Patrika

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Golden Jubilee Meeting

At the invitation of the Indian Institute of Science, the Academy held its 50th Annual Meeting at the Indian Institute of Science, Bangalore from 6 to 8 February 1985. Originally scheduled for 5 days from 7 to 11 November 1984, the meeting had to be postponed due to the tragic death of Prime Minister Indira Gandhi on 31 October 1984.

The inaugural function of the Golden Jubilee Meeting was held at Chowdiah Memorial Hall on the morning of Wednesday, 6 February 1985. Before the beginning of the meeting Prof. S. Ramaseshan, President spoke of the tragedy which befell the country last October and took away prematurely a great Prime Minister, who had done so much for science and the country. All those present stood in silence for a minute in honour of her memory.

Prof. Ramaseshan in his introductory remarks said that it was particularly appropriate that the Golden Jubilee Meeting of the Academy was being held at Bangalore, at the invitation of the Indian Institute of Science, where the inaugural meeting of the Academy was held fifty years ago on 31 July 1934. The Founder President of the Academy was also a Director of the Institute. While discussing where the Academy should be situated, Prof. Raman had written "The strongest claim for Bangalore being the seat of this All India Academy arises from the location in it of the Indian Institute of Science. This Institute being devoted to research work in several fundamental and applied sciences, has the advantage of possessing an unrivalled science library... Its staff and students come from all parts of India and the Institute exists to serve the needs of the whole country".

Prof. Satish Dhawan, Chairman of the Reception Committee, a past President of the Academy and a former Director of the Indian Institute of Science, welcomed the delegates. He said that science had all along retained its unique quality and active concern for the spirit of enquiry. Its strength, beauty and integrity were incorruptible. The Academy in its fifty years of life had tried to strengthen these with success. However, despite the revolutionary developments in science, especially in the last 100 years, mankind was yet to fully enjoy its fruits. Science could preserve its vital and unique place in society by educating the young and encouraging enquiry through international understanding.

Prof. S. Dhawan welcoming the delegates.

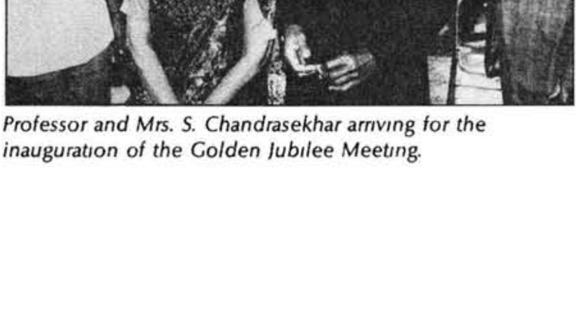
Prof. C. N. R. Rao, the present Director of the Institute and one of the Vice Presidents of the Academy next welcomed the delegates and spoke of the unbreakable bond between the Institute and the Academy.

Prof. T. S. Sadasivan, who took over as President in 1971 after Prof. Raman's death, released the illustrated volume on the "Indian Academy of Sciences—the first fifty years". He spoke of the Academy and its past history and hoped Fellows will work together to make the Academy greater than before.

The five Foundation Fellows, who were present, were next introduced to the audience by Prof. Ramaseshan and were garlanded by Prof. M. G. K. Menon, a former President of the Academy. They were Prof. S. Chandrasekhar, Nobel Laureate and distinguished astrophysicist, Prof. K. S. G. Doss, a pioneer in sugar technology and electrochemistry, Prof. V. V. Narlikar, distinguished mathematician, Prof. K. Ramiah, an outstanding pioneer in rice research and genetics and Prof. M. R. Siddiqi, mathematician and theoretical physicist and now the President of the Pakistan Academy of Sciences.

Messages from the Presidents of the Royal Society, the Indian National Science Academy and the Pakistan Academy of Sciences and from Dr. S. Varadarajan, former President of the Academy were read by Prof. M. G. K. Menon.

Prof. Menon, who next spoke, said that India had a long and distinguished tradition of science. Although this tradition "fell into disuse" during colonial rule, the past one century had seen a renaissance in science in India. Prof. Raman was one of those who ushered it in. The need today was to develop a reservoir of talented scientists. He said the scientific community should be grateful to the



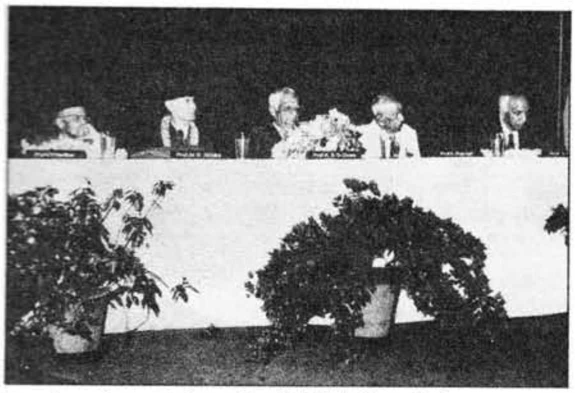
Professor and Mrs. S. Chandrasekhar arriving for the inauguration of the Golden Jubilee Meeting.

Prof. M. G. K Menon, Prof. S. Ramaseshan, Prof. C. N. R. Rao and Prof S Chandrasekhar at the inaugural function.

political leadership of the country for its significant support to the pursuit of science. Although these were long-range policies of Prime Ministers Nehru and Indira Gandhi, it was for the scientists themselves to see that science grew in the country. Since science had become highly professionalized and diversified the responsibility now rested more on the institutions and the scientists to promote it. He also called for more efforts to involve and encourage the youth in scientific activities and stressed the need to maintain excellence in scientific pursuits.

Prof. S. Ramaseshan before delivering his Presidential address on "New concepts in the architecture of solids", (summarised in the present issue), spoke of the Academy and its achievements in the last fifty years, such as the election to the Fellowship of outstanding young scientists, the promotion of high quality journals for prompt publication of research work by Indian scientists, conducting symposia, seminars and discussion meetings in order to provide a means of exchange of scientific knowledge amongst scientists and to bring this new knowledge to the attention of the whole scientific community, giving special encouragement to young scientists and their selection as Young Associates, and the institution of special chairs such as the Raman Chair. It was the dream of the founders of the Academy that "The Academy will be a company of thinkers, workers and expounders comprising members of the New Estate, upon whose achievements India must in future depend for its advancement and preservation and whose aim should be to promote the progress and to uphold the cause of science in both pure and applied branches".

Speaking of the future, Prof. Ramaseshan was confident that in India science and technology would flourish in the next 50 years. When the Academy was founded the amount of scientific research carried out in this country had been small, but in many of



Five Foundation Fellows (Prof. V. V Narlikar, Prof M R Siddiqi, Prof K S. G. Doss, Prof K. Ramiah, Prof. S Chandrasekhar).

the areas pursued, the contributions made were very significant. The work of C. V. Raman, S. N. Bose and others affirm the truth of this statement. The physical environment in which science was done in India was in a way comparable then to that anywhere else in the world. A great deal of expensive equipment was neither needed nor available, and communication was equally slow in India as in Europe. Scientists found out about each others' work by mail or through journals, so that the time scales involved were about the same. The intellectual environment was naturally different in different parts of the world, and when this was recreated here by some great mind, outstanding science seemed to follow.

He was proud that the Academy had helped in promoting a suitable environment for the promotion of science. He regretted that Indian Unversities' contribution to science had fallen in the recent past. Jefferson long ago had said of universities that "universities and academic institutions commit their pupils to the theatre of the world—with just enough learning to be alienated from the industrious pursuits and not enough to do service in the ranks of science". The Academy, the Government and all of us have to concentrate on improving the structure and function of the universities in order to bridge the gaps that exist in many areas of basic research and technology.

Prof. Ramaseshan introducing Prof. Chandrasekhar, spoke of how he started the pursuit of science at a very early age. During the next five and half decades he attempted and solved some of the most difficult problems in physics and astrophysics. "He has always cleared up every field he has taken up to study. He brings to bear his mathematical powers, invents new mathematical techniques on whatever problem he works on, whether it be in stellar structure, stellar dynamics, the interaction of radiation and matter, the hydrostatic and hydromagnetic stability or his most recent and perhaps his most significant work on Black Holes. Each study gives us a greater insight of nature and each of his studies has resulted in a magnificent book, classics which are benchmarks of his career. He is a savant *par excellence*. We feel privileged that he is here at this meeting, the most distinguished Fellow of the Indian Academy of Sciences".

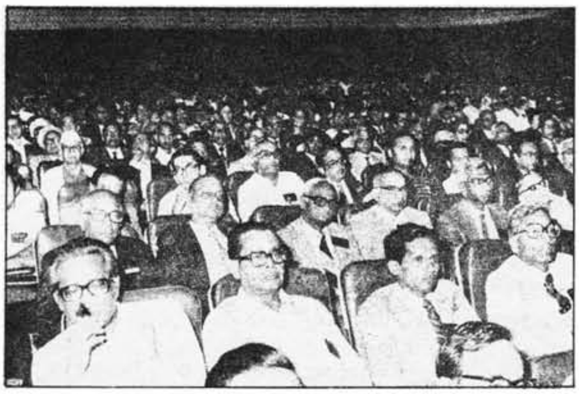
The Golden Jubilee lecture on "The pursuit of science; its motivations" is reproduced in full in the present issue.

There were two short specialized symposia, a series of four special lectures and six lecture presentations by new Fellows and Young Associates.

The first special lecture was given immediately after the inaugural function by Prof. M. G. K. Menon, on "Cosmic Ray Research and Cosmic Ray Physicists over the past fifty years in India". Tracing cosmic ray research and the contribution of cosmic ray physicists in the country, he recounted the work of great pioneers like Bhabha and Sarabhai in the field. He spoke of the work done with high altitude balloons and in deep mines. He spoke of how Indian contributions to cosmic ray research will soon grow to a significant proportion, when a large 400 ton detector for the observation and recording of the decay of nucleons is commissioned in the mines of Kolar, over 2500 metres below ground level. A few confined events suggestive of nucleon decay had been recorded at Kolar and if confirmed would add to the development of a new unified theory of forces.

In the afternoon the bust of the late Srinivasa Ramanujan, was unveiled by Mrs Lalitha Chandrasekhar at the Raman Research Institute. An account of this is given in this issue.

Prof. Menon garlanding Prof. Chandrasekhar.



Part of the audience at the inaugural function.

The first **Symposium on Animal Communication** was held on the morning of Thursday 7 February 1985.

Prof. Madhav Gadgil of the Centre for Ecological Sciences, Indian Institute of Science, introduced the subject. He suggested that in animals, the functioning of communicatory exchanges based on normal physiological changes is the mutual monitoring of the state of well being. Animal communication can be classified into four categories on the basis of how it serves the interest of the signaller and the recipient: when the interests of both parties are served, the communication is termed as an instance of mutualism; when the signaller benefits at the cost of the recipient, it is a case of deceit; when the recipient benefits at the cost of signaller it is a case of eavesdropping. The fourth possibility when both parties suffer is termed spite. It is debated whether spiteful communication exists at all, whereas instances of mutualism, deceit and eavesdropping or elements of the three have been clearly observed in the social behaviour of free ranging groups of tame elephants. The clearest cases of mutualism are recorded in case of communications amongst genetically closely related individuals such as elephant calves and their mothers and allomothers when compared to exchanges between adult cows. Altruistic behaviour in elephants is now believed to subserve the function of enhancing the inclusion fitness of the individuals concerned.

A logical extreme of similar behaviour was described by Dr. Vidyanand Nanjundiah of the Molecular Biology Unit, Tata Institute of Fundamental Research, in the case of cloning of genetically identical individuals. Single-celled amoebae multiply in numbers while the environment is good. When subject to environmental stress they signal to each other by periodically releasing expensive compounds like cylic A.M.P and aggregate to form a 'slug'. The slug differentiates into a fruiting body of genetically identical spores and a stalk of dead cells. The spatial segregation of the two kinds of cells is achieved in a proportion suitable for efficient spore dispersal.

On communication in colonies of social insects such as wasps and bees, Dr. Raghavendra Gadagkar of the Centre for Theoretical Studies, Indian Institute of Science, reviewed new evidence showing that insects were capable of discerning by smell the degree of generic relationship with not only their colony mates but also of other colonies.

Prof. M. K. Chandrashekaran of the School of Biological Sciences, Madurai Kamaraj University, discussed communication of 24-hour circadian rhythms in microchiropteran bats inhabiting a cave. Based on experimental work he concluded that circadian rhythms (such as the onset of dusk) were set by social communication (by means of calls) of activity rhythms by a few bats flying in and out, but such behaviour tended to 'freerun' or deviate from the normal periodicity when exposed to laboratory conditions. However, evolutionarily related species of bats appear to be able to communicate or synchronize rhythms at least partially when the frequency of their calls was similar, when compared to unrelated species.

The concluding lecture of the symposium on *Animal Communication* was that given by Dr. Stanley Rand of the Smithsonian Tropical Research Institute, Panama. With slides and recordings, he had documented various facets (loudness, pitch and frequency) of the mating calls of different species of frogs. The factors governing these calls include the need for males to advertise their own size but not to attract predators and the need to eliminate interference from other calls. Thus females using their own calls select males in their species with louder, more easily located calls utilizing the best resources, expecting to choose males who contribute the best genes to their offspring.

The second Symposium on the **Monsoons** was held on the morning of Friday 8 February 1985. Prof. R. Narasimha of the Centre for Atmospheric Sciences, Indian Institute of Science spoke first of the complexity of defining a system on the basis of known physical laws and mathematical techniques, and the size of the phenomenon which made meaningful data collection impossible. With recent advances such as satellite imagery and the adoption of a shorter time scale for data collection to study the climatic phenomenon on a continental scale, the probabilities of improving the predictability of monsoons were thought to be possible, provided a greater understanding of the basic physics and mathematics of it was achieved.

Prof. Sulochana Gadgil of the Centre for Atmospheric Sciences, Indian Institute of Science, described the 40-day cycle between the formation of the clouds, their migration over the sub-continent and their dissipation over the northern plains of India. The mode had tremendous potential for forecasting but the variations within a monsoon season and the total monsoon could be determined by a deeper insight into intraseasonal and inter-annual variations as well as the conditions on the surface of the land, such as deforestation which would lead to a decrease in the rainfall.

Prof. J. Shukla of the Centre for Ocean-Land-Atmospheric Interactions, Department of Meteorology, University of Maryland proposed that the day-to-day changes in weather are mainly due to the growth, propagation and decay of the synoptic scale disturbances which owe their origin to the instabilities of the larger scale atmospheric flow. Recent observational and modelling studies have suggested that although the synoptic scales lose their predictability within two weeks, the interactions on a global scale such as those between southern oscillation phenomena and 'El Nino' appear to be predictable upto a month or beyond. It has also been suggested that there may be additional predictablility due to the influence of the slowly varying boundary conditions at the earth's surface, particularly at low altitudes, the most important boundary forcings being sea-surface temperature, soil moisture, surface albedo and snow and ice.

This was followed by a discussion which concluded that an enormous task of data collection and processing was required beyond the conventional weather simulation models. Prof. Narasimha however pointed out that the dynamics of the precipitation seems to be unanswered viz., analysis of the annual rainfall pattern in one region (Bombay) showed that half the entire annual rainfall occurs over a cumulative time span of 24 hours. This seemed to call for a completely new explanatory theory.

There were three lecture presentations on the afternoon of Thursday 7 February 1985.

Dr. S. R. Gadre of the University of Poona spoke on "Electron density in chemistry". Over the past few years the density functional theory has evolved as a conceptually, and to some extent, practically useful method for studying the electronic structure of many-electron systems. Apart from providing a practical scheme for working out atomic molecular properties, the density functional theory has been useful in analysing various chemical concepts and should lead to a better understanding of several concepts in chemistry.

In his talk on the "Structure, conformation and charge density studies by x-ray diffraction", Dr. T. N. Guru Row of the National Chemical Laboratory, Pune spoke of x-ray diffraction techniques which have contributed immensely to the development of structural chemistry. Two major contributions are the structure solution of very large molecules like proteins, enzymes and viruses and the study of charge density distributions. The methods of structure solution are now very well established. The emphasis is therefore mainly on the possible chemical reactivity and its role towards better understanding of structure-activity relationships. Parallel developments in theoretical methods have also paved the way for interpretative analysis of charge density distribution maps.

Dr. L. C. Padhy of the Tata Institute of Fundamental Research, spoke on "Oncogenes: their function". This was followed by a special lecture by Prof. O. Siddiqi of the Tata Institute of Fundamental Research on "Neurogenetics of smell".

There were two more special lectures on Friday afternoon by Prof. C. N. R. Rao on "What maketh a metal?" and by Prof. M. M. Sharma of the University of Bombay on "Excursions into multiphase reactions". Multiphase reactions are ubiquitous and crucial in chemical industry and encompass a wide variety of systems. They are useful in the recovery of a variety of toxic and refractory chemicals from dilute aqueous waste streams. They are now acquiring importance in biochemical and electrochemical reaction systems where the introduction of an additional liquid phase bestows some important benefits.

The last three lecture presentations by new Fellows were by Prof. V. Krishnan of the Indian Institute of Science on "Biomimetic model reactions in photosynthesis", by Prof. G. Padmanaban on "Gene basis for drug metabolism" and by Prof. G. Srinivasan of the Raman Research Institute on "Spinning up a star".

Biomimetism implies duplication of functions performed by molecular entitities in biological systems either by modifying or isolating certain salient features pertaining to the real systems. Laboratory simulation of natural photosynthetic process is one of the most fascinating of problems. Research in this area has improved many known techniques and heralded newer methods and offer scope for studying multitudes of reactions involving many-electron transfers.

Prof. G. Srinivasan spoke on how certain pulsars on the sky that slow down in their spinning motion once again speed up, in cases where the pulsar has a companion star, which transfers its mass and angular speed to its partner. Several such binary systems had been found. The time taken for respinning the neutron star, which is of the order of a million years, depends on the rate of mass transfer and the magnetic field of the neutron star. Experimental evidence from four known binary systems had confirmed theory.

The Academy Workshop on **Supernovae, their Progenitors and Remnants** was held as scheduled from 29 October to 2 November 1984 at the Raman Research Institute. A brief report on the Workshop is published in this issue.

There were three evening functions all held at the Chowdiah Memorial Hall, Bharata Nrityam by Padma Subrahmanyam, Music by Smt. M.S. Subbulakshmi and Yakshagana by Shambhu Hegde and party. The Government of Karnataka hosted a banquet in Vidhana Soudha on 7 February 1985.

The Academy is grateful to the Indian Institute of Science and especially to the Chairman of the Reception Committee Prof. Satish Dhawan, the Director of the Indian Institute of Science Prof. C.N.R. Rao and to the Chairman of the Local Organising Committee Prof. R. Narasimha, and all the members of the various committees and sub-committees for the superb organisation of the Golden Jubilee Meeting, and to the many organisations who contributed generously towards the Golden Jubilee celebrations and made them possible. Our special thanks are also due to Prof. R. Narasimha and Prof. Madhav Gadgil for the organisation of two specialized symposia and to Prof. V. Radhakrishnan and his colleagues for the organisation of the Workshop on Supernovae, their Progenitors and Remnants held from 29 October to 2 November 1984.

It was altogether a memorable three days and all the Fellows and Young Associates and all the participants will long remember and cherish the warmth of the hospitality they enjoyed and the excellence of the arrangements. It was a unique Golden Jubilee celebration carefully and thoughtfully planned and beautifully organised.

Over 230 Fellows and 26 Young Associates took part in the Golden Jubilee celebrations. Photograph taken during the Meeting is reproduced on pages 16 – 18.

New concepts in the architecture of solids

From the Presidential address given by Prof. S. Ramaseshan at the Golden Jubilee Meeting on 6 February 1985.

Some of the most beauteous forms in which Nature expresses herself are crystals. One has only to walk through the Raman Collection to become acutely aware of this. These exquisite forms arise because of the apparent propensity of molecules or ions to arrange themselves in three-dimensional arrays or lattices.

It is this periodicity or translational symmetry which is the basis of x-ray crystallography. The lattice periodicity acts like an amplifier of the intensity of x-rays scattered in particular directions by the atoms and sharpens the diffraction maxima. The geometry of the lattice defines the positions of these spots while the molecular or ionic structure affects the relative intensity. A material which does not have translational symmetry cannot be a crystal; such substances are hence treated as liquids or glasses and expected to produce a diffraction pattern of diffuse rings. It is only because of the existence of translational symmetry in many solids that x-ray crystallography has made immense contributions to our understanding of the structure of matter. This has resulted in many advances in the fields of inorganic chemistry, mineralogy, organic chemistry and even to the understanding of the very processes of life.

Translational symmetry imposes many severe symmetry restrictions. For example, a five-fold axis of symmetry is forbidden in crystallography. This may be illustrated simply in two dimensions. A floor can be paved with identically shaped tiles which are parrallelograms, rhombuses, rectangles, squares, triangles or hexagons but not with equiangular pentagonal tiles. Because of this and other similar restrictions some of the most elegant solid shapes are excluded from the ambit of crystallography.

The ancient Greeks, who were great aesthetes and also geometers, discovered five perfect solids, the so-called platonic solids. They are the tetrahedron, the cube, the octahedron, the dodecahedron and the icosahedron. The last has twenty equilateral triangular faces with six five-fold axes. There may be a site symmetry in a crystal having icosahedral symmetry but it cannot survive the imposition of lattice translation.

This was the state of affairs till Roger Penrose of Oxford, one of our great living geometers, appeared on the scene. Mathematical recreations have been his favourite hobby. He was the originator, with his father, of the idea of the demon stairs, which go round without going higher, made famous by the lithograph "Ascending and Descending" by the renowned Dutch artist Escher.

One of the basic questions Penrose asked himself was 'Can a floor be paved with a set of tiles having two or more *different* shapes that tile only non-periodically'. He discovered a set of two tiles that force non-periodicity. To understand this, we go back again to the Greeks who discovered the golden mean or the golden section. This ratio is said to be the one most pleasing to the eye, and is the basis of the stark beauty of the Parthenon and the other exquisite buildings on the Acropolis in Athens. This ratio of $1 : (1+\sqrt{5})/2$ derives from the ability of a rectangle to be subdivided successively into squares and rectangles. The two tiles of Penrose are derived from a rhombus of angles 72° and 108°, dividing the long diagonal in the golden ratio $(1 : \phi)$ and joining the obtuse corners. Two tiles result, one "kite" shaped and the other "dart" shaped. The rhombus, of course, tiles periodically and so one is not allowed to join the pieces in this manner. Forbidden ways of joining sides of equal length can be enforced in many ways, the simplest being to label the corners H and T (Heads and Tails) and to follow the rule that in fitting edges only corners having the same letter may meet. Using these simple principles one can tile any floor and the pattern necessarily will be non-periodical and will have no translational symmetry.

The properties of these Penrose tilings are indeed very beautiful. It can be proved that the number of Penrose tilings is "uncountable". There are local pentagonal symmetries — a symmetry forbidden by conventional crystallography. If one is living in a place tiled by one of the uncountable infinity of Penrose tilings, one cannot know which tiling one is on! "Suppose we have explored a circular region of diameter *d* and we call it a town where we live. If we ask ourselves how far we are from a region that exactly matches the streets of our hometown", the answer is a remarkable theorem of Conway which states: "never greater than 2*d*. If you walk in the right direction you need not walk more than 2*d* to find an exact copy of your hometown, with the same street pattern"!

I believe that Penrose was reluctant to disclose his extraordinary findings because he wanted to apply for patents but he did permit a popular exposition of his scheme to be written. When Martin Gardner's article in *Scientific American* appeared, I asked myself as to what the diffraction pattern of one of these non-periodic Penrose tilings would be. It is not difficult to visualise a possible atomic structure corresponding to a Penrose tiling and to calculate the diffraction pattern. One gets the most surprising result that the two-dimensional non-periodic Penrose structure shows sharp diffraction spots. These are arranged broadly on circular regions giving an appearance of a "diffuse" x-ray powder pattern composed of a large number of discrete sharp spots.

The next logical question is to ask whether there can be a Penrose non-periodic tiling in three dimensions. The person who answered this was A. L. Mackay of Birkbeck College, London. He generalised the two-dimensional Penrose tiling to three dimensions using two rhombohedra, one acute and the other obtuse using simple recursion relationships. These three-dimensional tilings project in two dimensions to the Penrose tiling described earlier, which in turn projects into one dimension as non-periodic lattice made up of two characteristic lengths which are related by the golden ratio.

All the above is in the realm of elegant theory and imaginative speculation. What is the experimental situation?

Crystals of gold having icosahedral symmetry which give sharp electron diffraction spots have been observed under the electron microscope. These have been explained as due to twinning, impurity twinning or distortion. One has to examine carefully whether these explanations were not prompted by a desire to preserve the sacred dogma of crystallography.

In the November 12, 1984 issue of *Physical Review Letters* there appeared a paper by four authors, where a metallic solid (aluminium alloyed with 14% atomic per cent manganese prepared by rapid solidification) showed very sharp electron diffraction peaks. These diffraction peaks could not be indexed to any of the conventional Bravais lattices. What took the world of crystallography by surprise was that the sharp diffraction patterns displayed all the symmetries of the icosahedron. The diffraction geometry looks similar to the optical transform of the Penrose tiling.

The discovery is a vindication of the concept held by some of us that "the crystal does not form by the insertion of components in a three-dimensional framework of symmetry elements. It arises from a local interaction between individual atoms, and symmetry elements are a consequence." It is obvious that one has to extend our concept of crystallinity to mean the degree to which identical components of a structure are in a similar environment. It has now been shown that by pursuing recursive relationships, it is possible to produce an infinite variety of structures which are regular but "non-crystalline". There can be no doubt that a new era in solid state architecture has been opened up.

This new type of architecture presents many challenges. The structural scientist has not only to determine the structure of the molecular or ionic conglomeration, but also the Penrose pattern – a problem more complex than that of conventional crystallography. Does a glass (or even a liquid) consist of micro-regions consisting of Penrose tilings distributed at random? Are there other modes of non-periodic tiling which are different from the ones discovered so far?

To the solid state physicist this non-periodic architecture presents a new class of solids with very peculiar band gaps (probably having special properties). To the organic chemist the Penrose tilings may suggest new possibilities of molecular structure in two and three dimensions.

When one looks at this new architecture one cannot but be reminded of something G. K. Chesterton said: "The world looks a little more regular than it is. Its exactitude is obvious, but its inexactitude is hidden. Its wildness lies in wait. There is a sort of treason in the universe".

The Pursuit of Science: Its Motivations

Golden Jubilee Inaugural Lecture delivered by Prof. S. Chandrasekhar, at the Golden Jubilee Meeting on 6 February 1985.

I am grateful to President Ramaseshan for assigning to me the privilege of addressing you on this occasion celebrating the Golden Jubilee of this Academy. But for the tragedy which engulfed the country on October 31st of last year, this celebration should have taken place on November 7th, the 96th birthday of the Academy's illustrious founder and its President for its first 35 years. That this occasion is a moving one for my wife – a former student of Professor Raman – and myself must be obvious to you; and I shall not expand on it. That the occasion is also precious to me, on scientific grounds, derives not only from my having been a fellow of this Academy for all of its fifty years, but equally from the purposes to which this Academy is dedicated. Unlike other national academies, it has not sought, nor has it strived, to influence the public policies of the national government; nor has it followed the practice of awarding innumerable prizes, lectureships and medals. The purpose of this Academy, as enunciated by its founder, is to promote the pursuit of science in this country by providing journals and periodicals for the publication of scientific papers of the highest standards, and avoiding the necessity to seek foreign avenues.

During Professor Raman's lifetime, the Proceedings of the Academy, both series A and B, were published without lapse and without delay. Since 1970, when the affairs of the Academy passed on to his successors, this prime objective has so remained. By the efforts, principally of President Ramaseshan, the number of journals which are presently sponsored by the Academy has increased almost ten-fold. And all these journals do indeed maintain the highest standards by strict refereeing. I cannot wish for the future of the Academy any better than to express confidently the hope that the steadily increasing standards of the past ten years will be sustained during the next fifty years.

I

I now turn with some apprehension to the subject of my address, 'The Pursuit of Science: Its Motivations'. This is a difficult subject if one is to avoid the common and the banal. The difficulty derives in large measure from the variety and the range of the motives of the individual scientists; they are varied and they are diverse: they are as varied as the tastes, the temperaments, and the attitudes of the scientists themselves. Besides, the motivations are subject to substantial changes during the lifetimes of the scientists. Indeed, it is difficult to discern a common denominator. What is it then, one can usefully say?

I shall skirt the problem and restrict myself to reflections – perhaps, disjointed – on the lives and the accomplishments of some of the great scientists of the past. And I shall, whenever possible, base my remarks on what they have themselves said or written. But reflecting on the motives and the attitudes of great men is beset with grave semantic difficulties of communication: the words and phrases that language allows in these contexts may suggest criticism or judgement. Therefore, let me make it clear from the outset, that my remarks, at no place, are to be construed as having overtones or undertones of criticism or judgement. Indeed, I have no rights to such criticism or judgement in the contexts I shall be speaking. I should also make it clear that during the course of my reflections that I shall present, thoughts derived from my own personal experience have been entirely absent. Since it may not be possible for me to emphasize these points at every stage, I shall begin with a quotation and a narration.

The quotation is from the concluding pages of Turgenev's *On the Eve* in which there is a statement by that silent but indomitable character Insarov:

We are speaking of other people; why bring in yourself?

My narration is of a conversation between Majorana and Fermi in the middle twenties when both of them were also in their middle twenties. (The conversation was reported to me by one who was present on the occasion.)

MAJORANA: There are scientists who 'happen' only once in every 500 years, like Archimedes or Newton. And there are scientists who happen only once or twice in a century, like Einstein or Bohr.

FERMI: But where do I come in, Majorana?

MAJORANA: Be reasonable, Enrico! I am not talking about you or me. I am talking about Einstein and Bohr.

Since I shall be talking principally about scientists in the class of Einstein, Bohr, and Fermi, I should indeed be 'reasonable'.

One final reservation. The circumference of my comprehension does not extend beyond a very limited circle of the physical sciences. This is, of course, a most serious limitation when I am addressing myself to so general a theme; but I must abide by my limitation.

II

For a discussion of the motivations which impel one to pursue the goals of science, no example is better than that of Johannes Kepler. Kepler's uniqueness derives from the position he singly occupies at the great crossroads where science shed its enveloping dogmas and the pathway was prepared for Newton. Kepler, in his inquiries, asked questions that none before him, including Copernicus, had asked. Kepler's laws differ qualitatively from earlier assumptions about planetary orbits: the assertion that planetary orbits 'are ellipses' in no way resembles the kind of improvements that his predecessors had sought. In his analysis of the motions of the planets, Kepler was not preoccupied with geometrical questions; he asked, instead, questions such as 'what is the origin of planetary motions'? 'If the sun is at the centre of the solar system, as it is in the Copernican scheme, should not that fact be discernible in the motions and in the orbits of the planets themselves?'. These are questions in physics; not in some preconceived geometrical framework.

While Kepler's approach to the problem of planetary motions was radically different from that of anyone before him, his work is preeminent for the manner in which he extracted general laws from a careful examination of the observations. His examination was long and it was arduous; it took him twenty and more years of constant and persistent effort; but he never lost sight of his goal. For him, it was a search for the holy grail in a very literal sense.

Before I describe the manner of Kepler's search, I should like to say that I am in no sense a scholar of medieval astronomy. My knowledge of Kepler is in fact mostly derived from Arthur Koestler's *The Sleep-walkers; a History of Man's Changing Vision of the Universe*, some collateral reading, and some discussions with scholars who know very much more. Koestler's sensitive account of Kepler, his life and his achievements, includes numerous quotations from Kepler's own writings. My remarks are largely based on these quotations.

From the outset Kepler realized that a careful study of the orbit of Mars will provide the key to planetary motions because its orbit departs from a circle the most; and it had defeated Copernicus; and further that an analysis of the accurate observations of Tycho Brahe was an essential prerequisite. As Kepler wrote:

> Let all keep silence and hark to Tycho who has devoted thirty-five years to his observations ... For Tycho alone do I wait; he shall explain to me the order and arrangement of the orbits...
>
> Tycho possesses the best observations, and thus so-to-speak the material for the building of the new edifice...
>
> ... I believe it was an act of Divine Providence that I arrived just at the time when Longomontanus was occupied with Mars. For Mars alone enables us to penetrate the secrets of astronomy which otherwise would remain forever hidden from us...

Indeed, Kepler went to extraordinary lengths to acquire the observations of Tycho which he so badly needed. It is not an exaggeration to say that he commited larceny; for, as he confessed:

> I confess that when Tycho died, I quickly took advantage of the absence, or lack of circumspection, of the heirs, by taking the observations under my care, or perhaps usurping them...

and as he explained:

> The cause of this quarrel lies in the suspicious nature and bad manners of the Brahe family, but on the other hand also in my own passionate and mocking character. It must be admitted that Tengnagel had important reasons for suspecting me. I was in possession of the observations and refused to hand them over to the heirs...

With Tycho's observations thus acquired, the question which Kepler constantly asked himself was: If the sun is indeed the origin and the source of planetary motions, then, how does this fact manifest itself in the motions of the planets themselves? Noticing that Mars moved a little faster when it is nearest the sun than when it is farthest, and 'remembering Archimedes', he determined the area described by the radius vector joining the sun to the instantaneous position of Mars, as we follow it in its orbit. As Kepler wrote:

> Since I was aware that there exists an infinite number of points on the orbit and accordingly an infinite number of distances [from the sun] the idea occurred to me that the sum of these distances is contained in the *area* of the orbit. For I remembered that in the same manner Archimedes too divided the area of a circle into an infinite number of triangles.

This was the way Kepler discovered in July 1603 his law of areas. This is the second of his three great laws in Newton's enumeration that has been adopted ever since. The establishment of this result took Kepler some five years; for, already prior to the publication of his *Mysterium Cosmographicum* in 1596, Kepler had sought for such a law in connection with his association of the five regular solids with the existence of the six planets known in his time.

The law of areas determined the variation of the speed along its orbit; but it did not determine the shape of the orbit. A year before he had arrived at his final statement of the law of areas he had in fact discarded circular orbits for the planets: and in October of 1602 he had written:

> The conclusion is quite simply that the planet's path is not a circle — it curves inward on both sides and outward again at opposite ends. Such a curve is called an oval. The orbit is not a circle, but an oval figure.

Even after having concluded that the orbit of Mars is an 'oval', it took him an additional three years to establish that the orbit was in fact an ellipse. And when that was established he wrote:

> Why should I mince my words? The truth of Nature, which I had rejected and chased away, returned by stealth through the back door, disguising itself to be accepted. That is to say, I laid [the original equation] aside, and fell back on ellipses, believing that this was a quite different hypothesis, whereas the two, as I shall prove in the next Chapter, are one and the same... I thought and searched, until I went nearly mad, for a reason, why the planet preferred an elliptical orbit [to mine]... Ah, what a foolish bird I have been!

At long last in 1608 his monumental *Astronomia Nova* was published. As Koestler has written:

> It was a beautifully printed volume in folio, of which only a few copies survive. The Emperor [Rudolph] claimed the whole edition as his property and forbade Kepler

to sell or give away any copy of it 'without our foreknowledge and consent'. But since his salary was in arrears, Kepler felt at liberty to do as he liked, and sold the whole edition to the printers. Thus the story of the *New Astronomy* begins and ends with acts of larceny, committed *ad majorem Dei gloriam.*

Ten more years were to elapse before Kepler discovered his third law: that the squares of the periods of revolution of any two planets is in the ratio of the cubes of their mean distances from the sun. The law is stated in his *Harmonice Mundi* completed in 1618. Here is how Kepler describes his discovery:

> On 8 March of this present year 1618, if precise dates are wanted, [the solution] turned up in my head. But I had an unlucky hand and when I tested it by computations I rejected it as false. In the end it came back again to me on 15 May, and in a new attack conquered the darkness of my mind; it agreed so perfectly with the data which my seventeen years of labour on Tycho's observations had yielded, that I thought at first I was dreaming.

Thus ended Kepler's long and arduous search for his holy grail.

In his first book, *Mysterium Cosmographicum*, Kepler exclaimed:

> Oh! that we could live to see the day when both sets of figures agree with each other.

Twenty-two years later, he added the following footnote to this exclamation in a reprint edition of *Mysterium Cosmographicum* after he had discovered his third law and his poignant cry had been answered:

> We have lived to see this day after 22 years and rejoice in it, at least I did; I trust that Maestlin and many other men will share in my joy!

III

In his novel, *The Redemption of Tycho Brahe*, Max Brod, the Czech writer who is known for his publishing, posthumously, the great works of Franz Kafka, portrays and contrasts the characters of Tycho Brahe and Kepler. While Brod's novel is historically, grossly inaccurate, yet the following imagined perception of Kepler by Tycho is an artist's idealization of what a scientist like Kepler might have been:

> Kepler now inspired him [Tycho] with a feeling of awe. The tranquility with which he applied himself to his labours and entirely ignored the warblings of flatterers was to Tycho almost superhuman. There was something incomprehensible in its absence of emotion, like a breath from a distant region of ice...

Is the tranquility and the absence of emotion which Brod attributes to his imagined Kepler, ever attained by a practising scientist?

May I digress a little at this point to say that Max Brod, when he wrote his novel, *The Redemption of Tycho*, was one of a small group in Prague that included Einstein and Franz Kafka. It has been said that Brod's portrayal of Kepler was influenced by his association with Einstein. Thus Walter Nernst is reported to have said to Einstein, "You are this man Kepler."

IV

As I have stated, the most remarkable aspect of Kepler's pursuit of science is the constancy with which he applied himself to his chosen quest. To use a phrase of Shelley's, his 'was a character superior in singleness'. But does the example of Kepler provide any assurance of success for a similar constancy in others? I shall consider two examples.

First, the example of Michelson. His main preoccupation throughout his life was to measure the velocity of light with increasing precision. His interest came about almost by accident, when the Commander of the Naval Academy asked him (then an instructor at the Academy) to prepare some lecture demonstrations to illustrate Foucault's refinement of Cornu's determination of the velocity of light. That was in 1878; and it led to Michelson's first determination of the velocity of light in 1880. On the 7th of May 1931, two days before he died and fifty years later, he dictated the opening sentences of a paper, that was posthumously published and which gave the results of his last measurement. Michelson's efforts resulted in an improvement in our knowledge of the velocity of light from 1 part in 3000 to 1 part in 30,000, *i.e.* by a factor 10. But by 1973 the accuracy had been improved to 1 part in 10^{10}, a measurement that made obsolete, beforehand, all future measurements. Were Michelson's efforts over 50 years then in vain? Leaving that question aside, one must record that during his long career, Michelson made great discoveries derived from his delight in 'light waves and their uses'. Thus his development of interferometry leading to the first direct determination of the diameter of a star is wonderous. And who does not know the Michelson-Morley experiment which, through Einstein's formulation of the special and the general theory of relativity, changed and changed irrevocably our understanding of the nature of space and time? But it is a curious fact that Michelson himself was never happy with the outcome of his experiment. Indeed, it is recorded that when Einstein visited Michelson in April 1931, Mrs. Michelson felt it necessary to warn Einstein "Please don't get him started on the ether."

A second example is Eddington who devoted the last 16 years of his life to developing his 'fundamental theory'. Of this prodigious effort he said, a year before he died:

> At no time during the past 16 years have I felt any doubt about the correctness of my theory.

Yet, his efforts have left no trace on subsequent developments.

Is it wise then to pursue science with a single objective and with a singleness of purpose?

V

While Kepler provides the supreme example of sustained scientific effort leading to great and fundamental discoveries, there are instances in which great thoughts have seemingly occurred spontaneously. Thus, Dirac has written that his work on Poisson brackets and on his relativistic wave equation of the electron were consequences of ideas

> ...which had just come out of the blue. I could not very well say just how it had occurred to me. And I felt that work of this kind was a rather 'undeserved success'.

Dirac's recollection, that his ideas underlying his work on Poisson brackets and his relativistic wave equation of the electron came to him 'out of the blue', is an example of what is apparently not a unique phenomenon: Those who have made great discoveries seem to remember and cherish the occasions on which they made them. Thus, Einstein has recorded that

> When in 1907 I was working on a comprehensive paper on the special theory of relativity...there occurred to me the happiest thought of my life...that *'for an observer falling freely from the roof of a house there exists* — at least in his immediate surroundings — *no gravitational field'*.

This 'happy thought' was, of course, later enshrined in his principle of equivalence that is at the base of his general theory of relativity.

A recollection in a similar vein is that of Fermi. I had once the occasion to ask Fermi, in the context of Hadamard's perceptive *'Essay on the Psychology of Invention in the Mathematical Field'* what the psychology of invention in the realm of physics might be. Fermi responded by narrating the occasion of his discovery of the effect of slow neutrons on induced radioactivity. This is what he said:

> I will tell you how I came to make the discovery which I suppose is the most important one I have made. We were working very hard on the neutron-induced radioactivity and the results we were obtaining made no sense. One day, as I came to the laboratory, it occurred to me that I should examine the effect of placing a piece of lead before the incident neutrons. Instead of my usual custom, I took great pains to have the piece of lead precisely machined. I was clearly dissatisfied with something; I tried every excuse to postpone putting the piece of lead in its place. When finally, with some reluctance, I was going to put it in place, I said to myself: 'No, I do not want this piece of lead here; what I want is a piece of paraffin.' It was just like that with no advance warning, no conscious prior reasoning. I immediately took some odd piece of paraffin and placed it where the piece of lead was to have been.

Perhaps the most moving statement in this general context is that of Heisenberg relating the moment when the laws of quantum mechanics came to a sharp focus in his mind. He has written,

> One evening I reached the point where I was ready to determine the individual terms in the energy table, or, as we put it today, in the energy matrix, by what would now be considered an extremely clumsy series of calculations. When the first terms seemed to accord with the energy principle, I became rather excited, and I began to make countless arithmetical errors. As a result, it was almost three o'clock in the morning before the final result of my computations lay before me. The energy principle had held for all terms, and I could no longer doubt the mathematical consistency and coherence of the kind of quantum mechanics to which my calculations pointed. At first, I was deeply alarmed. I had the feeling that, through the surface of atomic phenomena, I was looking at a strangely beautiful interior, and felt almost giddy at the thought that I now had to probe this wealth of mathematical structures, nature

had so generously spread out before me. I was far too excited to sleep, and so, as a new day dawned, I made for the southern tip of the island, where I had been longing to climb a rock jutting out into the sea. I now did so without too much trouble, and waited for the sun to rise.

There is no difficulty for any of us to share in Heisenberg's exhilaration of that supreme moment: we all know of the difficulties and paradoxes that beset the 'old' Bohr-Sommerfeld quantum-theory of the time and we also know of Heisenberg's long puzzlement over these difficulties and paradoxes with Sommerfeld, Bohr, and Pauli. And he had already published at that time his paper with Kramers on the dispersion theory – a theory which in many ways was the precursor to the developments that were to follow.

But what is our reaction to Heisenberg's account of his ideas on the theory of elementary particles that he developed some thirty years later, after his tragic experiences during the war and his disappointments and frustrations of the post-war years? Mrs. Heisenberg, in her book on her husband, has written,

> One moonlight night we walked all over the Hainberg Mountain, and he was completely enthralled by the visions he had, trying to explain his newest discovery to me. He talked about the miracle of symmetry as the original archetype of creation, about harmony, about the beauty of simplicity, and its inner truth.

And she quotes from one of Heisenberg's letters to her sister at this time:

> In fact, the last few weeks were full of excitement for me. And perhaps I can best illustrate what I have experienced through the analogy that I have attempted an as yet unknown ascent to the fundamental peak of atomic theory, with great efforts during the past five years. And now, with the peak directly ahead of me, the whole terrain of interrelationships in atomic theory is suddenly and clearly spread out before my eyes. That these interrelationships display, in all their mathematical abstraction, an incredible degree of simplicity, is a gift we can only accept humbly. Not even Plato could have believed them to be so beautiful. For these interrelationships cannot be invented; they have been there since the creation of the world.

You will notice the remarkable similarity in the language and in the phraseology of this description with the description of his discovery of the basic rules of quantum mechanics some thirty years earlier. But do we share in his second vision in the same way? In the earlier case, his ideas won immediate acceptance. In contrast, his ideas on particle physics were rejected and repudiated even by his long time critic and friend Pauli. But it is moving to read what Mrs. Heisenbeg writes towards the end of her biography.

> With smiling certainty, he once said to me: 'I was lucky enough to look over the good Lord's shoulder while He was at work.' That was enough for him, more than enough! It gave him great joy, and the strength to meet the hostilities and misunderstandings he was subjected to in the world time and again with equanimity, and not be led astray.

VI

A different aspect of the effect a great discovery can have on its author is provided by the autobiography entitled *The Traveler* by Hideki Yukawa. The book was written when Yukawa was past fifty. One would normally have expected that an autobiography entitled *The Traveler* by one whose life, at least as seen from the outside, had been rich and fruitful, would be an account of his entire life. But Yukawa's account of his 'travels' ends with the publication of his 1934 paper describing his great discovery with the sombre note:

> I do not want to write beyond this point, because those days when I studied relentlessly are nostalgic to me; and on the other hand, I am sad when I think how I have become increasingly preoccupied with matters other than study.

VII

While all of us can share in the joy of the discoveries of the great men of science, we may be puzzled by what those many, very many, less perceptive and less fortunate, are to cherish and remember. Are they, like Vladimir and Estragon, destined to wait for Godot as in Samuel Beckett's play; or, are they to console themselves with Milton's thought 'they also serve who stand and wait'?

VIII

I now turn to the role which approbation and approval play in one's pursuit of science. The example of Newton 'voyaging through strange seas of thought alone' is not one that any of us can follow.

I have referred to Eddington's lonely efforts in pursuing his fundamental theory. In spite of his expressed confidence in the correctness of his theory, Eddington must have been deeply frustrated by the neglect of his work by his contemporaries. This frustration is evident in his plaintive letter to Dingle written a few months before he died:

> I am continually trying to find out why people find the procedure obscure. But I would point out that even Einstein was considered obscure, and hundreds of people have thought it necessary to explain him. I cannot seriously believe that I ever attain the obscurity that Dirac does. But in the case of Einstein and Dirac people have thought it worthwhile to penetrate the obscurity. I believe they will understand me all right when they realize they have got to do so—and when it becomes the fashion 'to explain Eddington'.

The lack of approval by one's contemporaries can have tragic consequences when they are expressed in the form of sharp and violent criticisms. Thus, Ludwig Boltzmann, greatly depressed by the violence of the attacks directed against his ideas by Ostwald and Mach, committed suicide 'as a martyr to his ideas', as his grandson Flamm has written. And George Cantor, the originator of the modern theory of sets of points and of the orders of infinity, lost his mind because of the hatred and the animosity against him and his ideas by his teacher Leopold Kronecker, and was confined to a mental hospital during the last many years of his life.

IX

A case very different from the ones I have considered so far is that of Rutherford.

Consider his record. In 1897 he analyzed radioactive radiations into three types: α-particles, β-rays and γ-rays, a nomenclature that has survived to this day. In 1902 he formulated the laws of radioactive disintegration: the first time a physical law was formulated in terms of probability and not certainty: a forerunner of the probability interpretation of quantum mechanics that was to become universal some 25 years later. In 1905-1907 he formulated, together with Soddy, the laws of radioactive displacement and identified the α-particle as the nucleus of the helium atom; and, together with Boltwood, initiated the determination of the ages of rocks and minerals by their radioactivity. In 1909-1910, there were the experiments of Geiger and Marsden, the discovery of the large angle scattering of α- rays and Rutherford's formulation of his law of scattering and the nuclear model of the atom. Then in 1917 he effected the first laboratory transformation of atoms: that of nitrogen-14 into oxygen-17 and a proton by α-ray bombardment. In the twenties, he was associated with the clarification of the relationship between the α-ray and the γ-ray spectra. And 1932—the *annus mirabilis* as R. H. Fowler called it—saw the discovery of the artificial disintegration of Li into two α- particles by Cockroft and Walton, of positrons in cosmic-ray showers by Blackett, and the neutron by Chadwick—all of them in Rutherford's Cavendish. In the following year Rutherford, together with Oliphant, himself discovered hydrogen-3 and helium-3. Altogether, then, an accomplishment unparalleled in this century.

Rutherford's attitude to his own discoveries is illustrated by his response to a remark of one who was present at the moment of one of his great discoveries: 'Rutherford, you are always on the crest of the wave.' To which Rutherford responded: 'I made the wave, didn't I?' Somehow from Rutherford's vantage point everything he said seems right, even including his remark, 'I do not let my boys waste their time' when he was asked if he encouraged his students to study relativity!

Rutherford was a happy warrior if ever there was one.

X

So far, I have tried to illustrate some facets of the pursuit of science by drawing on incidents in the lives of some great men of science. I shall turn now to some more general matters.

I shall start with an example. It has been reported that when Michelson was asked towards the end of his life, why he had devoted such a large fraction of his time to the measurement of the velocity of light, he replied 'it was so much fun'. There is no denying that 'fun' does play a role in the pursuit of science. But the word 'fun' suggests a lack of seriousness. Indeed, the Oxford Dictionary gives to 'fun' the meaning 'drollery'. We can be certain that Michelson did not have that meaning in his mind when he described his life's main interest as 'fun'. If not, what precisely is the meaning we are to attach to 'fun' in the context in which Michelson used it? More generally, what is the role of pleasure and enjoyment?

While 'pleasure' and 'enjoyment' are often used to characterize one's efforts in science, failures, frustrations, and disappointments are equally, if not more, the common ingredients of scientific experience. Overcoming difficulties, undoubtedly, contributes to one's final enjoyment of success. Is failure, then, a purely negative aspect of the pursuit of science?

A remark of Dirac's describing the rapid development of physics following the founding of the principles of quantum mechanics in the middle and the late twenties is apposite in this connection.

> It was a good description to say that it was a game, a very interesting game one could play. Whenever one solved one of the little problems, one could write a paper about it. It was very easy in those days for any second-rate physicist to do first-rate work. There has not been such a glorious time since then.

Consider in the context of these remarks, J. J. Thomson's assessment of Lord Rayleigh in his memorial address given in Westminister Abbey:

> There are some great men of science whose charm consists in having said the first word on a subject, in having introduced some new idea which has proved fruitful; there are others whose charm consists perhaps in having said the last word on the subject, and who have reduced the subject to logical consistency and clearness. I think by temperament Lord Rayleigh belonged to the second group.

This assessment of Rayleigh by J. J. Thomson has sometimes been described as double-edged. But could one not conclude, instead, that Rayleigh by temperament chose to address himself to difficult problems and was not content to play the kind of games that Dirac describes in his characterization of the 'glorious time' in physics as a time 'when second-rate physicists could do first-rate work'?

This last question concerning Rayleigh's temperament raises the further question: after a scientist has reached maturity, what are his criteria for his continued pursuit of science? To what extent are they personal? And to what extent are aesthetic criteria like the perception of order and pattern, form and substance, relevant?

Are such personal criteria exclusive? Has a sense of obligation a role? I do not mean obligation with the common meaning of obligation to one's students, one's colleagues, and one's community. I mean, rather, obligation to science itself. And what, indeed, is the content of obligation in the pursuit of science *for* science?

Let me finally turn to a different aspect. G. H. Hardy concludes his *A Mathematician's Apology* with the following illuminating statement:

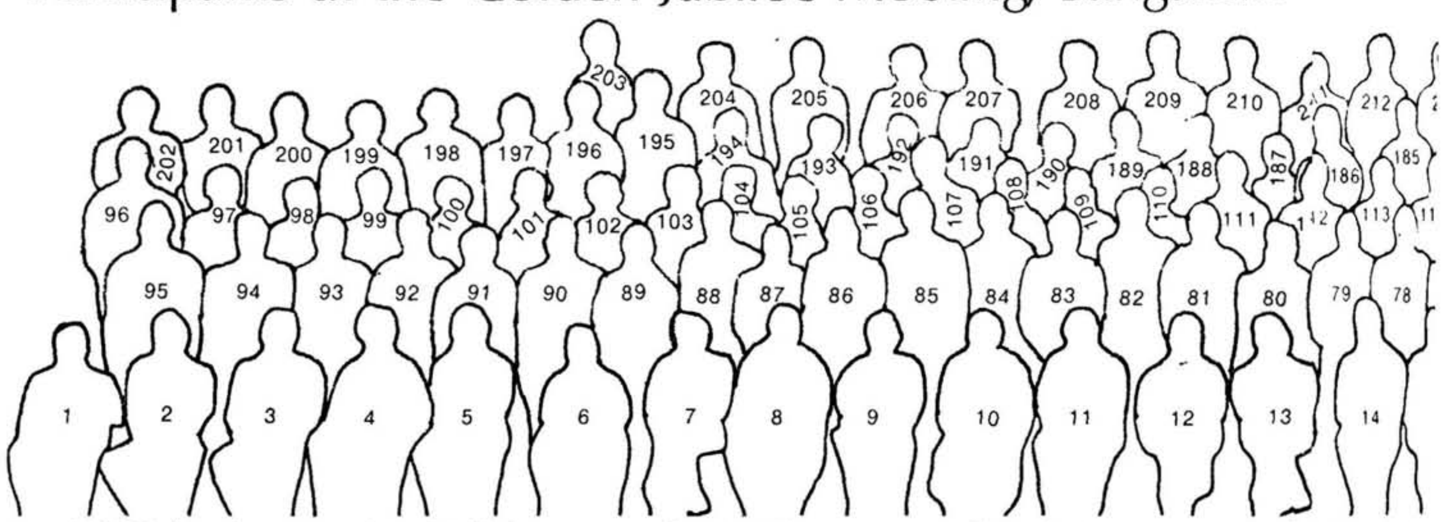
> The case for my life, then, or for that of anyone else who has been a mathematician in the same sense in which I have been one, is this: that I have added something to knowledge, and helped others to add more; and that these somethings have a value which differs in degree only, and not in kind, from that of the creations of the great mathematicians, or of any of the other artists, great or small, who have left some kind of memorial behind them.

Hardy's statement is made relative to mathematicians; but it is equally applicable to all scientists. I want to draw your attention particularly to Hardy's reference to one's wanting to leave behind some kind of a memorial *i.e.*, something that posterity may judge. To what extent, then, is the judgement of posterity (which one can never know) a conscious motivation in the pursuit of science?

XI

The pursuit of science has often been compared to the scaling of mountains, high and not so high. But who amongst us can hope, even in imagination, to scale the Everest and reach its summit when the sky is blue and the air is still: and in the stillness of the air survey the entire Himalayan range in the dazzling white of the snow stretching to infinity. None of us can hope for a comparable vision of nature and of the universe around us. But there is nothing mean or lowly in standing in the valley below and awaiting the sun to rise over the Kunchenjunga.

Participants at the Golden Jubilee Meeting, Bangalore



1. C. S. Vaidyanathan
2. P. R. Adiga
3. P. S. Narayanan
4. D. Balasubramanian
5. P. M. Bhargava
6. Sulochana Gadgil
7. S. R. Valluri
8. M. R. A. Rao
9. S. Paramasivan
10. S. C. Sheth
11. S. Kedharnath
12. K. S. G. Doss
13. R. Subrahmanyan
14. R. Narasimha
15. S. Chandrasekhar
16. M. D. Shami
17. B. Nag
18. V. V. Narlikar
19. M. R. Siddiqi
20. O. Siddiqi
21. C. V. Subramanian
22. B. N. B. Rao
23. K. Venkateswarlu
24. C. N. R. Rao
25. S. Chandrasekhar
26. S. Ramaseshan
27. Lalitha Chandrasekhar
28. V. Puri
29. M. S. Narasimhan
30. P. S. Rao
31. S. Dhawan
32. M. G. K. Menon
33. T. S. Sadasivan
34. K. Ramiah
35. K. Ganapathi
36. M. Venkataraman
37. P. C. Vaidya
38. D. V. Bal
39. S. Jones
40. P. R. Krishna Rao
41. C. Ramaswamy
42. R. S. Krishnan
43. H. Y. Mohan Ram
44. A. Mani
45. M. S. Vardya
46. P. K. Malhotra
47. L. R. Row
48. H. N. Siddiquie
49. R. Natarajan
50. B. B. Biswas
51. B. L. K. Somayajulu
52. F. Ahmad
53. L. K. Ramachandran
54. N. H. Wadia
55. A. Ghosh
56. C. S. Seshadri
57. S. Ramanan
58. R. Vijayaraghavan
59. M. M. Taqui Khan
60. N. Rudraiah
61. K. Ramachandra
62. J. C. Bhattacharyya
63. S. Biswas
64. Y. S. Murty
65. T. K. Roy
66. J. S. Yadav
67. N. B. Nair
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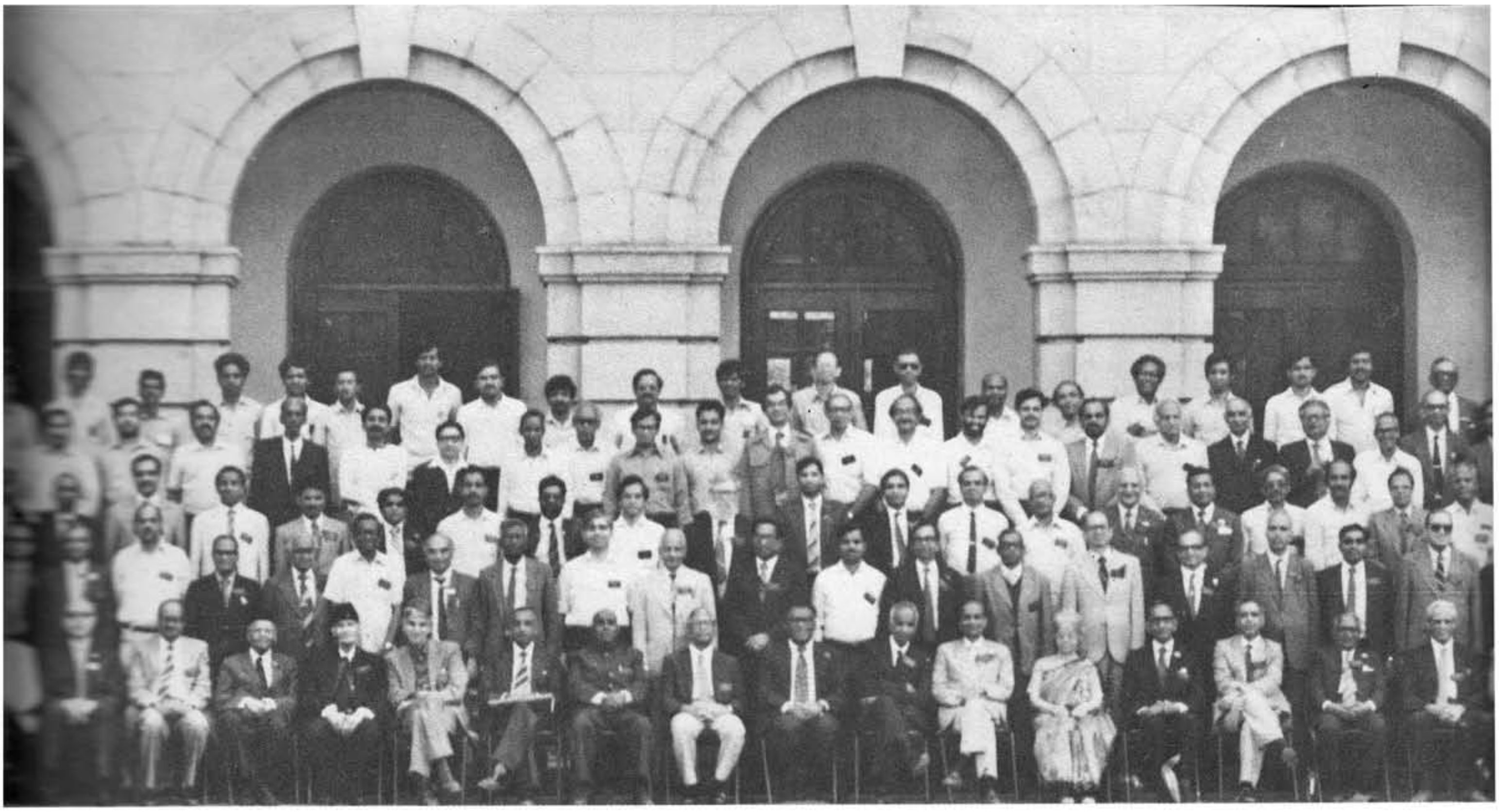
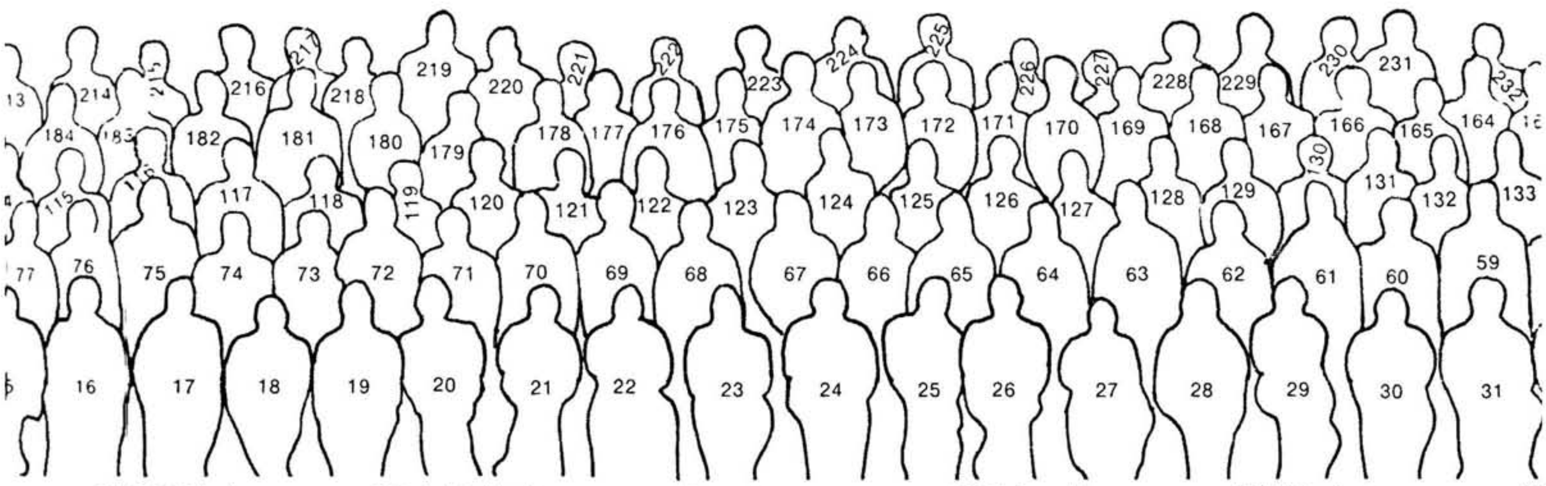
69. P. K. Kaw
70. M. N. Rao
71. Rama
72. P. Kumaraswamy
73. P. V. Kulkarni
74. K. D. Abhyankar
75. V. R. Gowariker
76. R. K. Varma
77. R. V. Bhonsale
78. C. M. Srivastava
79. H. O. Agrawal
80. V. Baliah
81. T. R. Govindach
82. K. S. Yajnik
83. J. V. Narlikar
84. V. K. Gaur
85. V. S. Arunachala
86. P. R. Roy
87. M. K. Asundi
88. S. Ranganathan
89. B. C. Subba Rao
90. M. R. Raghavend





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91. P. R. Mahadevan
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94. T. R. Kasturi
95. A. V. Rama Rao
96. G. Mehta
97. G. S. R. Subba Rao
98. S. Krishnan
99. S. K. Rangarajan
100. S. Ranganathan
101. P. Balaram
102. K. P. Sinha
103. B. R. Iyer
104. A. K. Kembhavi
105. T. Padmanabhan
106. H. S. Mukunda
107. R. A. Mashelkar
108. C. R. Bhatia
109. C. J. Saldanha
110. N. K. Notani
111. V. L. Chopra
112.
113. K. P. Abraham
114. S. Mitra
115. P. Ramachandra Rao
116. P. Krishna
117. P. M. Mathews
118. S. D. Prasad
119. M. V. Bhatt
120. A. Pati
121. R. J. Azmi
122. J. N. Goswami
123. A. S. Rand
124. M. G. Kulkarni
125. S. P. Modak
126. G. P. Kalle
127. B. K. Nayar
128. L. D. Kapoor
129. D. V. S. Jain
130. J. Barnabas
131. J. Thomas
132. T. N. Ananthakrishnan
133. S. Suryanarayanan
134. T. J. Pandian
135. S. S. Kalbag
136. Joy David
137. B. S. Ramakrishna
138. R. Ananthakrishnan
139. M. A. Viswamitra
140. R. Chidambaram
141. P. R. Pisharoty
142. K. N. Rao
143. B. N. Bhargava
144. S. R. Rajagopalan
145. V. Sasisekharan
146. T. V. Desikachari
147. V. V. R. Varadachari
148. R. M. Varma
149. H. Sharat Chandra
150. A. S. Kolaskar
151. D. D. Pant
152. H. D. Kumar
153. M. N. Bose
154.
155. S. Swaminathan
156. K. S. Viswanathan
157. B. M. Udagaonkar
158. S. P. Pandya
159. C. Ambasankaran
160. Raj Mahindra
161. R. Kumar
162. G. V. Subba Rao
163. V. Krishnan
164. E. S. Raja Gopal
165. J. V. Bhat
166. B. K. Bachhawat
167. T. Ramakrishnan
168. C. Siva Raman
169. M. R. Das
170. K. N. Ganesh
171. T. B. Bhat
172. K. Neelakantan
173. V. Balakrishnan
174. P. S. Ramakrishnan
175. B. K. Mishra
176. K. K. Kannan
177. K. Venkatesan
178. V. S. Narasimham
179. L. K. Pandit
180. V. Nanjundiah

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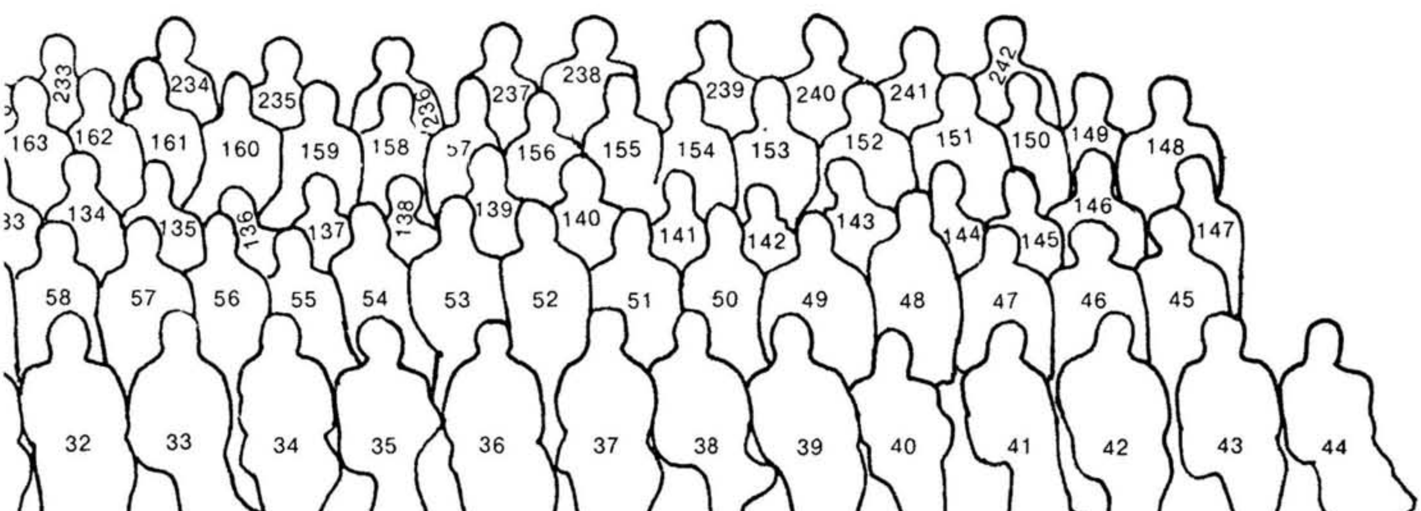

181. C. R. Narayanan
182. M. K. Chandrashekaran
183. R. Gadagkar
184. L. C. Padhy
185. B. S. N. Rao
186. B. Krishna
187. V. Padma
188. R. Ranjini
189. K. Shanthi
190. R. Shyamala
191. Meena
192. M. Srimathi
193. K. Shashikala
194. H. S. Mani
195. M. Vijayan
196. S. R. Shetye
197. P. Venkatakrishnan
198. S. L. N. G. Krishnamachari
199. G. Madhavan
200. K. J. Rao
201. V. S. R. Rao
202. C. V. Vishveshwara
203. N. R. Moudgal
204. S. R. Gadre
205. T. N. Guru Row
206. G. S. Bhat
207. S. V. Kailas
208. K. R. Shivanna
209. M. M. Johri
210. E. D. Jemmis
211. K. R. Anantharamaiah
212. B. Sethumani
213. U. L. Bhat
214. N. Sheriff
215. C. Vedamurthy
216. G. V. Narahari
217. M. S. Venugopal
218. R. Rangaswamy
219. C. S. Ravi Kumar
220. A. Nagaraj
221. L. Hanumanthappa
222. G. Chandramohan
223. H. Channaiah
224. Madhav Gadgil
225. N. Mukunda
226. G. Nath
227. P. K. Maitra
228. A. N. Bhaduri
229. P. Babu
230. M. S. Murali
231. P. Jayaraj
232. M. Shadaksharaswamy
233. S. K. Trehan
234. A. N. Radhakrishnan
235. N. A. Narasimham
236. G. Swarup
237. G. Srinivasan
238. V. Radhakrishnan
239. B. V. Sreekantan
240. R. Cowsik
241. M. S. Raghunathan
242. T. P. Prabhu

Presentation of Srinivasa Ramanujan Bust

Professor Ramaseshan making his introductory remarks

On 6 February 1985, at a function held in the Library Hall of the Raman Research Institute, a bronze bust of Srinivasa Ramanujan was formally presented to the Indian Academy of Sciences by Prof. S. Chandrasekhar and Mrs. Lalitha Chandrasekhar. While offering the bust as a gift to the Academy, Prof. Chandrasekhar had written that "the only positive request we should like to make is that the bust be placed in an appropriate place in the Raman Institute as a companion to the bust of Raman". It is only fitting that the Raman Institute will have the busts not only of the greatest physicist of India but also the greatest mathematical genius of our times, who also happened to be an Indian.

Welcoming the gathering, Prof. Ramaseshan recalled the events which had led to the present occasion. Three American Mathematicians, Prof. George Andrews, Prof. Richard Askey and Prof. Bruce Berndt had played an important role in the genesis of the bust. Prof. Richard Askey of the University of Wisconsin at Madison in particular had been so impressed with Ramanujan's mathematical discoveries that he had taken the initiative to have the bust made by the distinguished sculptor Paul Granlund, the expenses being met by contributions from over a hundred mathematicians of the world. A minimum of 4 copies was necessary. Four were made, one for Mrs. Ramanujan, one for Prof. Askey, one for the Chandrasekhars and the fourth is the bust gifted to the Academy.

Ramanujan, Prof. Ramaseshan recalled, is the greatest mathematician India has produced and in some sense the quality of his mathematical genius has not been equalled anywhere in the world during this century. Prof. G. N. Watson wrote "Seeing some of Ramanujan's equations gives me a thrill which is indistinguishable from the thrill of seeing some of the incomparable sculptures of Michael Angelo". His discoveries in this sense are similar to Einstein's General Theory of Relativity which were considered by Landau and Lifshitz as probably the most beautiful of existing theories. Indeed Einstein himself was mesmerized and wrote at the end of his first paper "Scarcely anyone who fully understands this theory can escape from its magic".

Chandrasekhar has spoken of the beauty and the quest for beauty in science. Bhabha, speaking of Raman, had recognised that all great scientists are creative artists. Raman loved and enjoyed the beauty he saw in the glittering rainbow, the twinkling star, the sparkle of a dust particle in the sun's ray, the blue of the sky, the colour of the plumage of birds. Just as a musician can recognise his Mozart, Beethoven or Schubert or Thyagaraja or Muthuswami Dikshitar, any mathematician can recognize his Cauchy, Gauss or Jacobi. He concluded by saying how greatly honoured the Academy is by the gracious gift by Prof. and Mrs Chandrasekhar of the bust of the great Ramanujan to the Academy.

Before formally presenting the bust, Mrs. Lalitha Chandrasekhar spoke as follows :-

"I am sure all of you would want to know how this bust of Ramanujan that I am going to unveil presently came into being. The story started with a trip Professor George Andrews made to Cambridge, England, about eight years ago in 1976 in search of unpublished manuscripts of Ramanujan. He found in the Ramanujan Archives at the Trinity College Library a new manuscript that had been deposited by Professor R. A. Rankin and Professor J. M. Whittaker some five to ten years earlier. He discovered in this way what is now referred to as Ramanujan's "Lost notebook". In it Andrews found some 600 formulae that Ramanujan had apparently worked out during the last year of his life.

The discovery of this remarkable notebook spread in the mathematical world and also caught the attention of the media. *The New York Times* interviewed Professor Andrews and a major story appeared. *The Hindu* followed with a more extensive interview during Andrews's visit to Madras in 1981. During that visit Andrews read an article in *The Hindu* about an interview with the widow of Ramanujan, Janaki Ammal. In it she was quoted as saying, "They said years ago that a statue would be erected in honour of my husband. Where is the statue?".

Richard Askey, Professor of Mathematics, at the University of Wisconsin, at Madison, Wisconsin, who came to hear of this statement of Mrs. Ramanujan thought that if no one was going to make that bust, he was going to see that it was made.

"Now, it so happens that only one photograph of Ramanujan was available for the project Professor Askey undertook; and that is the photograph which appears in Hardy's Harvard 1936 lectures on "Ramanujan". Incidentally, when Hardy was to give those lectures, no good photograph of Ramanujan was available. He had asked Chandra who was then a Fellow at Trinity College to find one for him when he visited India in 1936. On that visit Chandra met Mrs. Ramanujan in Madras and discovered that she had in her possession her husband's last passport and in it there was a photograph of Ramanujan. Chandra made three enlargements of this photograph of which he gave one to Mrs. Ramanujan, sent the second to Professor Hardy for inclusion in his book on "Ramanujan"—Professor Hardy's comment on receiving the photograph was: "It seems to me an extremely good one. He looks rather ill (and no doubt was very ill), but he looks all over the genius he was"—and the third has been a constant companion of Chandra's in his office ever since. It is there beside the bust I am going to unveil. "To this day Chandra says, "That is my best contribution to mathematics". It is this photograph that has been the basis of all the pictures of Ramanujan we see.

Mrs. Lalitha Chandrasekhar presenting the bust of Srinivasa Ramanujan.

"Askey asked Chandra for this original enlargement in order to make the portrait bust. It is a challenge for a sculptor to transform a two-dimensional photograph into a three-dimensional bust. Paul Granlund, sculptor-in-residence at Gustavus Adolphus College at Saint Peter, Minnesota, took up the challenge, and he also considered it an unique opportunity since he was captivated by Ramanujan's face. Besides, he must have been influenced by Professor Askey's enthusiasm. Granlund was willing to undertake the project with the assurance that a minimum of four busts will be bought. Of the four, only three were definitely ordered to begin with: one was for Mrs. Ramanujan, the second for Askey, and the third for us. The one for Mrs. Ramanujan was bought from contributions by mathematicians the world over, but mostly from America; and a generous grant from the Raman Research Institute Trust.

"The first bust was cast and Professor Askey brought a photograph of it to show it to us in Chicago. We were delighted to see it. After he left, it suddenly occurred to us that we should get another bust of Ramanujan and present it as our gift to India. But where should it be placed? It seemed to us that the most appropriate place for the bust will be in the same hall where one can pay our respects both to the greatest mathematician of India and the greatest physicist of India: Ramanujan and Raman. It was clear to us also that we should present the bust to the Indian Academy of Sciences.

"Having made this decision, Chandra called Madison to tell Askey about it. But since Askey had left on a vacation, we traced him to his vacation spot! Askey must surely have been surprised when we told him that we would like a second bust.

"In some ways we feel ennobled that we can in our humble way pay our tribute to the greatest intellect our country has produced.

"News of the bust being made for the Indian Academy of Sciences spread and other institutions in India became interested. The Tata Institute asked for one and the Defence Department in Delhi for another. So altogether Granlund made six busts.


Part of the audience at the function

"The one to Mrs. Ramanujan was unveiled in Madras by Mrs. Ramaseshan a few months ago. We had a private ceremony in our apartment for some mathematicians when we unveiled ours. I know you are anxious to see the bust of Ramanujan. There is no reason for any more delay. It is with immense pleasure that I now unveil this bust of Srinivasa Ramanujan so that all of you can look upon him, or rather, that he can gaze upon you with his thoughtful expression. In some special sense it would seem right to say that Ramanujan belongs not only to India but to the entire world since his phenomenal rise from poor circumstances to become the source of inspiration to so many has amazed mathematicians everywhere. I thank you for your patience in hearing me".

In his speech, Prof. Chandrasekhar expressed his gratitude to the more than 100 scientists and mathematicians of the world and to the Raman Research Institute Trust who had contributed money for the bust. He added that as long as people do mathematics, the work of Ramanujan will continue to be appreciated.

He then read a tribute to Ramanujan from Prof. Askey which is reproduced in full below:-

"Srinivasa Ramanujan is a name known to all mathematicians, and most know something about his life. However if I am typical, it is only possible to start to appreciate his greatness by working on problems close to those he studied. My own introduction to some of Ramanujan's work came in the academic year 1975-76. First, George Andrews and I discovered some new orthogonal polynomials and the further we went the more we had to use some identities of Ramanujan. Then in the spring of 1976 Andrews went to Europe for a meeting and stopped in Cambridge to see what old manuscripts he could find. One find was not a manuscript but 140 pages of formulas in Ramanujan's handwriting. There is a direct line from these pages to the bust which is being dedicated today, but before telling that story, the incredible story of the work on these sheets should be outlined.

"As you all know, Ramanujan returned to India in 1919 and died a year later. What is only now starting to be appreciated is the seriousness of his illness in England the previous two years. These pages are not dated, but from internal evidence they were written late in Ramanujan's life, much of it in his last year. Two thirds of the pages deal with basic hypergeometric series and most of this work is significantly deeper than Ramanujan's earlier work on the same subject. Try to imagine the quality of Ramanujan's mind, one which drove him to work unceasingly while deathly ill, and one great enough to grow deeper while his body became weaker. I stand in awe of his accomplishments; understanding is beyond me. We would admire any mathematician whose life's work was half of what Ramanujan found in the last year of his life while he was dying.

"Some of Ramanujan's work has one quality which is shared by very little other work. Most mathematics, including some very good work, is predictable. Much of the rest seems inevitable after it is understood, and it would eventually be discovered by someone else. Little of Ramanujan's work seems predictable at first glance, and after we understand it there is still a fairly large body of work about which it would be safe to predict that it would not be rediscovered by anyone who has lived in this century. Then there are some of the formulas Ramanujan found that no one understands or can prove. We will probably never understand how Ramanujan found them.

"The story of the thread from these sheets to the bust is simple. Andrews has done a lot of very deep work trying to understand what Ramanujan discovered. Eventually *The New York Times* heard about it and interviewed him. *The Hindu* followed with a more extensive interview, and also published an interview with Ramanujan's widow, Janaki Ammal. She lamented the fact that a statue of Ramanujan had never been made, although one had been promised. Andrews sent me copies of these interviews, and after a couple of months my subconscious finally got through to my conscious mind and it was clear that a bust should be made. Since Janaki Ammal was 80, time was important, so it was up to individuals rather than governments or societies, since institutions move slowly. My first reason for wanting a bust was simple; if

Ramanujan's widow wanted one she should have it. That was the least we could do to show our appreciation of Ramanujan to someone who had been a great help to him. Later I realized there was a second reason, which Janaki Ammal must have realized all along. She knew Ramanujan, and while she did not understand his mathematics, she knew that he was one of the few whose work will last. As long as people do mathematics, some of Ramanujan's work will be appreciated. Fame is a strange thing, and is often fleeting. An interview on a television program is now the accepted form of honor. In Ramanujan's case a more permanent memorial is appropriate: one which can be appreciated by those who do not understand his mathematics should be added to the memorial Ramanujan made for himself with his work.

"I am pleased to have played a role in this, and would like to thank the more than one hundred mathematicians and scientists who contributed money for the bust which was presented to Janaki Ammal. The bust being dedicated today was donated by a couple who are now friends, Subrahmanyan and Lalitha Chandrasekhar. When I asked Chandra about the appropriateness of a bust of Ramanujan, he immediately replied that it was a good idea and they would do all they could to help. They did. Finally I want to thank the sculptor, Paul Granlund. While he does not appreciate Ramanujan's mathematics as those of us who have studied it do, he studied Ramanujan's passport photo deeply, and the results show in the bust. He probably understands some things about Ramanujan that we do not."

Academy Workshop on Supernovae

The Academy Workshop on Supernovae, their Progenitors and Remnants was held from October 29 to November 2, 1984 at the Raman Research Institute, Bangalore, as part of the Golden Jubilee celebrations.

From ancient times, astronomers have recorded the appearance of "new" stars, the so-called novae, at positions in the sky where nothing was seen before. Just fifty years ago Baade and Zwicky singled out the supernovae, a special class of such events accompanied by a sudden increase in the luminosity to a billion times that of the sun and an ejection of matter at about ten thousand kilometers per second. Such events are rare – the last one observed in our galaxy was in 1604 – but can be studied in other galaxies and also by the expanding cloud of gas, energetic particles, and magnetic fields that they leave behind. The Crab Nebula is perhaps the best known supernova remnant, associated with an explosion in A.D. 1054 recorded by Chinese astronomers.

The supernova phenomenon is connected with almost every branch of astrophysics, as emphasized in the very first invited talk of the Workshop given by L. Woltjer (European Southern Observatory). All sufficiently massive stars are believed to end their lives in this manner. The chemical elements heavier than helium are believed to originate in nuclear reactions in such stars and return to the interstellar gas in the final explosion, which may itself produce some elements like iron. The energy of the explosion is a major factor in the dynamics of the interstellar gas and can compress it to trigger off a collapse leading to star formation. There are even speculative ideas relating the formation of entire galaxies to such explosions. While there is a broad consensus about the phenomenon, many aspects remain poorly understood despite intensive observational and theoretical studies – for example, the precise origin of the energetic particles and magnetic fields in supernova remnants, the connection with cosmic rays, the actual chemical abundances, etc. Observations often do not show a simple pattern of expansion, no doubt because the explosion occurs in an inhomogenous medium.

V. L. Trimble (Universities of Maryland & California) discussed the events in the evolution of the progenitor star before the explosion, including the many different causes leading to a collapse of the core in stars of different masses. The observational counterpart of this process is believed to be the type II supernova, with hydrogen lines in its spectrum. The bounce which must occur to convert an implosion into an explosion is best understood in terms of the formation of a central neutron star. However, all theoretical attempts to reproduce an explosion of the required strength, or any explosion at all, have failed, barring some very recent work with super-computers. The type I supernova, which has a spectrum without hydrogen lines, is now believed to result from the complete disruption of a star by the nuclear energy released in the more or less rapid "burning" of a carbon-oxygen core. An important aspect of this model is the energy released over a period of about hundred days by the decay of radioactive nickel to cobalt and iron, accounting for the characteristic exponential decrease of the emitted light after the explosion. It is not clear whether the nickel and cobalt produced has yet been detected but there is evidence for iron in the spectra of Type I supernovae.

With the commissioning of the Very Large Array, a highly sensitive combination of radio telescopes, the study of the radio emision from supernovae in external galaxies has become possible. K. W. Weiler (National Science Foundation, USA) reviewed this field. There are two basic mechanisms for radio emission. The first depends on the kinetic energy of the expanding outer layers of the star. In the second model the rotation and magnetic field of the central neutron star play an important role. R. A. Chevalier (University of Virginia) and F. Pacini (Arcetri Observatory, Florence) who are mainly responsible for the two models were present to describe and defend their points of view. The Crab Nebula is widely believed to be an example of the second mechanism, which naturally explains its "filled" appearance. The first mechanism is believed to operate in the shell remnants in which the radio emission is concentrated in a ring. D. H. Clark (Rutherford Appleton Laboratory, England) reviewed radio and optical studies of supernova remnants and the statistical information derived from them. He presented recent observations delineating the three-dimensional structure of the Crab Nebula. Filled centre remnants were the subject of seminar presentations by K. W. Weiler and K. S. Dwarakanath (Raman Research Institute). While the first talk reviewed the latest observational material, the second was aimed at estimating how frequently such remnants were born, concluding that those resembling the Crab were rather rare. G. Srinivasan (Raman Research Institute) made out the case for the Crab being therefore atypical, the neutron star being a slower rotator in most cases. D. Bhattacharya (Indian Institute of Science) suggested that the recently discovered supernova remnant in the Large Magellanic Cloud was best understood as a combination between the two types, even though the central pulsar is remarkably similar to the Crab. One of the talks by F. Pacini dealt with his rather successful picture of the high energy (optical and x-ray) emission from pulsars.

The rich variety of phenomena resulting from stellar evolution in binary systems was covered by E.P.J. van den Heuvel (Amsterdam) who brought out the connection with the different types of x-ray sources and rapidly spinning neutron stars (milli-second pulsars) which can result. Evolution in binaries, while complex, is nonetheless highly relevant to the real world since the vast majority of massive stars do occur in such systems. V. Radhakrishnan (Raman Research Institute) presented his recent work with C. Shukre on the connection between the observed velocities of radio pulsars and the properties of the binary systems from which they must have been released by supernova explosions.

Some of the other topics dealt with in seminars were explosions in molecular clouds and the origin of cosmic ray antiprotons (S. A. Stephens, Tata Institute of Fundamental Research), the spectrum and composition expected for acceleration of cosmic rays in supernova remnants (R. Cowsik, Tata Institute of Fundamental Research), statistics of supernovae in elliptical galaxies (R. K. Kochhar, Indian Institute of Astrophysics), thermal evolution of magnetic fields in neutron stars (A. Ray, Tata Institute of Fundamental Research), point radio sources near supernova remnants (J. H. Van Gorkom, USA), statistics of radio pulsars (R. A. Chevalier) and radio observations of the jet of the Crab Nebula (T. Velusamy, Tata Institute of Fundamental Research). The final summing up by L. Woltjer brought out the progress which has been made as well as the problems and gaps in our understanding which remain. Both the modelling and the comparison with observation need to be done with more rigour for further progress.

International Symposium on Theoretical Physics

To celebrate the Diamond Jubilee of Bose Statistics, an International Symposium on Theoretical Physics, sponsored and supported by the Department of Science and Technology, the Indian Academy of Sciences, the Indian National Science Academy and the Indian Institute of Science was held from 19 November to 1 December 1984 at the Indian Institute of Science. The focal themes of the Symposium were (a) quantum theory of observation and measurement, and (b) symmetry breaking and its application to various areas such as quantum field theory, condensed matter physics, unification of the fundamental forces, physics of early universe etc. Thirty participants, most of them young scientists, attended the Symposium.

There were 26 lectures by scientists from India and abroad. Among these were three lectures by Prof. J. P. Vigier, on the Causal non-local character of Bose-Einstein Statistics and the Interpretation of recent quantum measurements, and two lectures by Prof. H. Rauch of Vienna, on his experiments on quantum phenomena using neutron interferometry. Prof. F. de Martini described his experimental work dealing with photon interferometry and the measurement of zero-point fluctuations of the electromagnetic field. These experiments, with those of Aspect on photon correlation related to the Einstein-Podolsky-Rosen problem, brought to focus some recent developments in the quantum theory of measurements. Prof. Vigier in his talks emphasized his interpretation where quantum objects simultaneously have wave and particle aspects, a view developed in the twenties by Einstein and de Broglie and amplified later by David Bohm. This was intimately connected with the nature of the vacuum state. In a paper on the superfluid state of particles and antiparticles constituting the vacuum state, Prof. K. P. Sinha suggested that this provides further support to the Einstein-de Broglie ideas, in the sense of a medium in which the quantum particles move.

Prof. E. C. G. Sudarshan discussed a dynamical model for the coupling of a quantum object and a classical apparatus. Dr R. Bhandari spoke on the coherence and tunnelling in macroscopic systems. An interesting discussion of the entropic formulation of uncertainty relations was presented by Dr M. D. Srinivas.

A highlight of the conference was the special lecture delivered by the Nobel Laureate Prof. S. Chandrasekhar on "The gravitational collapse and the problems of singularities in general relativity" (*Patrika* 9, page 2). On the second theme, there were several lectures on symmetry breaking and its ramifications. Prof. G. Morandi described spontaneously broken symmetries in quantum many-body systems. Profs. G. Marmo, S. P. Mishra and A. N. Maheshwari discussed Kaluza-Klein theories and gravity-induced weak symmetry breaking in supergravity. Prof. G. Rajasekaran spoke on colour symmetry breaking, and Profs. S. N. Biswas and T. Pradhan on gauge models and measurement of quantities through gauge fields. Dr N. D. Haridass gave a talk on gauge theories on a lattice, a rapidly developing field. Prof. A. P. Balachandran discussed his recent work on the nucleon as a soliton, which extends the pioneering work of Skyrme 20 years ago. Talks on symmetry breaking in condensed matter physics included one on a broken symmetry approach to the liquid-solid transition and the crystalline solid by Prof. T. V. Ramakrishnan, and one on broken symmetry in antiferromagnets by Prof. C. K. Majumdar. Dr R. K. Kaul spoke on Solitons in supersymmetric field theories. Symmetry breaking in the early universe was the subject of a talk by Prof. K. P. Sinha covering his recent suggestion that above a critical temperature gravity becomes repulsive.

Other talks which were mathematical in character involved the group manifold approach by Prof. T. Regge, group theoretical methods in optics by Prof. N. Mukunda and a survey of boson stochastic calculus by Prof. K. R. Parthasarathy. Other topics were quantum first passage problems and blocking of evolution by repeated observations by Prof. N. Kumar, Bose-Einstein condensation in spin-polarized hydrogen by Dr K. N. Shrivastava, quantum mechanical tunnelling by Dr D. K. Roy, subnatural line-widths in a radiation matter interaction by Prof. G. S. Agarwal and scaling phenomenon in small-division problems by Rahul Pandit.

Apart from lectures in theoretical physics, there were 7 evening lectures on the life and work of famous Indian scientists, on S. N. Bose by E. C. G. Sudarshan, on P. L. Bhatnagar by B. Buti, on M. N. Saha by J. C. Bhattacharyya, on H. J. Bhabha by P. M. Mathews, on

G. N. Ramachandran by V. S. R. Rao, on Harish-Chandra by M. S. Narasimhan and on C. V. Raman by S. Ramaseshan.

The proceedings of the Symposium will be published shortly.

PRAMĀNA

Report by the Editors

Pramāna, a monthly journal of Physics, started in 1973 as the first of the theme journals of the Academy, will be completing its 24th volume in June 1985. It has the unusual distinction of crystallising the collaborative effort among the Indian Academy of Sciences, the Indian National Science Academy and the Indian Physics Association. Pramāna covers the full range of theoretical and experimental physics, from pure speculations on the one hand to applied instrumentation on the other. A strict refereeing system with two independent referees has paid dividends in maintaining a consistently high standard, making the journal a premier vehicle for the publication of physics articles from India.

The Editorial Board meets periodically to ensure steady improvement in the journal. Among recent changes are the introduction of a Letters Section for rapid publication of important discoveries and the acceptance of review articles. The inclusion of the Physics and Astronomy classification scheme number for each paper has greatly assisted in the choice of referees for each paper and the publication of the abstracts in the standard abstracting journals.

Yet another major policy change is the decision to publish felicitation volumes to honour outstanding scientists. One such volume to commemorate Dr Raja Ramanna's 60th birthday was published in March 1985. The Golden Jubilee year of the Academy saw the publication of two special issues in April and October 1984.

The number of pages published each year has shown an upward swing in recent years, a measure of the acceptance of the standing of the journal and a result of the many years of devoted and dedicated effort on the part of the first Chairman of the Editorial Board. The Academy's decision to enlist a professional agency to promote and distribute the journal abroad is also beginning to bear fruit. Clearly the community of Indian physicists must set their eyes on making the journal an accepted international forum for publishing the best papers in physics.

The silver jubilee volume in July 1985 will see a slight change in the title of the journal. It will bear the title of *Pramāna — Journal of Physics* instead of Pramāna — a Journal of Physics. The dropping of the article 'a' may appear as a minor deletion. It is something more — a movement towards the international form of the journal, which no longer requires an explanation of the Sanskrit word. Another change will be to include a list of articles accepted for publication in later issues of the journal. Readers will thus get 1 or 2 months of advance notice of articles of interest to them.

Papers in astronomy and astrophysics, which used to be published in Pramāna, are now published in the Journal of Astrophysics and Astronomy, started in 1980. The question of starting other theme journals is under the active consideration of the Editorial Board. Since about 40 per cent of the papers published in Pramāna now are on condensed matter physics, and about 30 per cent on nuclear and particle physics, it might soon become necessary to start additional theme journals in physics.

Obituaries

In the passing away of **Dr S. Y. Padmanabhan,** Indian science has lost one of its leading plant pathologists. Suchindrum Yegnanarayana Padmanabhan was born on 11 September 1916. He graduated from the Presidency College, Madras with an Honours degree in Botany in 1937 and then joined the Indian (then Imperial) Agricultural Research Institute, Pusa for his Associateship in mycology and plant pathology. In 1939 he started his career as a Senior Research Assistant at the Sugarcane Research Station at Pusa. Leaving his Pusa job in 1943, he successively worked as Mycologist, G.M.F. Campaign, Bengal (1943-46) and Assistant Mycologist, Anakapalle (1947) until he was appointed Mycologist, Central Rice Research Institute, Cuttack (1947-66) and later Director (1966-76). His selection as Director of the Institute, was a rare distinction for a plant pathologist, as Directorship of such institutes usually went to plant breading scientists.

There is little doubt that the training he had at the Presidency College, Madras under the late Prof. T. Ekambaram and at the Pusa

Institute under the late Dr B. B. Mundkur and the experience he gained in applied botany and agriculture, contributed to his success as one of the most competent agricultural scientists. His work at the CRRI led to the mapping of rice diseases and the initiation of studies aimed at control of disease, especially the blast disease of rice. There were also major breakthroughs in the areas of higher yields and productivity and implementation of the newer ideas of integrated pest and disease control. Padmanabhan was elected a Fellow of the Academy in 1970. For the original research work he did, the Utkal University awarded him the D.Sc degree in 1974. He was President of the Indian Phytopathological Society (1973) and a recipient of the Rafi Ahmad Kidwai Prize (1974).

As a plant pathologist, Padmanabhan was especially interested in breeding for disease resistance, plant disease forecasting and integrated plant disease management. The last official position he held was as Adviser to the Assam Agricultural University (1977-78). However, he continued to be active and was about to take up work at the Centre for Advanced Study in Botany, Madras University on the problem of integrated plant disease management, when he suddenly passed away. Apart from his competence as a scientist, Dr Padmanabhan was a person whose friendship, geniality and kindness will be cherished by all those who knew him.

Prof. Mulk Raj Sahni was born in Bhera (now in Pakistan) on 1st March 1899 in a family of distinguished Indian scientists. A son of the late Prof. Ruchi Ram Sahni and the younger brother of the late Birbal Sahni, he was educated at the Government College, Lahore, Emmanuel College, Cambridge and the Imperial College of Science and Technology, London from where he obtained both Sc.D and D.Sc degrees in geology.

Before joining the Geological Survey of India in 1929, he worked for a short period as Professor of Geology, at Banaras Hindu University. During his 29 years of service with the Geological Survey of India as Palaentologist and later Director, he reorganised the Palaentological Division of the Survey creating five research sections for the study of Invertebrates, Vertebrates, Micropalaentology, Palaeobotany and Palynology.

After his retirement from the Survey, Prof. Sahni founded the first Advanced Centre of Palaeontology and Himalayan Geology in India and the first Geology Department in the Panjab University at Chandigarh. During his tenure as Professor of Geology (1958-64), he created facilities for the study of several branches of geology particularly palaeontology, stratigraphy and tectonics. He was distinguished in many fields of geology and will be always remembered for his contributions to British and Indian Palaeontology, general geological problems, tectonics and to engineering geology.

He was the founder President of the Palaentological Society of India and an Honorary Fellow of the Palaentological Society of USSR. He was a Foundation Fellow of the Academy.

His autobiography "Landmarks and Strandlines" was completed three months before his death on 13 January 1983.

He is survived by his wife, a son and two daughters.

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