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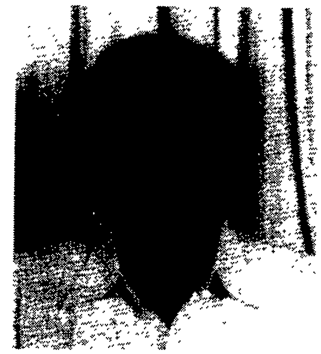

Evolution of an astrophysicist

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1. The primordial days

I was born in the city of Kolhapur in Maharashtra but brought up in the northern city of Benares, as it was then spelt in the British India (After independence, the name was changed to Banaras and subsequently to "Varanasi".), almost from time t = a few months. The reason for this transition was that my father, Vishnu Vasudeva Narlikar was then Professor of Mathematics at the Benares Hindu University (BHU, hereafter). Indeed, before I proceed further a few words about my parents would be in order. Both of them hailed from Kolhapur.

My father was the youngest son of Vasudevashastri (Narlikar) who was a Sanskrit scholar and well known for his public discourses on religious texts. I have met several old Kolhapurians who recalled his knack of conveying abstract religious and philosphical ideas in a style appreciated by the common listener. However, he died before my father had reached his teens. It was fortunate nevertheless that my father, who had already shown marks of scholastic excellence received proper guidance in his school and college days to go in for an academic career. Thus after the Vidyapeeth and Rajaram High School in Kolhapur, he went to Bombay, to the Elphinstone College and later to the Royal Institute of Science. His name is marked on the Roll of Honour at the RIS (-now the "R" has been dropped) as he topped the Bombay University Examination when he took his B.Sc. degree. Thereafter he went to Cambridge[1] for the Mathematical Tripos examination where also he performed with distinction having won the Tyson Medal for astronomy in 1930.

In 1932 while he was a research scholar working with Eddington as an Isaac Newton Student at Cambridge, with the prospects of spending a year or so at the United States at one of the leading astronomy centres like Harvard or Caltech, my father's future career received an unexpected turn. He was visited in Cambridge by Mahamana Pandit Madan Mohan Malaviya, the founder of BHU who had come to England for the Round Table Conference. Ever on talent spotting hunt for his University, the Mahamana had heard of my father and wished to invite him to join the BHU as a Professor of Mathematics. Thus it was that when

[1] By default, "Cambridge" in this article will stand for the University of Cambridge, England.

my father returned to India in 1932 with the intention of going back to Cambridge he visited the BHU and after seeing the academic environment there, changed his mind, and accepted Malaviyaji's offer to join as Professor and Head of the Department of Mathematics. He was hardly twenty four at the time... and he remained at the university till 1960.

My mother was born in the old Kolhapur family of Shrikhandes who were known as Huzurbazars, their acquired title as the domestic bursars of Chhatrapatis, the rulers of the Kolhapur state from Shivaji's dynasity. In a striking departure from those days, my mother was encouraged to take up higher studies and took the M.A. degree of Bombay University in Sanskrit. She was married to my father in 1937.

Perhaps I should mention that my parents shared a common interest in Sanskrit. Indeed my father tells me of an episode in his life when an obvious injustice in the marking of his Sanskrit paper in junior college made him opt for mathematics! It was my mother, however, who got me interested in the Sanskrit classics.

2. Seeds of growth

I have gone into these biographical details to highhight the fact that I was fortunate to grow up in an academic environment. Indeed, beyond the confines of my family, the BHU campus itself was enjoying a golden age. Its theme song "Madhur Manohar Ateeva Sundar, Yeh Sarva Vidya Ki Rajadhani" (This sweet, delightful and very beautiful place is the capital of all learning) was well justified with scholars and students from all corners of the country congregating to its beautiful campus. The Vice-Chancellor was no less a person than Dr Sarvapalli Radhakrishnan. (Ironically, after independence, when the BHU received the exalted status of a "Central University" it began to lose its All-India character).

Mathematics was my favourite subject : in other subjects I had to put in effort whereas in maths everything seemed to follow smoothly. My father, sensing my aptitude for the subject, began to introduce me to all those treasures that (unfortunately) lie well beyond the reach of a typical schoolchild. A schoolboy usually has his father as the ideal to emulate : I was no exception in wanting to grow up to be a professor of mathematics. Towards the end of my school career, we had one of my maternal uncles, Morumama, staying with us as he had come from Kolhapur to the BHU to do his M.Sc. in mathematics. He later became Professor of mathematics at the Institute of Science (the old RIS) and was universally acclaimed for being that rare commodity, a good teacher.

This trait of his was already apparent to me when we started interacting. In our house we had a couple of wall black boards for me and my brother to work on. Morumama would dig up some mathematical puzzle or a geometrical rider or some other fiendish problem and would write it on the board under the title: "Challenge Problem for JVN". Then as a challenge it would remain there till either I solved it or admitted defeat. By attempting and solving such problems I had gone way ahead of my school syllabus and this training turned out to be useful to me for solving the tough problems of the Mathematical Tripos that I was to face later in Cambridge.

3. Recombination of interests at Cambridge

Following my father's footsteps, I also applied for admission to Cambridge after completing my B.Sc. at BHU in 1957 and was admitted to his old college, the Fitzwilliam House (now renamed Fitzwilliam College). Before going to Cambridge I received extensive and useful briefing from another uncle of mine, Vasantmama, elder brother of Morumama, who had done a Ph.D. in probability theory at Cambridge under the guidance of Harold Jeffreys, Eddington's successor to the Plumian Professorship at Cambridge. The postwar Cambridge that he knew was closer to what I was going to, compared to the prewar Cambridge that my father knew. For me, however, it was a phase transition. Not having lived away from home for the first nineteen years, I was now expected to go to an altogether different country and not to return till I completed the three year course. More significantly, I was going from a relaxed system of teaching and examination to one where the policy was "swim or sink".

Another briefing which emphasized this in no uncertain terms, I received from Piroja J. Vesugar, the Iron Lady who served as the Director of the J. N. Tata Endowment for higher education of the Indians. Both my father and uncle had been Tata Scholars and now I was to join their company. She gave me several practical doses of advice and admonition...it was only later that I discovered that underneath that stern exterior lay a warm and understanding heart, reserved only for the achievers.

The Mathematical Tripos in the fifties did not believe in early specialization. You specialized only in the Part III, which was a precurser to research. The majority of students completed Part II in three years, got a Cambridge degree and then opted for careers, like teaching, industry, politics and diplomacy, etc. elsewhere. With my limited financial resources I decided to do Part II in two years and the third part in the third year. This meant moving on a "fast track" along with the best of the native undergraduates. In retrospect it all looks fun borne of challenge. Not so in those hectic days which included a scooter accident that broke my leg and I had to appear in the Part II examination with my leg in plaster. However, it was all worth it when I attended the traditional ritual announcement of the result from the gallery of the Senate House by the Chairman of the Examiners, and heard my name being called amongst the "Wranglers".

By now, as I mentioned earlier, I had a taste of different branches of maths and since I had already decided to opt for research, now was the time to recombine my interests and decide on a subject of specialization. For me the decision in favour of astronomy had already been made for the following reasons.

Amongst the lecturers I had heard at Cambridge, those dealing with astronomy were the most inspiring. In particular, the most charismatic was Fred Hoyle who had just succeeded Harold Jeffreys to the Plumian Chair. His lectures on general relativity and cosmology were most interesting. They described a subject that was opening out with promises of surprises and challenges. So at the end of the Part III of the Tripos when I emulated my father in winning the Tyson Medal, I followed him up further by registering as a research student under the Plumian Professor. A scholarship from the Cambridge University and further support from the Tata Endowment made it possible for me to continue at Cambridge for another period of three years.

I recall the bright and warm June morning in 1960 when I walked up to Fred Hoyle's residence in Clarkson Close for my first briefing. I was shortly returning to India for a two month holiday and wished to know how I should plan my initial months as a research student. The matter was important, more so as Hoyle himself was to spend the autumn (Michaelmas) term at Caltech and so I would be missing his guidance for this initial phase.

We sat outdoors sipping lemonade while discussing possible research problems to undertake, ranging from stars to cosmology. I noticed, however, that Hoyle had not mentioned the steady state theory as a subject for research. Why? I asked. He replied that it being a rather controversial subject he did not feel that a Ph.D. student should get into it. Somewhat reluctantly, I settled for a problem dealing with spinning universes. Let me describe it next.

Spinning Universes: The idea of a spinning universe had first been mooted by the mathematical logician Kurt Gödel in an article in the Reviews of Modern Physics [2] in a special issue devoted to the 70th Birthday of Albert Einstein. Such a model is anti-Machian in the sense that in the local inertial frame the distant parts of the universe are seen to rotate. (Back in the 1890s Ernst Mach had noted that the distant universal background determines the local inertial frame, and he had sought to relate it to his concept of the origin of inertia.) Spinning universes had excited attention because of the possibility that the outward centrifugal force generated by rotation in the universe may be able to combat the inward force of gravity and thereby avoid the singular states of infinite density and breakdown of spacetime geometry that beset the standard big bang models. In particular, O. Heckmann and E. Schücking had generalized Godel's model with the expectation that they would be nonsingular. Hoyle wanted me to investigate this claim and if correct, to work out primordial nucleosynthesis in them. For, if the spinning universe is nonsingular, it should oscillate and during the contracting phase the nuclei should break back to protons and neutrons as opposed to their fusion in the expanding phase.

Since Hoyle was to leave for the USA in late September, I planned to get back before that date so as to spend some time with him before his departure. I also wanted to maximize my stay in India. Somewhat arbitrarily, I chose to get back on September 6 and this date turned out to be optimal! Upon my return I rang up Fred Hoyle's residence and learnt that he had advanced his departure to the following day! However, I could see him that evening in his room in St. John's College. So jet-lagged and all, I managed to do that and it turned out to be a wise decision. For, he arranged for me to visit the relativity group at King's College, London and to attend the seminars conducted there by Felix Pirani who would look after me while Hoyle was away. These seminars turned out to be very useful to me.

In research, however, the situation is different from solving a Tripos problem. For, here one does not know what the answer to the problem is going to be. By the time Fred Hoyle returned from the United States, I was able to show that the Heckmann-Schücking models

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[2] Rev. Mod. Phys., 21, 447 (1949).

do not produce realistic oscillating universes. So the question of doing nucleosynthesis calculations did not arise. Thus, I found myself looking for another problem to tackle. It landed on my lap in an unexpected fashion.

Radiosource Counts: In January, 1961, Martin Ryle and his colleagues at the Cavendish Laboratory announced the result of their latest radiosource survey which led them to the conclusion that the universe is evolving. In particular, they found that if they count radiosources out to different flux levels, the faint ones predominate and are more in number than would be expected in a static Euclidean universe with a uniform density of sources. More importantly, Ryle & Co. argued that the number flux density relation found by them showed that there were more sources per unit volume in the universe in the past than today, and thus it disproved the *steady state theory* proposed by Hermann Bondi, Tommy Gold and Fred Hoyle in 1948. For, the "steady state" assumption requires that the number density of sources should remain the same at all epochs.

This finding was posed as a direct challenge to Fred Hoyle! Did his theory become invalid or was there a loophole in the Ryle claim? Hoyle had the germ of an idea: what if the ability of a galaxy for becoming a radiosource increased with its age? By 1960, the optical identifications of radiosources were beginning to reveal the preponderence of elliptical galaxies which were considered to be rather old. For a random observer in a steady state universe, the older galaxies would on an average be farther away and combined with the above age effect it might lead to the kind of source counts that Ryle was finding. Hoyle set me the task to work this out. Unlike the present times when one would go to a workstation to do the sum all one had then was the primitive EDSAC computer which had to be programmed by the machine language punched on a paper tape, or one could use the Facit hand calculating machines. Time was limited, as Ryle was set to present his result to the Royal Astronomical Society on February 10, the second Friday in February, 1961. But within ten days we were able to hammer out a reasonable looking counter-example.

For me February 10 turned out to be a day of ordeal by fire. Because, Fred Hoyle discovered that a prior unavoidable engagement prevented his attending the RAS meeting that day. So he asked me to present our work as a reply to Ryle.... in marked contrast to his earlier protective instinct that sought to shield a Ph.D. student from a controversial field! He rehearsed me on what to say, how to say it and most importantly, how to manage within the alloted ten minutes. In the end, it went off well judging by the responses I received from several leading astronomers present there. In any case, the episode helped me acquire self confidence so that I could hold on my own in future scientific debates.

Later I followed it up with a more detailed computer simulation, perhaps the first of its kind in the new era of electronic computers. Realizing the limitations of the EDSAC at Cambridge, Hoyle rented time on the newly established IBM 7090 in London. I would go to London with a programme in the morning, punch cards if necessary and give them to the operator at the IBM. In the evening the results, if any, would be handed over. If there were bugs in the Fortran programme this would mean another trip to London.

I had occasion to recall this experience a few years ago to the IUCAA graduate students who were unhappy that during the construction of our buildings they had to walk two hundred metres to get to their workstations. Presumably in the future, their own graduate students would compalin if they have to move from one desk to another in the same room to use a computer.

4. Growth of research : The linear regime

The Ryle episode brought me in closer contact with the steady state theory which eventually formed the main part of my Ph. D. thesis. But I should mention another intervention that led me on to another research track which kept me occupied for several years. The intervention was by Hermann Bondi at a summer school in Verenna situated on Lake Como and it source lay again in the Einstein 70th Birthday Volume of Rev. Mod. Phys. but in a different paper.[3]

I recall it as a seminar on a wet day in June 1961, delivered by an even wetter Bondi who, reeling under hayfever was continually sneezing and blowing his nose. But that did not take away the excitement of the work he was describing... work on action at a distance: the electrodynamics which originated with the work of John Wheeler and Richard Feymann (whose second paper had appeared in the above Rev. Mod. Phys. volume). It will take me too far from the main theme of this article to describe it *in extenso*; but I will give a brief resume of it here.

The Wheeler-Feynman Theory: Although the study of electrodynamics started as an action at a distance theory, it later switched to the field format when James Clerk Maxwell could successfully explain all observed features of classical electrodynamics with the help of interaction between particles and fields. But some twenty years prior to Maxwell, in 1845, Gauss had wondered whether the action at a distance concept could be made to work if the action propagated not instantaneously, but with a finite speed, "*such as the speed of light*" *(sic)*. But attempts to follow this path led to fresh difficulties. For, if one charge affected another by a delayed, i.e ., retarded interaction, by Newton's third law of motion, the second charge should react back on the first by advanced interaction. Clearly, if advanced interactions occur in reality, they would play havoc with our causality-based experiences.

In 1945 and 1949 Wheeler and Feynman wrote two important papers in the Reviews of Modern Physics, on what they called the *absorber theory of radiation.* In these papers they highlighted the role of the universe in such a delayed action framework. If the universe is a perfect absorber, it will suppress all the anti-causal advanced interactions. They found that the static Euclidean universe does satisfy this condition. However, in their choice of a static universe Wheeler and Feynman had hit upon a singular case for this model is itself time-symmetric! Thus when they reversed the sign of the time coordinate, they found that the universe also allows for the purely advanced interactions to survive the interference process. In fact, they needed an extraneous time asymmetry to make a distinction between the two cases. For this they chose thermodynamics. By the choice of a-priori initial conditions which

[3] J.A. Wheeler and R.P. Feynman, *Rev. Mod. Phys.,* 21, 424 (1949) and *Rev. Mod. Phys.,* 17, 17 (1945).

are time-asymmetric and lead to the observed thermodynamic arrow of time, it is possible to make a distincion in favour of the purely retarded solutions.

Early in 1961, Jack Hogarth had taken a second look at this problem and found that had Wheeler and Feynman chosen to work within the framework of the expanding world models they would not have found this ambiguity of advanced as well as retarded interactions both providing self consistent solutions. Hogarth noted that retarded interactions are absorbed in the future light cone of typical particle whereas the advanced ones absorbed along the past light cone. The two light cones may not behave identically in a typical expanding universe, as they do in the static Euclidean case considered by Wheeler and Feynman.

If the future half of the universe (with respect to an observer) is a perfect absorber, but the past half is not, the universe is able to suppress all advanced interactions from being observed. The reverse is true if the past observer is perfect but the future one is not.

This was the result that Bondi reported at the Verenna seminar. What had excited him was the result that the steady state model satisfied the correct response condition, viz, the future absorber being perfect but not the past absorber whereas the standard ever-expanding big bang models satisfied the reverse (and wrong!) response condition. Both Hoyle and I shared this enthusiasm. The beauty of the result was that it did not depend on numerical details of matter density, Hubble's constant, etc, i.e, parameters that all along have posed measurement problems to astronomers. In short, the kind of interpretational uncertainties that plague the standard observational tests of cosmological models (of the kind we had previously encountered with the radiosource counts) were absent here. By observing a purely local experiment in the laboratory to check that electromagnetic signals indeed propagate into the future only, one could draw important cosmological conclusions.

This was thus a starting point for a long line of investigations for the two of us, as Hogarth's investigations still left many important questions unanswered. Could action at a distance be generalized to curved spacetimes used for describing realistic cosmologies? Was it quantizable? Was quantum electrodynamics via the action at a distance format superior to the field theoretic version, especially in dealing with the ultraviolet divergences that have still not been sorted out in the latter? And, as a global paradigm, can action at a distance replace field theory in all branches of physics? Our investigations continue to this day, although we have provided affirmative answers to all except the last question. For me it was a particularly satisfying moment when the Reviews of Modern Physics published in 1995 an article by Fred Hoyle and me[4] reviewing the current state of progress of action at a distance electrodynamics at the classical and the quantum level. By demonstrating that correct cosmological boundary conditions can lead to the elimination of divergences in QED we had essentially completed the programme started by Wheeler and Feynman fifty years earlier.

Conformal Gravity: Could action at a distance be extended to other interactions? Our explorations led us in 1964 to an elegant theory of gravity that, on the one hand, had its origin

[4] Rev. Mod. Phys., 67, 113 (1995)

in Mach's principle and, on the other, led to general relativity as the many-particle approximation. Again I shall confine myself only to the concepts.

Mach's principle originated in the last decade of the last century and had raised important issues concerning interia that had remained unaddressed since Isaac Newton's time. For example, in the *Principia*, Newton has described the rotating bucket experiment. It is an experiment that is simple to perform but raises very fundamental issues. Suspend a water-filled bucket from a hook in the ceiling; give a twist to the hanging rope and let go. As the twist unwinds, the bucket rotates. And one notices that the water surface becomes curved, rising towards the periphery. Newton could explain this with the concept of intertial forces which arise when one is viewing a dynamical system in an accelerated frame. The rotating bucket provides such a frame and the inertial force is the so-called centrifugal force. But what bothered Newton was that here was an absolute effect (viz. the curvature of the water surface) that tells you which frame is rotating and which is not. What fundamental criterion does one use to identify the non-rotating frame? Newton got round this conceptual problem by simply postulating the absolute space, as an idealized reference frame that happens to be non-accelerated and in which his laws of motion would hold without having to invoke interial forces.

Ernst Mach, in his book *The Science of Mechanics*[5] took a critical view of this problem and argued that rather than postulate an absolute space one could identify it with the background of distant objects in the universe... Newton's bucket may be considered to rotate with respect to this background. Granted this observation, Mach raised further questions. Since the concept of interia is defined with relation to the laws of motion and the laws require the universal background, does it not follow that it is the background that defines inertia? Can one meaningfully describe the motion of a single particle in an otherwise empty universe?

Mach himself did not define his principle in any precise or quantitative way, but it has influenced a great body of physicists and philosophers of science. Einstein himself was influenced by this thinking and hoped to incorporate it into his theory of relativity. Later it was demonstrated that the relativity theory does not incorporate Mach's principle (vide the Godel model described earlier), and Einstein felt that the fault lay in the action at a distance concept.[6] Others nevertheless sought to continue efforts to bring Machian ideas into theoretical physics.

Since action at a distance had proved workable in electrodynamics, Hoyle and I felt that it was ideally suited to give quantitative expression to Mach's ideas. In particular, one could use the action at a distance technique for defining the inertial mass of a typical particle in terms of the rest of the particles in the universe. In addition we used symmetry arguments including the concept of conformal invariance (which had worked in electrodynamics) to

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[5] Ernst Mach : The Science of Mechanics, Chicago : Open Court (1983).

[6] For details see the "Autobiographical Notes" by Einstein in "Albert Einstein, Philosopher, Scientist" edited by P.A.Schilpp, The Library of Living Philosophers 1949; and also "Mach's Principle : From Newtons Bucket to Quantum Gravity" edited by Julian Barbour and Herbert Pfisteer, Birkhauser, Boston (1995).

almost uniquely fix this interaction. Conformal invariance described how, by varying the local standard of length (and time), one could also alter the mass scale in order to bring the equations back to the same form. Maxwell's and the actions at a distance electrodynamics both satisfy conformal invariance, as does the Dirac equation in quantum mechanics; but not the general theory of relativity. Our conformally invariant gravity theory therefore fitted in well with these other theories.

The theory had a manifestly Machian origin : and it reduced to the standard general relativity if one chose a conformal frame in which all particle masses were constant. Thus we had established the link between Einstein and Mach. But in addition, the theory also provided the answer to the question as to why the force of gravity is attractive and not repulsive. For, the so-called gravitational constant that this theory leads to in the *Einstein limit* of constant masses, is positive. Moreover, the theory also demonstrated that the spacetime singularity that inevitably arises in general relativity is due to one's insistence on this particular conformal frame.

For me Thursday June 11,1964 was another important day when Fred Hoyle and I were invited to present this theory at a meeting of the Royal Society. Presenting a new theory of gravity with the portrait of Isaac Newton looking down can indeed be an experience. The general reception was favourable and the theory also received considerable publicity at the popular level.

The conformal theory did not, however, make predictions different from general relativity in the weak gravitational fields and as such there were no known experimental tests to distinguish it from the theory of relativity. In the late 1970s and early 1980s, I suggested a variation of this theory to explain the anomalous redshifts in quasars and galaxies. In the 1990s this theory was used as the basic theory for the quasi-steady state cosmology. I shall return to these issues later.

I received my Ph.D. from Cambridge in 1963 and it was a special pleasure for me to have my parents present in Cambridge on the occasion. By then I had become a fellow of King's College, Cambridge and was enjoying the life of a Cambridge don with all the perks that a fellowship brings. I was particularly fortunate that my neighbour in King's was no less a person than the legendary E.M. Forster. With his former association with the princely states of Dewas and Kolhapur, Morgan Foster felt a special affinity towards me, a Kolhapurian! Even though he had a background steeped in classics and the liberal arts, he was very much interested in what was going on in cosmology. On many occasions I had explained to him the latest developments in the subject and I recall a memorable occasion when Fred Hoyle paid him a visit and the two discussed subjects covering both arts and the sciences. My lasting regret : that I did not tape record that dialogue between the two great minds.

In 1965 I paid a visit to India at the invitation of the Indian Council for Cultural Relations. It was a two month visit and during that period I visited several educational establishments, delivering lectures. Everywhere I encountered tremendous enthusiasm and adulation with huge crowds turning up for what should have been a technical or semi-technical talk. I was somewhat bewildered and sometimes overwhelmed by this reaction which had been generated by the publicity enjoyed by the new theory of gravitation. Seeing large crowds I had to "tone down" the level of my talks and although it was a pleasure for the human ego to be so honoured and feted, I had the disappointment that barring a handful of academia, there were very few who made the effort to understand what my work was all about.

It was, however, a privilege to meet Prime Minister Lal Bahadur Shastri and the Education Minsiter M.C. Chagla, both men of integrity whom I respected and President Radhakrishnan whom I had admired from a distance during my schooldays at the BHU. There were numerous offers and invitations to come back and settle in India, but I deferred the decision till I carried some of the work described above to a more advanced stage of completion. There was also another development in the offing at Cambridge which made it more attractive for me to stay on there for a few more years. I describe it next.

The Institute of Theoretical Astronomy: Fred Hoyle had felt all along that with the new astronomical revolution round the corner Cambridge should have an active theoretical centre for research in astronomy and astrophysics that would not only have a good core staff but would also attract a large number of visitors from other astronomical institutions and observatiories. With Fred Hoyle's great reputation and persistent efforts, the Institute of Theoretical Astronomy came into existence in 1966. I was amongst its first appointees as staff members. The IOTA did demonstrate how lively a place a scientific institution can be. I had watched at close hand Fred Hoyle's vision translate into reality and the attendant moments of pleasure, fulfillments as well as frustration. Little did I expect that when I reached his age I would be involved in a similar exercise under Indian conditions.

5. Career in India : The non-linear regime

In 1969 I wrote a letter to Prime Minister Indira Gandhi expressing my intention of returning to India in 1972 (when the first six years of IOTA would be over), recalling the offers made by Shastriji and Chaglaji. She wrote back in cordial terms promising to create conditons suitable for my work in any institution that I wished to join. During my visits to India in 1960, 1963, 1965, 1966, 1968-69 I had closely watched the academic scenario in India and based on this experience I opted to join the Tata Institute of Fundamental Research. Given the choice I would not have liked living in Bombay (which has become even less livable now), but the conditions in TIFR and its academic environment came closest to what I was accustomed to at Cambridge. Thus I was happy to receive an offer from M.G.K. Menon to join the institute at a convenient time. After devoting considerable thought to this important matter I decided to join in October 1972.

It would be an interesting sociological exercise to see how the pattern of return to India by students who had gone abroad for higher studies has changed with times. My impression is that during the British Raj, barring a few exceptions these scholars invariably returned to take up careers in the ICS or in the universities. This homeward flow continued in the fifties but now there were more who went to the USA in preference to the UK and many of these began to settle there. In the sixties the conditions in our universities began to decline in a significant way and the brain drain to the west began to pick up. Thus my decision to return to India after some fifteen years abroad was considered somewhat exceptional and also adventurous but it was nevertheless appreciated and welcomed. Somehow, since 1964 I have continued to be in the limelight and there have been countless occasions when "the man in the street" has complimented me on puttling my trust in the mother country. I myself have not regretted that decision.

My wife, Mangala (nee' Rajwade) whom I married in 1966 backed me up in this decision. We felt that we would rather bring up our young daughters (then 30 and 4 months old) in an Indian environment. My parents were expected to stay with us after my father's retirement as the Lokamanya Tilak Professor in the University of Poona (now Pune) and so we would ideally have liked a three or four bedroom flat. Unfortunately, such flats did not exist in the TIFR housing colony.[7] In the end we settled for a two bedroom flat in the colony within seven minutes walking distance to the institute. I preferred this to a larger flat faraway and commute across the ever-growing Bombay traffic. Indeed, considering the burdens of running a household under Indian conditions of scarcities, looking after the in-laws, and growing children, I think my problems were far less than those of my wife. It is to her credit that while dealing with all these daily chores she managed to complete her Ph.D. degree in mathematics and then continue a teaching career.

At the TIFR, I was put in charge of the Theoretical Astrophysics Group which then had hardly half a dozen members. During my sixteen odd years the group grew to over fifteen members with a reasonbly wide coverage of A & A from comets to cosmology. My own interests continued partly along the lines adopted at Cambridge and partly along new areas like high energy astrophysics, relativistic astrophysics, quantum cosmology, cosmic tachyons, etc. During my Cambridge tenure at the IOTA I had guided two students to their Ph.D.s and informally advised another from London (this last one being Paul Davies). At the TIFR I continued to have a steady input of research scholars and I learned considerably from the experience of guiding them as well as from the lecturing that I did in the graduate school.

Since returning to India, however, my sphere of activities grew non-linearly beyond the steady growth of research output. I began to discover that I enjoy writing popular science articles and delivering popular scientific talks and public lectures. I also discovered that there is a considerable scope in the media, both print and electronic, for cultivating public interest in science. Unfortunately not many scientists are coming forward to particpate in this activity.

Cambridge, and the U.K. in general has a long tradition of science popularisation. In A & A alone one may mention names like James Jeans and Arthur Eddington, followed later by Fred Hoyle, Dennis Sciama, Clive Kilmister, etc. My maiden attempt at popular science writing was an article in the magazine *Discovery* in 1964 in which I described the phenomenon of gravitational collapse in the light of the discovery of quasars and the discussions at the Dallas Symposium (the first in the series of what have come to be known

[7] It was expected that such flats would be built by 1973-74, but it happened some twenty years later, long after I had left TIFR!.

as the *Texas Symposia)* on the emerging area of relativistic astrophysics. That effort had proved rewarding and I had continued writing such articles occasionally. Likewise, the experience of listening to as well as delivering evening lectures at societies of undergraduates at Cambridge and other British universities had also opened out to me the potentialities of public lecturing.

My own (highly personal) observation about the Indian scientific establishment is that it has its own caste system. The top caste is occupied by those who direct institutions, attend committees and commissions determining the fate of science funding as well as of fellow scientists. Next come research workers: theoreticians followed by experimentalists while the last on the list are science popularisers. A single scientist may be engaged in more than one of these activities: the above caste structure relates to the relative importance and recognition attached to them. If we are to truely reflect the priorities for a healthy growth of science in this country and its impact on society, this caste system needs to be eliminated.

It was around 1974 that I began to experiment with science popularisation in my mother tongue Marathi as well as in Hindi, a language I was comfortable with because of my BHU days. It was not easy and the effort needed considerably exceeded doing the same in English. But in terms of impact on the audience these efforts were rewarded many times over. Emboldened by these experiences I also took a stab at writing science fiction in Marathi, again finding considerable scope for it. Indeed,I have preferred travelling to a small town for delivering a public lecture to jetting it to Delhi or Bangalore for a committee meeting.

Looking back, I find that my 200 months at TIFR were on the whole enjoyable and satisfying, in terms of my research, teaching, writing and science popularisation. Nevertheless, over the years, especially in the mid-eighties, I had begun to be alarmed at the widening gap between the research institutes like TIFR and universities. Universities which had served as cradles for academic activity in the pre-independence era had begun to lose their quality and momentum after 1947. The trend was already apparent at BHU when I was an undergraduate there. Rather than take note of the growing malaise, and taking corrective action, the powers that be seem to have decided that the disease was incurable and that the salvation for Indian science lay outside the universities - in the research institutes.

If the research institutes were created with a mandate to add to and improve the research and teaching facilities in universities, their effect would have been beneficial. Instead they grew up as ivory towers to which only a selected few would gain entry. This "holier than thou" attitude has helped neither the institutes nor the universities. While the universities are deprived of adequate facilities, the institutes complain of not getting good students, not realizing that this species has to be bred and nurtued in the universities. Working at TIFR I was increasingly feeling my isolation from the universities.

In TIFR, awareness of the problem prompted in 1985, an experiment of outstation teaching at the Physics Department of the University of Pune. Several of us participated in selecting motivated students (who were awarded UGC scholarships) for doing an M.Sc. degree and in teaching them courses at Pune.

I wish the experiment had continued and become a standing feature to be emulated at other places. There are reasons as to why this did not happen and these ought to be studied by experts in the sociology of science in India.

6. The creation of large scale structure at IUCAA

In late 1987, fresh developments took place that brought me closer to the university sector and led ultimately to my transition from TIFR to the campus of Pune University.

They began with the decision by TIFR to site the Giant Metrewave Radio Telescope project near Pune and to shift the bulk of its radioastronomy group to the campus of Pune University. The university welcomed the move and made land available to the TIFR for locating its radio astronomy centre there. The expectation was that the university would benefit directly as well as indirectly by having such a major national project on its campus. However, considering the importance of the GMRT project, Yash Pal, Chairman of the University Grants Commission felt that a more ambitious effort should be made by the UGC so that the benefits of the GMRT project were shared by all universities.

Indeed. taking the broader view, the UGC realized that astronomy and astrophysics as subjects for teaching, research and development were neglected in universities. Workers in these fields were working more or less in isolation in the mathematics or physics departments in several universities, and they lacked adequate facilities in the form of library, computers, data, instrumentation, access to observatiories, contact with the latest developments in their fields, etc. Rather than provide these facilities in all universities (which would turn out to be enormously costly) where they may remain under-utilised, it would be better to create a national resource centre at one place for all university users. Indeed, this requirement fitted well into the "Inter-University Centre" format which the UGC had just begun to try out. The first IUC, called the Nuclear Science Centre, was coming up around the pelletron facility on the campus of the Jawaharlal Nehru University. So why not create an Inter-University Centre for Astronomy and Astrophysics (IUCAA) at Pune right next to the GMRT Centre?

After considerable brainstorming on this idea, Yash pal asked me if I would take on the responsibility of setting up IUCAA and give directions to its programmes. I went through the pro's and con's. The pro's were: (i) Here was a chance of getting involved in an activity that would definitely be beneficial to the university sector. (ii) This gave me an opportunity of interacting with the university academia. (iii) It was a challenge that had fired my imagination. The con's were : (i) I was leaving a well cushioned position in a leading institute for a place where no infrastructure existed. (ii) I was warned by leading scientists that relying on UGC's continued support was risky. (iii) I was similarly cautioned by pukka-Bombayites that by going to Pune I was going into the wilds.

Certainly, leaving TIFR to set up IUCAA was a phase transition and the pluses and minuses had to be carefully weighed not only by me but also by my family. We had gone through a similar exercise in 1972 when the phase transition was even more drastic... but we were sixteen years younger then! As on that occasion, we decided to take up the challenge and I said "Yes" to Yash.

Then things moved pretty fast for Indian conditions. On December 29, 1988, IUCAA had already become a Registered Society, and the first series of meetings of its apex bodies took place in the morning while in the afternoon Yash Pal laid the foundation stone of the centre's buildings. Of the many fortunate things, that worked themselves out for the nascent institution, was the participation by Charles Correa as the architect of the buildings, an ambitious complex of some 20,000 sq. m. built up area. The IUCAA Campus today has certainly surpassed the early expectations.

But on June 1, 1989 when I left TIFR to join IUCAA as a full-time Director, I found myself on the doorstep of the Golay Bungalow where a single room of floor area of around 10 sq. m. was all that IUCAA had to begin its humble existence with Naresh Dadhich from the Mathematics Department of Pune University had joined as Project Coordinator and his knowledge and contacts within the local environment were to be immensely useful. A skeleton infrastructural staff also joined to share the burdens.

I will not go into the trials and tribulations of those early days[8] but will draw the line at December 28, 1992 when the IUCAA buildings in their full glory were dedicated to their users by G. Ram Reddy, the then Chairman of the University Grants Commission. The occasion was made more memorable by the Dedication Lecture delivered by the Nobel Laureate S. Chandrasekar, an Honorary Fellow of IUCAA.

The academic programmes for which IUCAA had been set up had, however, not waited for the buildings. As our built up area increased we increased our academic programmes and the infrastructure. There were several shifts of the Library, for example and of the offices. The interaction and participation of university faculty and students grew as the available space and facilities expanded. Now that everything is in its rightful place a more aggressive strategy for involving the universities will follow.

My own research has continued uninterrupted despite these "distractions"! In 1992, in a collaborative programme with Fred Hoyle and Geoffrey Burbidge I have become involved in a revival of the old steady state idea into a more viable form as the "quasi-steady state cosmology" (QSSC). In the mid-1960s Hoyle and I had worked on a scalar field formalism to describe creation of matter within the framework of Einstein's equations. In 1966 we had arrived at a cosmological model that was to prove to be strikingly similar to the inflationary model that Alan Guth proposed fifteen years later. The QSSC combines the original features of the steady state theory with the idea of oscillatory fluctuations (cycles of expansion and contraction) arising from switching on and off of the creation process. The basic gravitational theory for this cosmology is the 1964 Machian theory proposed by Hoyle and me.

Thanks to the support I receive from my colleagues I am able to indulge in my research and activities of science popularisation. I sometimes say (not entirely jokingly) that my role as the Director of IUCAA has become redundant!

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[8] See the "IUCAA Story" for details!