially ionized plasmas, of relevance to the space physics and astrophysics. There were small scale experiments which could be designed comparatively easily and executed and were an appropriate choice to start an experimental programme with. Though simple by contemporary standards, they yielded valuable results. These experiments were carried out by an initially two member experimental group with a strong support of ideas, suggestions and discussions from the members of the plasma theory group.

In the year 1975, I directed a Plasma Physics Summer School at the hill resort of Saputara (dist. Dang, Gujarat) which proved to be a watershed in the evolution of plasma physics not just at PRL but in the whole country. We had invited a country wide participation in the summer school. The participants were introduced to current topics like "Solitons" and "Plasma Turbulence" and non-linear plasma theory. It turned out that these topics continued to be the focus of research by many groups in the country over the next decade and more.

## 11. In astrophysics

My flirtations with astrophysics began sometime around 1977, when I suggested to Dr. A.R. Prasanna, a relativist who had just joined PRL, that we study the orbits of a charged particle in the field of a magnetized black hole. With his background in relativity and mine in plasma physics, this would be an ideal problem to tackle. The idea in my mind was that we should eventually study plasma dynamics in the vicinity of a magnetized black hole with a view to application to the accretion disks. We studied these orbits rather comprehensively, but for some reason the collaboration could not continue.

My next excursion into another astrophysical problem was to study the dynamics of gravitating systems, with particular reference to the galactic dynamics and the spiral structure of disk galaxies. I had been long fascinated by the dynamics of gravitating systems which are similar to plasmas in the long range nature of their interaction, but differ from them in that they all have the same sign of "charge". It was obvious that this would lead to many fascinating phenomena, peculiar to the gravitational systems.

The work on the spiral structure of disk galaxies was carried out with my student Ashok Ambastha and led to some very significant results with reference to the nature of the spirals, 'leading' vs 'trailing', identifying the conditions under which one obtains the generally observed 'trailing' patterns.

Apart from Ashok Ambastha, I had two more students who were working with me in theoretical physics at the same time, namely Nagesha Rao and Avinash Khare (Avinash Khare has since south-indianized his name to K. Avinash). The latter two worked on plasma physics problems, with Nagesha working on the non-linear entities, the solitons, and Avinash working on fusion related problems. The assignments of these diverse class of problems, to the three students was deliberate on my part as it enabled me to explore different areas with different students. At the same time, it kept me constantly on my toes, making me jump from one area to another. In addition, I also had another student Dhiraj Bora during the period who carried out experiments stimulated by the predictions of my theory on the existence of a multiplicity of residence life times in an adiabatic mirror trap.

I had two more students subsequently between 1984-1990 Sunil P.S. Rawat in experimental physcis and B.P. Pandey in theoretical plasma physcis.The former studied the charged particle motion in periodic magnetic fields related to my quantum like theory and the latter on the global galactic magnetic fields, a topic of great current astrophysical interest.

The period from 1976-1990 during which all the above mentioned students worked with me, though full of other problems, both personal as well as laboratory related, was academically a very engaging and a very satisfying period. The major part of the credit goes to these students of mine. In the work carried out with each one of them we were able to get some very new and fascinating results. Today each one of them is an accomplished physicist in his own right and it gives me a great sense of pleasure and satisfaction to see them perform scientifically as mature physicists. I consider them as my ultimate "awards"

The year 1979 was a very eventful year in the history of PRL and to some extent in my life as well. Until around 1978 things were flowing somewhat smoothly at PRL except for occasional hiccups, which are not unusual in any institution. There are grievances that staff members air from time to time. They are both genuine as well as manufactured. Many a time manufactured grievances are a consequence of the ambition of a few individuals of the staff to play a leadership role. While genuine grievances are generally acknowledged and can be taken care of, the manufactured grievances have obviously a way of proliferating and can in no way be redressed. However a lack of communication with the members of the staff can lead to misunderstandings and a lack of appreciation of each others point of view. The ground is then ripe for the generation of spurious grievances and the exploitation thereof. The year 1979 saw PRL plunge into a state of chaos as a result of the agitation carried out by some staff members over such grievances. The state of agitation which continued for almost ten years altered the face and character of PRL for ever.

At the end of the 1977 Plasma Physics College organized by the International Center for Theoretical Physics (ICTP), I was nominated to be a co-director for the Plasma Physics Colleges for the years 1979 onwards. The running of the 1979 Plasma Physics College at ICTP was an exhiliarating experience, which gave me a rare opportunity of interacting with an international community of plasma physicists, both the young ones and the senior ones. It was a particularly pleasant experience to be amongst the enthusiastic young plasma physicists from developing countries.

I continued to work as co-Director of the subsequent Plasma-Physics College until the year 1985.

## 12. The Directorship years

In September 1985, just after my return from a conference in Italy, I was quite suddenly asked to be the Dean of the Faculty which came to me as a surprise. Even more surprisingly, I was appointed the Deputy Director from January 1986. It put me in a dilemma, whether I should accept it or not. I could have, of course, declined, but that, I thought, would reflect poorly on me, as appearing not to shoulder a responsibility which was being assigned. My pleasures, however, lay more in the scientific works than in the power and prestige that a Directorship appears to bestow. Nevertheless, when the then Director Dr. Pandya decided to go on leave for six months from April 1, 1986, and appointed me as Acting Director, I had no alternative but to serve.

With the Laboratory already in the throes of the Union-led turmoil I faced one agitation after another during the six months period that I was the Acting Director. There was no room for any scientific activity for me which came to a standstill. I heaved a sigh of relief when this period ended on October 1, but clearly not for long.

As I should have suspected, the Directorship of PRL was offered to me in May 1987. While I felt greatly honoured at being reposed such a faith and confidence, I was definitely uneasy. The problems that I was to inherit were gigantic though not insurmountable. PRL was at that time a rather disturbed institution. But my uneasiness was due to something more than that. I felt strongly that my scientific career would be severely jeopardized just when I was engaged in some very fascinating fundamental problems in quantum mechanics and classical mechanics. At the same time I did not want to let down those of the Council members who saw me worthy of this onerous responsibility. I also thought that if I can bring PRL back on rails, and be able to create a kind of sizzling scientific environment, I would get a different kind of 'satisfaction'. So with some trepidation I accepted the offer and took over as Director PRL on June 1 1987.

That was a very crucial decision of my life. There lay eight years of my career ahead of me, and I had to make a choice between two alternatives: To spend this valuable time in the hope that I would be able to do something for PRL, to bring it back to a vigorous state of health, however heavy may be the odds. Or a somewhat self-centric course, namely to spend the remaining eight years, continuing to enjoy doing science. While the result of the latter course was certain that I would certainly do science and enjoy it, but that of the former was not at all so. Would I be successful in whatever job was ahead of me? Would I be granted a sense of satisfaction after all this? It was not at all clear. I nevertheless, chose the former course, which was full of uncertainties and pregnant with the possibilities of eventual disappointment and disillusionment. Knowing all this fully well, I had chosen this course, may be in the fond hope that I may be given some credit for bringing PRL back to health.

It was quite obvious that this directorship would not be a bed of roses, and a far cry from the status that Sarabhai, for example, enjoyed as the director of the same laboratory. The status of the directorship at that moment in time stood seriously eroded. Given all the problems that the laboratory was engulfed in, I found myself severely handicapped. It was a very serious challenge indeed. However having accepted the position, I took my job with full determination. I was, however, fortunate to have with me the goodwill of a large number of my colleagues, and I was confident that with their help and support I would be able to carry out the difficult tasks ahead.

Within two months of taking over I had a taste of the Union-led agitation over the matter of holidays in PRL. The Union members carried out a most unbecoming campaign of verbal terror and violence directed against me (as also against the previous directors) which unfortunately the courts do not recognize as violence, but rather a fundamental right of the unionists to be used as an instrument to press for their demands, however irrational or illegitimate. I, however, consider it as an ugly blot on the norms of society which considers itself civilized. An institution which is supposed to be a seat of learning and a centre of excellence and whose dignity should have been upheld had been brought down to the level of a fishmarket, all in the name of 'social justice' which to my mind had not been so badly compromised as to invite such degradation of values.

A strong affidavit which I personally drafted in order to arouse the conscience of the courts against such an uncivilized behaviour brought forth some results. The agitation was stopped and the Union members who till then thought that they could get away with anything were now restrained but they were certainly far from out.

There, however, remained two major demands of the Union. One was the "reinstatement" of the five dismissed employees whose cases had been lying in the courts. The second was the case of a former Union leader who had been claiming a right to a permanent position in continuation of his two years of temporary position as a post-doctoral fellow.

Both the problems were rather sticky. There was the apprehension among the faculty that if taken back, these five dismissed employees could indulge in the same kind of misbehaviour for which they were dismissed in the first place. I personally did not share that apprehension given the overall state of affairs at that time. Some Union leaders had begun to see the futility of continuing the agitation. Nevertheless, one had to ensure that it would not happen. One had therefore, as a precondition, to get from them a letter of apology for their misbehaviour, and a promise that they would not indulge in the same kind of activities in future. With some hard bargaining lasting over two years the matter was finally settled in June 1989.

No such thing was possible for the second case which had much more serious spatiotemporal implications. The case had been pending in the Gujarat High Court. If we lost the case, it would have had serious implications not only for PRL but for similar other institutions in the country. Again, after a lot of hard work involving myself, our administrative chief, our legal cell and our lawyers, we were finally able to win our case in the Gujarat High Court. An appeal in the Supreme Court against the judgement was also subsequently dismissed.

I heaved a sigh of relief as also many of my colleagues. Finally, the most difficult cases facing PRL were resolved after more than two years of rather hard work over these extra academic issues. Even so, the academic work had to continue, as it was the *raison dètre* of PRL and the director's main responsibility to direct it. I had more time now to devote to the academic affairs, however.

There were two important issues which needed urgent attention. One was the reorganization of the scientific areas of PRL. This was necessitated by two evolutionary features of PRL at that point in time. One, (i) that all those scientists who joined PRL in its early years (in the fifties) were due to retire by about 1990. The areas of activity represented by them would then either cease or at least go through a major reorientation upon their retirement. Secondly, (ii) major changes of emphasis were taking place internationally in the different areas of PRL's activities. For example, while in the sixties and seventies, the ionosphere (E and F regions) was the focus of attention in aeronomy, the middle atmosphere had begun to attract the attention of the scientists globally, through such programmes as the MAP (Middle Atmosphere Programme). In the upper atmosphere studies also the attention was shifting towards the studies of the neutral component (-the thermosphere).

In view of these exigencies, the activities of PRL required a major reorganization, both in terms of their nature as well as the groups representing them. In the aeronomy area, fortunately, the middle atmosphere studies had already been initiated in the early eighties. These needed to be further expanded and strengthened. Thus, over the next eight years, major strides were made in the development of the programme for the studies of the minor constituents whereby a cryosampler was fabricated and flown successfully. A lidar system was also acquired for the studies of the aerosols in the stratosphere, and has been fully in operation since 1991.

A new programme involving the studies of the neutral atmosphere using the optical techniques was also initiated during the period. In a sense, it was a revival of the old programme of air glow studies, but now with vastly improved techniques developed at PRL, namely day glow spectrophotometry, it gave spectacular results which attracted wide global interest and attention.

The area of Astronomy and Astrophysics was a relatively new area in PRL, and it still needed a lot of nurturing, both in terms of the development of back-end instrumentation as well as its scientific manpower. The development of an Infra-Red Observatory which was initiated during the Directorship of Devendra Lal, was still far from complete. When I took over as Director, while the mechanical and the control parts of the telescope were almost complete, the 1.2m mirror still awaited finishing.

Unfortunately, when the mirror did arrive from Bangalore around Dec. 1987, and was installed on the telescope it behaved more like a light bucket, giving a resolution of ~6". However, this could be either due to the intrinsic poor quality of the mirror or due to distortion in it because of the possible faulty support system, or both. It was not possible to eliminate any one of the causes, without examining each of them in detail. To be sure, there was a bad structural patch in the mirror near the cassegrain hole, which defied rectification. But it was claimed by the IIA team that this patch could not have been responsible for the poor resolution displayed by the telescope, and that the overall resolution of the mirror was around ~3"; therefore that there must be support-induced distortions in it which have contributed to the degradation in the resolution. After a series of meetings and deliberations among the ISRO engineers (who had fabricated the mechanical structure of the telescope) and the IIA team, it was felt that the mirror should be tested independently for its possible defects, because, according to the estimation of the ISRO engineers, even the maximum distortion in the mirror due to the support system could not have caused so much degradation of the resolution.

Consequently, I requested Roedrick Willstrop of the Institute of Astronomy, Cambridge to visit PRL and to make a thorough examination of the mirror for its possible defects. A series of tests carried out with the Hartman screen revealed severe problems with the mirror which were indeed the ones responsible for the poor resolution.

In order that I am not misunderstood in relating the above episode I must add that the primary reason for the muddle was the unusual nature of the blank that was procured for the mirror. It was, for those times a rather nonstandard blank with a 10:1 aspect ratio (a thin blank) as against the standard 6:1 aspect ratio blank. Since the PRL blank was a "thin" one, it required a much more careful grinding. Furthermore, it later turned out that this Cervit blank was not only harder than a Zerodour one but had probably uneven hardness. Hence it seems to have caused more than usual problems.

There was a log jam. The mechanical system had been designed for the thin mirror and would require major refabrication work if we were to change to the 6:1 aspect ratio mirror. This was thus ruled out. Should we then acquire a new 10:1 aspect ratio Zerodour blank and get it ground and polished? Since this was a nonstandard dimension it was to take a minimum 3-4 months to procure such a blank. In order to save time (and also money) we finally took the decision to send out the same old mirror for regrinding and polishing if some optician could be found who would do the job. Luckily, we were able to locate one, Sinden and Co., in Cambridge who did the job superbly and gave us an excellent mirror with a seeing limited 2" resolution.

The telescope was finally commissioned in November 1994 after an unusually long wait. The compensation was that eventually we had a high quality performance telescope. I chose to give the above mentioned narration of events so as to share my experience with others if it could be of use to them.

Making a telescope is a hazardous business, particularly in today's fast moving scenario. Time is of the utmost importance. A delay is expensive on two fronts: First, it delays the opportunities to the workers that they have been waiting for. Second, and perhaps more serious, it tends to make the telescope more obsolete by the time it is commissioned. One has to be extremely careful in venturing to underake telescope making. Delays are inevitable; but they can be absorbed only if the dimensions and the designs are futuristic. With large telescopes (~8m) already in the offing on the world scene and with the "active" and "adaptive" optics already signalling the state of the present art, quick decisions and urgent actions are extremely important. A strong case can be made out for a 4m class of telescope with "adaptive" optics, for India provided it can be installed in the next 5-6 years.

All the time, however, while the commissioning of the telescope was awaited, the PRL astronomers had been fabricating a whole set of back-end instruments. To date, the list includes (1) IR photometers, both (i) 'continuous variable filters' (CVF) and (ii) millisecond response fast photometer for occultation work. (2) Fabry-Perot interferometer, both (i)'central aperture scan' and (ii) imaging. (3) Polarimeter both (i) optical (UBVRI) and (ii) Infrared (JHK), and (4) CCD camera (optical). An IR-CCD is in the process of being fabricated. Also in the process of being acquired are an IR Fabry-Perot (resolving power 5000) and an IR - camera with a grating spectrograph (resolving power 1000) NICMOS-3 in 1-2 $\mu$ range.

In the absence of their own telescope the PRL astronomers had to use other telescopes in the country: the Vainu-Bappu telescope (VBT) at Kavalur, and that of the U.P. State Observatory, Nainital were frequently used for which the required back-end instruments had to be transported back and forth. With the commissioning of our telescope at Gurushikhar, the PRL astronomers have now been able to carry out their work much more thoroughly and systematically.

I should perhaps add a comment which pertains to my views on the difficulties that the astronomy research in India is faced with. I think it will be fair to say that GMRT is the only world class astronomy facility in India today. But in both IR and optical we are far behind. However, it is not as if we cannot do good astronomy with what we have, namely the VBT and the Gurushikhar IR telescope, for example. But the important question is that how 'visible' that research is going to be on the international scene. We should either be able to make some 'news' or be able to cover a particular set of areas thorougly through solid systematic contributions involving both observations and theoretical modelling. The "newsworthy" research generally belongs these days to the 'big league' which work on the frontiers, like 'large scale structures', or gravitational wave astronomy, etc. But there is, on the other hand a lot of interesting astronomy (and astrophysics) which belongs to the second class. The problem of star formation is an area which comes to mind. If one could have interinstitutional groups which could work on some such chosen, potentially interesting fields, then the Indian astronomy community could make a substantial mark on the international scene for its solid contribution. Unfortunately, a series of unrelated and unconnected contributions by individual workers do not add up to very much in terms of international impact.

Two areas constituting the Earth Science Division were earlier designated as "Geocosmophysics" and "Archaeology and Hydrology". The latter was subsequently changed to Continental Paleoclimatology in view of the shift of interest of its members. It was also clear that Paleoclimatology was emerging as a very important discipline in view of the general concern of the world community about the global climate change due to natural and anthropogenic factors. It was now widely appreciated that climate needs to be understood, as to how it changes on various time scales and what causes these changes. Clearly this was a discipline which needed to be encouraged and strengthened.

The 'Geocosmophysics' area was a conglomerate of several subdisciplines, which could be named as 'aqueous geochemistry' (the studies of the trace chemical loadingsof rivers and oceans) 'oceanic paleoclimatology', chemical oceanography and 'meteorites and solar system evolution'. Besides, there were also a few scientists who studied glaciers and deserts and some others who studied geological processes as well as gechronology.

There was thus a very clear case of reorganization of this division into areas of study which were more coherent in their purpose and approach as well as focussed on futuristic issues. It was thus reorganized into two areas:

i. Climate studies and Oceanography
ii. Solar System studies and Geochronology

Two major thrusts were identified. Geochronology was to emphasize the understanding of the Indian subcontinental geological evolution. Both these areas are of highly futuristic potential, which are opening up to vast unexplored territories. A recently found connection between the Himalayan orogeny and the global climate change triggered by the Tibetan uplift adds excitement to such studies.

The Solar System studies in the past concentrated on the early solar system processes whose signatures lie buried in the meteorites. However, there lies vast scope for expanding these studies to cover the formation of solar system bodies - the planets and their satellites, besides their physical features and their environment. It was with such futuristic projections of their activities that these two areas were constituted.

A further synthesis at the inter-divisional level was also envisioned whereby the 'Climate studies' group of the Earth Science division could fruitfully collaborate with the Atmospheric Science division as the middle atmosphere studies also have a direct bearing on the climate.

The Theoretical Physics Division which had strong groups in plasma physics and nuclear physics until the seventies got drastically reduced when a major part of the plasma physics group moved away to constitute the Institute for Plasma Research. It was a severe blow to plasma physics at RPL. It became necessary to redefine the nature and scope of the theoretical physics division, in view of the changed scenario and the emergence of some new disciplines on the international scene.

We already had an experimental group in Astronomy and Astrophysics. A larger astronomy scenario includes such highly current and futuristic topics such as Cosmology and Large scale structure formation in the Universe. A particle physics phenomenology group was thus inducted with this view, which was to expand later to include the other related disciplines.

With the increasing complexity of scientific research today and its multifaceted character, it is widely recognized that a group activity is not just desirable but imperative, particularly in the experimental areas. To make any meaningful advance, a group of scientists have to explore the various aspects of a problem in a complementary fashion and arrive at "results" which represent a dent. One needs a radical departure from the past practices where invidual scientists made isolated measurements, which in today's scenario, however, do not add up to much, unless they pertain to the same system. A coming together of scientists is therefore essential. Unfortunately, the latter represents a problem of great complexity namely that of human dynamics, with all the clashes of egos, territories and self-interests. A recognition has, however, to dawn on the scientists that the group interest automatically covers individuals' interests. It is easier said than achieved, however. My attempts at forging grouping was at best, only a partial success.

## 13. Some reflections

One may ask me, as to how I feel after serving as a Director for eight years. I have often reflected over the events, and over the state of being a director over these eight years: What have been my achievements as a director? Has it been all worth the effort which took up more than a fifth of my professional life span? I am well aware that external perceptions may differ from my own perceptions. Nevertheless I must record my own as objectively as is possible for anybody to be, about himself.

Given the state that PRL was in at that time, few people would have been envious of my position. The most immediate task before me was the restoration of peace and tranquility at PRL. The other task, the central task of a director, was to carry out a scientific rejuvenation of PRL and to reorient and reorganize its scientific activities so as to bring them in tune with the current and futuristic trends.

While the trauma of agitation that PRL went through did affect its efficiency and work culture, the scientific activities also did not remain unscathed. The agitation had vitiated the entire scientific ambience. The colloquia and the seminars which were often disrupted by agitationists had also dwindled with time. The faculty, however, continued to make heroic efforts to sustain the scientific activities in the face of these adversities. My task as a director at that crucial juncture was to negotiate a tough corner and bring PRL from the bumpy road on to a relatively smooth and a peaceful one and to steer it scientifically towards current and futuristic directions.

Anyone who visited PRL during the agitation period would testify, that the first of these tasks, trying and difficult though it was, has been fully carried out. It took all the ingenuity, diplomacy, and negotiating skills that I could command (not having had any primary training in any of these items) and of course the complete cooperation of my colleagues both from the faculty and the administration. It was a no mean task, I can say today, and the one that I would have neither the courage nor the energy to undertake again. I am afraid not many would appreciate today (and much less so in future) the importance of that effort.

On the scientific front also- (the next important item of the agenda-), PRL was at a crucial juncture when I took over as director. My task was two fold. One was to rejuvenate it in terms of the restoration of seminars and colloquia and day to day scientific discussions which had dwindled drastically over the years. But the second, a more important one with a long term perspective was to reorient and to reorganize the scientific areas, so that PRL should be poised for a take off on a futuristic course in scientific research activities. This goal has been achieved substantially with respect to the various divisions as has been already elaborated upon. The directions have been clearly delineated. In the process, the activities have been made more coherent and purposeful, encouraging the scientists at the same time to work in a complementary and cooperative mode in order to make meaningful contributions to the respective areas of research. What is needed now is pursuing these directions with vigour and the addition of more scientific personnel to provide the necessary strength.

What about my own scientific contributions? Do I feel a sense of satisfaction about my scientific achievements whatever they are? My frank answer is 'not really'. I feel that I could have done much more. And I do not mean it in terms of my publication tally, which I never attached too much importance to; but in terms of translations of my ideas into concrete work. There are many reasons for this, both personal and institutional which it would be unwise to elaborate on. Even so I have some satisfaction in seeing some of the works having been recognized. One of them has, in fact, led to my name being attached to the equations that I gave and to the 'mode' that they predicted.

Yet my most favourite work is not this one but the one which even after 25 years of publication, and extensive experimental verification remains largely unappreciated. I have already described it earlier. I consider it to be my real life time creation whose importance has not been understood by the scientific community so far. But if it is examined in a true scientific spirit without any prejudice, its importance can hardly be ignored. Notwithstanding this lack of recognition, I am happy that in my life I have been able to unearth something which is unusual and new! So I have no regrets.

But I do find it astonishing and disappointing that an experimental result (already published in an international journal), so interestingly unusual and surprising and crying for attention has so far not attracted the attention and scrutiny of the Indian scientific community (barring a few exceptions) inspite of having been repeatedly brought to its notice. I emphasize "the Indian scientific community" because of our lament *ad nauseum* of the lack of creativity. And yet if something appears on the scene which gives a strong indication of being 'new' does not get the attention that it deserves. It is not my case that any result expected or unexpected should be accepted as such. But there is a strong case for a thorough uncompromising scrutiny of a result that promises to be new. After all how often do we encounter a result that is so unexpected.

There are, however, other disappointments that I have felt in terms of attitudinal trends towards the pursuit of science generally in the country as well as at PRL. I do not know whether this trend is more widespread, but it has certainly been happening at PRL over the last many years. There was a time when there used to be intensive interaction not only among the scientists of a given discipline, but among scientists across other disciplines also. Such discussions were promoted through the agency of colloquia and seminars which were largely attended earlier. Scientists, young and seniors, took them seriously and asked penetrating questions, which naturally encouraged discussions. Such discussions are a sign of the vigour and vitality for any scientific institution. Unfortunately, over the years the attendance in the seminars and the colloquia has been declining, and post seminar discussions have been fewer if at all. Scientists tend to attend seminars largely only in their own field. The general scientific base of the scientists has been shrinking and getting more and more specialized with time. There is a definite attitudinal change that has occurred and has been occurring. People are not interested in knowing what the others are doing. The emphasis is shifting on one's personal acheivements in terms of number of papers published and presented in conferences, rather than creating a healthy scientific group environment at the place of work.

This does not augur well for the long term health of science. After all, the papers that one writes and publishes are meant to be read by others who may find them either interesting or useful. But the subset of scientists who find what others publish as interesting is shrinking, because fewer people are motivated to read others' papers unless it is of direct relevance to their own work. With the specialization proceeding at the rate that it is, then in the limit, expressed in a lighter vein, the author will read only his papers. And yet the volumes of research journals will continue to grow!

There is, however, another even more disconcerting trend which is in evidence today. The world of science does not appear today to be governed by the ideals of science, like objectivity, truth and honesty - the virtues that my good old teacher imbibed in me. It now stands divided into cliques and vested interest groups who guard their domains very jealously. None can enter their domains unless one conforms to their point of view. If one dares to differ from that "canonical" formulation propounded and propagated as the "truth" by the particular group, you will have a great deal of trouble waiting for you to get your papers published in "respectable" refereed journals.

What kind of science can you then do? Follow the canons? Should one then do science at all? One chooses a career in science for the pleasure of unearthing the beauty of nature in the spirit of an objective inquiry. It would be much simpler to be a priest than to become a scientist if canons are to be followed. One is at least honest about it.

Of the Indian Science scene itself, there is a sense of dissatisfaction among many Indian scientists that not all is well with Indian science. Why is it, they ask, that not many original ideas germinate on the Indian soil? In fact, not even countably few after almost fifty years of rather generous science funding. Some have argued that it has not been generous enough to be able to compete against a highly competitive western science. But it can be counter argued that the ideas do not require huge funds, nor does their feasibility demonstration. Their final implementation may do.

I believe that the real malaise lies elsewhere; in our very attitude to science and in our identity as scientists. Do we for instance, start our careers with enough self-confidence in ourselves that we can be as creative as those in the rest of the world? If we did, then we would strive towards that achievement either individually, if we could, or as a collective of Indian scientists. It is compulsive to compare the scientists of the yesteryears with those of today. There are many more scientists today than there were during the 30's (say). The science base is much wider today, and there is tremendous technological progress in space and atomic energy. Yet there is something amiss. The creativity of the yesteryears (Raman and Bose era) seems to be sorely missing. Creativity is intimately tied to one's sense of identity and individuality. If identity is diluted creativity is the sufferer. Individuality has to be nurtured from the very young age, and a sense of one's identity developed. Conformity, which is the order of the day, annihilates both. The whole education system of today which has become highly competitive in a rather perverse way is in fact a competition to conform - to conform to standardised knowledge and information. There is no room left for imagination and to be different.

Our identity as individuals and as a nation is being continuously diluted and compromised through our own follies and a lack of a long time perspective. We are continuously working towards dissolving our scientific identity through overdependence on western patronage, including material support and approbation. We have failed to evolve our own scientific code of ethics.

Continued (over) contact and collaboration with the foreign scientists ties us down to their ideas, line of thinking and approach and traps us in the happy illusion that we are working on the latest and are therefore on our creative best. The fact of matter, on the other hand, is that it leads to a surrender of our individual's identity and hence to a loss of one's own personal creativity. The tragedy is that one is usually not aware of that loss.

On the other side, it is only very rarely that a foreign collaborator will give you credit for any orginality that you may have displayed in a collaborative work. G.H. Hardy was an angel! Moreover, the late twentieth century is not the early twentieth century.

This is of course, not to suggest that we must abandon contacts with science and scientists abroad. Not only is it impossible in today's globalized environment, but in fact suicidal. Scientific work anywhere in the world spurs further work elsewhere. Windows of opportunities and lines of communication must be kept open. But one has always, to remember that the germination of creativity at home is to be the final objective. A parallel is worth mentioning: Without the exposure of science from the west there could not have been any Ramanujams, Ramans. Boses, Sahas and Sahanis. But they became those persons because I believe that they had something to prove. Can we bring back that kind of creativity? I am not suggesting any answers but we must THINK.

The other concern about future of science that I have is the public (and I mean here eventually the governmental) appreciation of science. To the public and the politicians on whom the support of science depends, science only means deliverance of certain results which can be directly perceived and utilized, like communication, computers, lasers, solid state electronics and a host of other publicly visible items. Unfortunately, the hard work of pure scientists that made all these things possible is not highlighted. I find it absolutely horrifying when even (some) scientists themselves start saying that scientists (meaning, of course, {other scientists) should cater to the public interest.

Science, it is rarely realized, is a serious and a hard job. It is not always that the search for truth about nature would yield dividends of public interest. Of course, a number of times it has been so translated. But there cannot be any future in such translations, if the search for truth is halted or constrained. It is not my case that public interest be ignored. But that regime belongs more to applied scientists and technologists than to pure scientists whose only reward is the pleasure they derive from making new and unexpected discoveries. To demand of them that they cater to public interests is to constrain their natural flow and thus stymie their creativity. A society cannot afford to do that for its long term survival as a sovereign, self respecting nation.