

*Silver Jubilee Article***My forty years in astronomy**

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This reminiscence is basically in two parts. The first part deals with the period 1973-80, when I was the Editor of the Bulletin of the Astronomical Society of India, and the other part covers my academic and research activities.

### 1. From the Editor's desk

Considering the growth of the astronomical activities in the country, it was decided to form the Astronomical Society of India. Three of us, K.D. Abhyankar, S.D. Sinhal and I drafted the rules and bye-laws for it. The society was formed, registered and it was decided to have a regular society publication, reflecting the astronomical activities in the country. In the early 1973, M.K. Vainu Bappu, the President of the Society, asked me to be the Editor of the society publication and to take the necessary steps to start a quarterly publication. I was somewhat hesitant but finally accepted the responsibility. The name of the publication was registered and the first issue came out in the second half of 1973; it contained the constitution and the bye-laws of the society, among other things. It was printed at the Onlooker Press, not an ideal press for scientific publications but it was located close to my office and its rates were reasonable for a young society like ours; however, it required proof reading 4-6 times, consuming an enormous amount of my time.

The Bulletin contained review articles, short research notes, society news, conference reports and starting volume 3, Editorials, which contained general items of overall interest and research developments. As I wanted to make the Bulletin an important research publication, I tried not only to get good review articles for it but devoted considerable effort to enlarge its circulation abroad, both institutional and personal. Research articles were always refereed, frequently requesting a referee from India and another from abroad. Some of the referees were very helpful. I still remember that a referee used an image intensifying technique to improve the photograph in a paper sent to him for refereeing. Another sent 30-40 xeroxed pages of an article, not easily available, for the author of the paper. To advertise the Bulletin, we made reciprocal arrangements for publication of our advertisement in the Astrophysics and Space Science, Solar Physics, the Observatory and the Publication of the Astronomical Society of

Japan. A supplement to the Bulletin was published, whenever large astronomical data needed to be printed. The Bulletin items were included in the Astronomy and Astrophysics Abstracts, INSPEC was also going to acknowledge it but my efforts to get it included in the science citation index did not succeed.

The Aryabhata satellite was to be launched around March 1975. I, therefore, wanted a picture of it for the cover of the March 1975 issue of the Bulletin. ISRO, when requested, refused to release any picture of the satellite even though they had published it earlier. I did not give up. I used my contacts in the Times of India and the March 1975 issue of the Bulletin did have the picture of Aryabhata on its cover.

I wrote an editorial on the level of research in astronomy in India using Science Citation Index (SCI) in vol. 4, no. 4 of the Bulletin. This was written while I was abroad and had an access to the SCI. When I returned to India, some senior astronomers expressed their unhappiness on this editorial; it was not surprising as their SCI was very low.

Total solar eclipse of 1980 was a very special event for us as the path of totality passed through India. We decided to bring out a special issue (vol. 8, # 2 & 3, 1980) to mark the occasion, with articles dealing with all facets of the solar eclipse, including biological sciences. We did receive a large number of articles; unfortunately, none of the biological articles managed to pass the referee.

I was taken aback when, in 1980, I heard that the Indian Academy of Sciences is planning to bring out a Journal of Astrophysics and Astronomy. I was not happy with this development as I did not think that the astronomical activities in India were of such a magnitude as to support two publications. I, therefore, talked to some senior council members of the Academy, gave them a written note about my views on their request and sent a copy of it on my own to M.K. Vainu Bappu as well. The Journal did start in 1980. I still feel, however, that we do not require two astronomical publications from India.

My aim was always to improve and enhance astronomical activities in India. My editorial based on SCI reflected that, even though it was misunderstood by some. Bart Bok was visiting India and he visited most of the centres where astronomical work was going on. I therefore requested him to write a Guest Editorial for the Bulletin, giving his appraisal of the activities going on at the various institutions. This report did contain some critical remarks about some of the centres. These adverse remarks were meant to trigger improvements. The Editorial was leaked out and acrimonious debate took place between the Director of one of the institutions, the President of the Society and me as an Editor, in which some members of the Editorial Board also joined in, and even Bart Bok was forced to get involved in it. I strongly believed in Editorial independence and so I resigned even before the completion of my second term.

I am happy that the Bulletin, which I started in 1973 is still going strong and is celebrating its **Silver Jubilee**. I feel proud that I was part of it in its infancy. I wish the Bulletin a bright future in the years to come.

## 2. Astronomical research - India, the USA and the Netherlands

I joined the University of Delhi in 1953 after completing my M.Sc. in Physics from the Banaras Hindu University, and registered for Ph.D. to carry out work on "Statistical Mechanics and its applications to Astrophysics". While working on the mass-radius relation for cold bodies (Vardya 1955), I felt the need to get a formal training in Astronomy. I, therefore, joined University of Michigan in 1955 for graduate studies and after two years, shifted to Yale University to work on my thesis on "The envelopes and interiors of late-type stars" under Prof. Rupert Wildt.

The main emphasis of the thesis was to compute hydrogen-helium adiabats incorporating the physics of molecular hydrogen to provide better boundary conditions to the radiative cores in late-type stars (Vardya 1960a ; Vardya & Wildt 1960). I had almost completed the work, when, while writing up, I discovered, to my shock, a mistake in my almost first equation. After I had convinced my thesis supervisor of my mistake (he was very nice and kind to invite me for dinner that evening to lift my sinking spirit and soothe my disappointment). I revised the whole thing including extensive numerical calculations, though it made a difference only in the third place of decimal. This work made a remarkable impact with almost all the later stellar evolution codes incorporating molecular hydrogen in their physics.

During the above calculations, I found that molecular hydrogen persisted even at high temperatures. This implied that either the equilibrium constants used for  $H_2$  did not hold at these high temperatures or the effect is real. If the latter is true, then  $H_3^+$  may be important in these regions.

While taking my morning shower one day at Berkeley, the idea of pressure dissociation, analogous to pressure ionization, suddenly occurred to me. Not knowing how to compute the effect of pressure dissociation, I wrote a small note on it, describing the basic idea with a hope that someone more knowledgeable may get interested and compute the effect. Everyone liked the idea but discouraged me about publishing the idea note. I did send it and it was published (Vardya 1960b). Later on, I did compute lowering of dissociation energy in  $H_2$  and  $H_2^+$  due to pressure (Vardya 1965a, 1966 a,b).

I had written a programme to compute grey model atmospheres (envelopes) incorporating molecular hydrogen and mixing length formulation of convection. Prof. L.G. Henyey and his group were using this code in their stellar evolution calculations. I wanted to use this programme to compute march of molecules in the envelopes of late-type stars. Before I could do more than the preliminary calculations, my time was up and I returned back to India. However, as luck would have it, I was back at Berkeley after a year and a half, and was able to resume the project with vigour. After extensive revision / improvements in the model atmosphere code, opacity code and molecular equilibrium code, I finally completed the work at the Joint Institute for Laboratory Astrophysics (JILA), Boulder, Colorado and wrote it at Utrecht Observatory, The Netherlands (Vardya 1966c). This work, started in 1960 got

completed only in 1966, but it was worth while going by the attention it received in the literature. Prof. P. Swings wrote to me a beautiful letter and later sent me a large number of reprints of 'unsuccessful' efforts made to solve this problem.

My stay at JILA made me look at the problem of march of negative ions in late-type stars (Vardya 1967). While at it, I convinced Prof. L. Branscomb about the need of laboratory work on  $\text{SH}^-$  and the work was done, through his good offices, at the National Bureau of Standards.

I had spent a lot of effort in writing the code for the model atmosphere. But I wanted this paper to be written jointly with Prof. Henyey. First he was reluctant but finally agreed. It became a part of his 'Studies in Stellar Evolution' series. In it we examined the role of various parameters on stellar evolution and it became a classic paper (Henyey et al. 1965).

I published a letter in 1966 entitled "Are carbon stars helium rich?" (Vardya 1966d). In 1967, I had gone to the General Assembly meeting of the IAU held at Prague. There I met Kurt Hunger, who told me with great excitement about his work on helium rich early type stars with high abundance of carbon. I mentioned to him to have a look at the above letter in the Observatory. Not only he did, he has been very faithful in referring to it whenever he has written a review on hydrogen deficient stars. To my amusement, the letter surfaced again, when in 1985 two groups, one in the USA and the other in Europe decided to test it numerically with model calculations; David Lambert flashed the first page of the letter at the IAU Colloquium No.87 held at Mysore, when I was chairing the session.

In the literature, there is enough confusion between  $\gamma$  = ratio of specific heats and  $\Gamma$ , the adiabatic exponent defined by  $(\partial \ln T / \partial \ln P)_{\text{ad}} = (\Gamma - 1) / \Gamma$ , because for a perfect gas, they are equal. This difference was brought out very clearly for pure photon gas (radiation) for which  $\Gamma = 4/3$  but  $\gamma \rightarrow \infty$  at non-zero temperature. Besides my paper (Vardya 1965b; see also Vardya 1964), Cox and Giuli (1968) is the only other reference where this distinction is clearly stated.

The sources of opacity in late-type stars were a big problem in computing decent model atmospheres. The idea of using pressure broadened wings of CaI line  $\lambda 4227 \text{ \AA}$  in M2V star was an exciting possibility. Fortunately, George Wallestein had a plate of HD95735 (M2V). It was amazing to see how broad the wings of this line was in this star. And we were able to estimate the factor by which the then known opacity could be off from reality.

During my one year sojourn at Utrecht (1965-66), my time was mostly spent in writing papers, the work for which I had done earlier at Berkeley and Boulder. So, when I joined the Tata Institute of Fundamental Research in late 1966, I had a clean slate with no unfinished work. Being alone in astrophysics, to start with, was hard enough and to set up a group in Theoretical Astrophysics was a new experience. But I got full support from all sides and S.P. Tarafdar started working with me for his Ph.D. soon after my joining. I asked him to look into sources of opacity in cool stars, with emphasis on molecules. This work led us to explain the missing solar ultraviolet opacity to the band absorption due to CO, SiO, OH and CH (Tarafdar and Vardya 1972). At the General Assembly meeting of the IAU held at Sydney in

1973, I just went into the commission meeting of "Observations from outer space". Several speakers were slated to speak on the missing UV opacity. However, after the first speaker, I drew attention to our work and that was the end of it during that session.

It was a singular honour for me when in 1969 Leo Goldberg invited me to contribute an article on the "Atmospheres of very Late-Type Stars" for the "Annual Review of Astronomy and Astrophysics", volume 8 (1970). This was the first article from India in this series, and it was very satisfying that our work in the field has been recognized (Vardya 1970a).

I was at NASA Goddard Space Flight Center for two years (1975-77). There, I tried to improve the mixing length formulation of convection by maximization of efficiency coefficient. I got a very good expression, with certain assumptions, and after writing it, I, unfortunately, threw away most of my work sheets, as I was returning back to India. At Bombay, I tried to check that expression but failed. I could not find why and so this major work remained unpublished, even though I presented preliminary results at several places in the States.

An IAU Colloquium on "Spectral classification of the future" was being organized at the Vatican in 1978. I was very much interested in attending this meeting but did not feel proper to ask for the funds from the Institute as I had never worked on spectral classification. As luck would have it, Father McCarthy was also interested in having me at the Vatican. He telexed me that if I am willing to give an invited talk on "Empirically oriented approach to spectral classification", the organizers would fully bear my travel and stay expenses. It was a very exciting possibility but it took me two days to accept the challenge and send him an affirmative reply as I had never worked in the field. With Helmut Abt chairing the session, I started my talk (Vardya 1979) as follows: "There is no better place for making a confession than the Vatican. Let me start my talk by confessing that I have never classified any stellar spectra. However, let me hasten to add.....". I raised in my talk two questions:

- (1) What happens to spectral classification at other wavelengths? and
- (2) What happens to it at other dispersions?

There were some comments after my talk on these questions but far more differing comments came afterwards, so much so that Janet Lesh talked with W.W. Morgan on the telephone for almost half an hour. The "Revised M.K. Spectral Atlas for Stars Earlier than the Sun" (Morgan et al. 1978), which was published soon after the meeting, contained clarification on these very two questions in bold letters. Even though I was not given credit, I felt happy that I have contributed, in a small way, to spectral classification.

When the Guest Observation programme was announced for the International Ultraviolet Explorer (IUE), we put in an application with a view to explore molecules in all kinds of astronomical objects, as the resonance lines of a large number of molecules fall in the region covered by the IUE. We did get six shifts and observed not only cool stars but hot as well. And it was really exciting to discover CO in the circumstellar shell of 9 Cep, a B21b hot star (Tarafdar et al. 1980).

A large number of empirical expressions, with mass (M), radius (R), and luminosity (L) as variables were available in the literature for stellar rate of mass loss. I was not very happy with these completely empirical fits. So I tried to approach it from the dimensional analysis point of view with M, R and L as variables and introduced gravitational constant G and velocity of light c for introducing some physics in it, and derived semi-empirical relations using observed rate of mass loss. I had difficulty in publishing this paper as the referees raised objections (like why G and c, and not e and h), which I was not willing to buy. As I was fully convinced of my arguments, and considered the referees comments as frivolous, I did publish it (Vardya 1984; see also Vardya 1987) and used these very results in other publications.

One of it was on the role of rotation on the rate of stellar mass loss. Most of the workers were using small samples, confined to small range of M, R and L, and even projected rotational velocity,  $v \sin i$ , and did not find any effect. I felt that this is because the scaling (proportionality) constant was absorbing most of the effect due to rotation. Therefore, I took a very large sample, with large range in spectral classification, M, R, L and  $v \sin i$ , and found that rotation definitely enhances the rate of mass flux (Vardya 1985; see also Vardya 1990).

I was fortunate to be invited to Amsterdam in 1985 to make use of the IRAS data in my field of cool stars. It was a difficult year to go due to personal reasons and to the impending IAU General Assembly and the associated symposia and colloquia. But I did go for three months and looked into the LRS spectra of mira variables, as I was looking into the properties of long period variables at that time. This was really my first experience with raw data but I was fortunate to obtain some interesting results on silicate feature and identify SiO emission as well (Vardya et al. 1986). This brief visit helped me in proving several other properties of long period variables, later on, using the IRAS data.

It was therefore puzzling when in 1988 I was invited to contribute a write-up on the long period variables for Astronomy and Astrophysics Encyclopedia (Vardya 1992), as my interest on this subject per se was only three years old. When I received the volume, to my surprise I was the only one to have any contribution from India.

It is perhaps not a good thing but I have found myself more than once, contradicting other peoples results or finding errors in their derivation/calculations (Shah and Vardya 1972; Tarafdar and Vardya 1970, 1975; Vardya 1970b, 1975, 1987). Once we found that the published results were off by eight orders of magnitude; the referee wanting to avoid confusion between factor of eight and eight orders of magnitude insisted that we write it as  $10^8$ .

### 3. Some odds and ends

Life gives varied experiences. I used to stammer a lot in my early days, though I told myself that it is psychological and not physical. To overcome it, I used to take part in debates, whenever possible, not always with pleasant consequences. It continued even when I went to the U.S.A. In 1957, at the University of Michigan, Ann Arbor, in the Solar Physics Course, I was to speak on Sunspots. I wrote the talk and read it fast lest I stumble and stammer. After the talk, I answered the questions calmly and without stammering. Several persons commented, why did

I read the talk as you know the things so well. That really gave me confidence and my stammering really decreased such that now very few people even know that I used to stammer a lot.

When I went to Berkeley in 1959, Prof. L.G. Henyey was the Chairman of the Astronomy Department. I found out soon after reaching there, that Prof. Henyey is a perfectionist, instead of encouraging may even discourage you, and may never return the paper given to him for comments. Knowing the ground rules, the things went very smoothly. For example, he wanted me to work on his stellar evolution project on day to day basis; I refused but developed the stellar atmosphere code independently and then got it integrated in the stellar evolution code. My rating with him went up after I was awarded the JILA Annual Visiting Fellowship, (in spite of the fact that he had forgotten to send his confidential report), so much so that he offered to give me a regular position in the department; I declined as my first priority was to return to India. I have never regretted that decision.

I really wanted to investigate the stellar structure of cool stars and their evolution. But circumstances destined me to remain only on the surface. My several attempts to probe the deeper layers did not fructify for various non-academic reasons, both in India and abroad.

I was normally sending my papers for publication to the *Astrophysical Journal*. However, an action of Prof S. Chandrasekhar as Editor of the *Astrophysical Journal* in 1964 made me extremely unhappy; knowing my limitations, I did not send any paper to Ap.J. from then on till he was the editor.

In conclusion, I enjoyed working abroad but have loved to be a part of the Indian scene. I only hope that my contribution to Astronomy was of some use. Whatever I have been able to achieve would not have been possible without the support and encouragement of my family, my teachers and guides, my colleagues and co-workers and I owe them, one and all, my sincere gratitude.

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