REFLECTIONS ON Be STARS AND THE Be PHENOMENON

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ABSTRACT. After three specialized meetings on Be stars, it appears that the Be phenomenon is more complicated than we have thought. Five fundamentally different models are now competing. All five are actually based on an assumption that the Be stars are only a specialized (perhaps more easily noticeable) case of a more general phenomenon. Carefully arranged observations may decide between competing concepts, for example between spheroidal and disk-shaped circumstellar envelopes, and they can decide on the location of the cool strata (emitting Balmer lines) and hot strata (N V, C IV, Si IV). Eclipsing interacting binaries provide valuable clues.

A TALE OF TWO SYMPOSIA AND ONE COLLOQUIUM

Be stars are B stars with emission lines in their optical spectra. In my private definition, the Be phenomenon includes both the Be stars and the Be people, because science is, after all, human. By popular demand, supergiants of high luminosity are excluded from Be stars; supergiants of high IQ are not excluded from the Be people. Be stars differ very little from ordinary B stars, and Be astronomers differ only a little from other astronomers, except that they become a bit touchy if you praise a model which is not theirs. Therefore, it came to me as a big surprise when I asked the participants of this Colloquium to vote on the following question: "How many fundamentally different models are needed in order to explain all the Be stars?". Only three astronomers raised their hands when the proposition was: "Only one model suffices." However, they are well-known proponents of three very different models, so that their vote actually means: "At least three!" And so went the overwhelming consensus of the participants. In fact, one of them came to me afterwards to complain that there should have been one more option, namely: "It's an example of chaos in the spirit of the preceding talk by Dr. Hearn."

I believe that the realization of the inherent complexity of the Be phenomenon is one of the fundamental conclusions one has to draw from the three recent meetings on Be stars. This is a strikingly different situation compared to the idyllic times that still prevailed some 20 years ago. Then, the model outlined by Otto Struve in 1931 was almost generally accepted as <u>the</u> explanation, and was only being modified from time to time in order to accomodate new findings.

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I am happy that my lobbying for the first symposium fully dedicated to Be stars helped stir the quiet waters. According to George Bernard Shaw, each of us has, for every action, two reasons: one noble and the other real. In my case the noble reason was my opinion that it was high time to revise Struve's model. I found it partly challenging and partly irritating to read Struve's account that "The revolving-ring of gas hypothesis, in rapidly rotating stars, was written in the course of a rather long evening." (Struve 1958). Struve was, of course, "the astronomical giant of the twentieth century" (Huang), "by native instinct...he was beyond other men" (Herbig (1970), and, should you wish to hear a somewhat different opinion, "...when it came to an interpretation of this /new spectroscopic/ evidence, Struve exhibited also an imagination worthy of a modern poet, and the daring reconnaissance spirit of the cavalry man he was in the days of his youth" (Kopal 1981). Still, I felt, nearly fifty years later, we should be able to do better. And we did. I am tempted to say that at our first meeting, at the Symposium No. 70 in 1975, I had a definite feeling that Struve's model was "gone with the wind", I mean stellar wind. True, his basic picture of a rapidly rotating star surrounded by line-emitting gas remains; but the idea of a star shedding a ring of gas just as a consequence of its rapid rotation was found untenable. Su-Shu Huang devised ingenious variations of the elliptical ring model, but such a thin ring cannot explain the absorption lines of shell stars; and there are other objections (Marlborough 1976).

It was realized that some additional force must support the circumstellar gas, and the search for such a force was the main topic of the 1975 Symposium. Many people gladly embraced the concept of the stellar wind, which was becoming very popular then, thanks to the Copernicus satellite. And the stellar wind model has stayed with us since, being revised steadily and becoming more and more complex and varied.

Yet another new model was added at the 1975 Symposium, that of an interacting binary system. Going back to the two reasons for proposing the Symposium, my real reason was to present the binary model. At the present (1986) Colloquium, Peter Conti formulated a nice parody on my 1975 opening remarks by saying "If you want a donkey to pay attention to you, you hit him hard over his head several times." Actually I had been more gentle and used the case of my friend the playwright who had long speculated how to make the audience forget their daily worries and concentrate on what's happening in the theatre right at the curtain-up time. In the end, he concluded that nothing else would do but to fire two cannon shots from the stage into the audience. Before the Symposium, in the years 1969-1973 (see Plavec 1976 a,b), we kept saying that mass transfer in an interacting binary provides a natural source of the circumstellar gas, which comes to the gainer with an excess of angular momentum, which then provides a natural support for the accretion disk. If, in addition to that, the gainer is a B star, we will observe emission lines and have a Be star. But new ideas must be presented not with a whimper but with a bang; and this was done at the Symposium by Peters, Polidan, and in the first place by Harmanec and Kříž, who shortly before (Kříž and Harmanec 1975) proposed that all the various Be stars are interacting binary systems.

Six years and 18 IAU Symposia later came the Munich Symposium, No. 98. The number of participants jumped from 53 to 90; number of papers presented from 48 to 81; and number of pages in the published Proceedings from 465 to 519. Here intervenes the well-known spectroscopic phenomenon of saturation: The publisher will not want an even bigger volume and the participants get saturated with talks, too. Enter three competing models: the classical one of an elliptical ring ejected by a rapidly rotating star; stellar wind from a single star, stimulated or enhanced by rapid rotation; and an interacting binary, supplying the envelope and the rapid rotation by mass transfer from an unstable component. The output of the meeting was a total of five models, although I feel that the meeting meant a final demise of the elliptical ring model. Two new models emerged: that of a non-radially pulsating star, in which a surface wave travels around the star in the direction opposite to rotation, and may drive mass ejection; and a model invoking a complex outer structure of a B star with a chromosphere and a corona, dominated by a mass flux from inside as well as a non-radiative energy flux, in addition to radiation.

The constant emergence of new models testifies to two things: that the Be model-builders are always on the alert for new ideas emerging in other fields, and that they react promptly to new observational facts. At the time of the first Symposium (No. 70), the concept of stellar winds was just emerging from the first ultraviolet observations of stars much more luminous and as a rule hotter than are the Be stars; yet it was immediately seized and applied to Be stars by Hutchings, Marlborough, and others. The concept of mass transfer in close binaries was also very young in 1975, yet its potential for explaining the Be phenomenon was quickly realized. Similarly, Baade, Bolton, and others applied the then new concept of non-radial pulsations to Be stars at the Symposium No. 98. The strong influence of new observing techniques can also be easily traced. In 1975, we felt the strong impact of infrared data, of the first ultraviolet spectra supplied by *Copernicus*, and of the first polarization studies. The 1981 Symposium was strongly influenced by the extensive studies of stellar winds in the ultraviolet spectra provided by the *IUE*, by X-ray observations, and by the quickly growing wealth of data coming from the southern hemisphere.

Another trend can also be recognized: the growing realization that the Be stars are not an isolated group of objects, as they appeared to be at the time of the Struve-McLaughlin-Huang elliptical ring model. In the stellar wind model, the Be stars are a continuation of the Oe stars; the strong radiative push provided by the high-luminosity OB supergiants is believed to be partly provided by the rapid rotation in the less luminous Be stars. The binary star model implies that in the wide sea of various types of interacting binaries, there are cases that by chance the mass gainer happens to be a B star, that is, provides enough ionizing photons to create Balmer emission lines radiated from the accretion disk. The hypothesis of non-radial pulsations stresses the affinity of the Be stars to their other neighbors in the Hertzsprung-Russell diagram, namely to the pulsating Beta Cephei stars. And the model of the chromosphere-corona complex dominated by the non-radiative energy flux from the deeper layers of the star, as advocated by Thomas, Doazan, Cannon, and others, stresses explicitly that the Be phenomenon is only an enhancement of a structural complex which should exist in all stars.

Now, another five years later, we have met in Boulder. Anne Underhill was able to present her model of Be stars, invoking magnetic loops. This raises the number of competing models to five, if I have been counting correctly. But the situation is actually even more complex, since there are several competing versions of the stellar wind model. My feeling has been that all these models are taking deeper roots; it may be difficult to overthrow any one of them completely. A peaceful but competitive coexistence seems to be indicated.

OBSERVATIONS: A CASE FOR LOTS OF ADMIRATION AND A LITTLE BIT OF WORRY

In my attempted survey, I paid attention to the models. But a much larger component of each of the three meetings consisted of new observational data. This is, of course, a necessary and healthy aspect, and I have already commented on the influence of new observational techniques upon the development of the models. The need for further and more refined observational studies has been stressed again and again. There is no doubt that, at least in principle, observations can decide between competing models. Is the hot, superionizing envelope located near the star, as Thomas insists, or far out, as the wind model (or some versions of it) postulates? Can we find at least one bona-fide, truly classical early Be star which is an interacting binary? Some more questions of this kind can be asked and, I hope, answered rather soon.

There is the additional problem of Be star variability. A very forceful demand has been made for careful, persistent, and systematic studies of the variations. Here comes my slight worry: Are we sure that the various variations are specific to Be stars? I have some experience with high-dispersion optical spectrograms, high- and low-dispersion IUE spectra, and continuous optical spectrophotometric scans. I have never ever seen identical profiles of any one absorption or emission line, or identical flux distributions. There are always differences, at least subtle ones. Here the observer comes in. If the star is a standard or an allegedly normal star, we tend to say: well, that's the inaccuracy of the observations; but if we anticipate peculiarities, we take the differences seriously. Ideally, everyone reporting varying profiles or continuous fluxes or anything else varying, should also present check observations on some "normal" stars. And even if we do confirm that the variation is specific to a Be star, the big question is: how far should we go in our attempts to interpret and model the details? One could defend the minutest scrutiny by recalling the famous case of the cosmic microwave background radiation. When Wilson and Penzias detected it, they at one moment considered the possibility that pidgeon droppings in their horn-shaped antenna may be the source. It is perhaps an ironical testimony about our position in the universe that the tritest biological phenomenon could almost be confused with the evidence of the biggest event in the universe. One could, however, cite thousands of examples where the minutest details remain just that, the minutest, unimportant details that must be ignored by science. It is often best to ignore them, at least temporarily. Take the case of the symbiotic stars. The detailed descriptions of their spectral peculiarities and their variation with time read like horror stories if you are trying to understand the overall character of the stars. They are still unexplained (not forgotten). But substantial progress was made by taking another route: studying the ultraviolet and X-ray spectra, and developing models based on theories of stellar and binary star evolution (Friedjung and Viotti 1982).

IMPORTANCE OF ECLIPSING BINARY STARS

Eclipsing binary stars are capable of furnishing the best data on the fundamental properties of single stars, provided that we select detached systems, in which each component evolved as a single star. I believe that the interacting eclipsing binary stars can provide important data on the structure of the circumstellar envelopes in Be stars. I realized with amazement during the past Colloquium how much similarity there is in the phenomena. In both cases, we suspect disks, and encounter cool regions with Balmer emission, and hot regions with superionization to N V, Si IV, C IV, Fe III etc. And we have problems with their respective location. Except that the eclipses give us a much better chance to solve this problem; for some timid first steps, see e.g. Plavec, Weiland and Koch (1982) and Plavec (1983).

PREDICTIONS

Meetings should be concluded by predictions about future developments. I think that the next meeting on Be stars will see many new optical data, obtained at high S/N and high dispersion with the CCDs. When will we meet again? Six years elapsed between Bass River and Munich; five years elapsed between the Munich Disagreement (on models) and Boulder. Pure mathematical minds would extrapolate simply and predict a four years interval. That would be wrong; we are dealing with the Be phenomenon, and its long-term variability is quasi-periodic. Our next meeting will take place in 1992 ± 2 years, exactly.

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