CONCLUDING REMARKS

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ABSTRACT. After a brief description of the difficulties and pitfalls of finding a correct interpretation of the symbiotic phenomenon, I emphasize what I consider to be the highlights of the meeting. Directions for future research are indicated.

In this talk I shall try to stimulate the later general discussion, and try to be a little provocative. Since the first IAU colloquium on this subject at the Haute Provence Observatory, there has been much progress in the physical understanding of symbiotic stars, so we decided to treat subjects in a different order. We wanted to consider physical processes early on during the meeting, and then see how observations fitted ! If both the theory of physical processes and the observations are good enough, it should not matter where we start, as the same reality is always studied. If our concepts are sufficiently broad and rigorous and our perceptions sufficiently numerous and accurate, we may expect to be able to reach reality. If not we may hope at least to obtain a useful way of representing in our minds what we know or think we know.

One question one can ask is : is there one symbiotic phenomenon ? Also what is it or what are they ? David Allen spoke to us about a primitive mammal, which he compared with a symbiotic star. This mammal is I believe only found in Australia; many symbiotic stars have been discovered in Australia by David Allen, but we cannot conclude that they are primitive ! Such reasoning is not valid; we must be somewhat more rigorous. We must look for contradictions between the observations and accepted ideas, and test the latter. In addition, nothing must be considered impossible, so we must not be chained to physical prejudices!

Almost all who work in the field of the symbiotic phenomenon consider symbiotic stars binary. A poll conducted by David Allen at the start of our meeting showed only one colleague who supported single star models. Since then I have received a telex from another who also supports such models. They are in a very small minority. This was not the situation 30 years ago. At that time Gauzit gave what he considered good reasons for believing the symbiotic star AX Per single. In the light of present day knowledge we can say that he was not aware of the

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complexities of binaries. The work of Boyarchuk in the optical about 20 years ago turned the tide towards binary models; one can perhaps state that ultraviolet observations with IUE played the main role in finally killing single star models. In this connections I would like to say how sorry I am that Professor Boyarchuk was unable to participate in this meeting.

Binary processes are however dependent on many different parameters; indeed one can wonder whether there are not too many parameters to derive definite conclusions. Symbiotic stars are often compared with cataclysmic binaries, whose general nature "we know". The latter are believed to consist of a white dwarf accreting from a Roche lobe filling companion, usually not very far from the main sequence. The white dwarf is thought to be surrounded by an accretion disk when the magnetic field is unimportant; when the field is large accretion is thought to take place via an accretion column. If you do not believe this type of model for cataclysmic binaries you will have a lot of trouble getting your papers accepted, while even if they are accepted after being read by a soft hearted referee, they will not be read by many people ! The question is how relevant are such ideas for symbiotic binaries. The latter appear to always have a cool giant mass donnor star which does not need to fill its Roche lobe. A cool giant can have a strong wind, especially if it is a Mira variable; while the main sequence, white dwarf or possibly neutron star mass gainer believed to be present, may accrete from this wind or by Roche lobe overflow. This new "orthodoxy" which is the framework in which most of us make our interpretations is less simple than that for cataclysmic binaries. Red giants and their winds are less well understood than stars near the main sequence, and all sorts of other physical processes can occur.

It is very easy to fall into traps or to become confused, even if the basic model just described is accepted as true. Some years ago I supported a model in which the symbiotic phenomenon was due to increased solar type activity of the cool giant, associated with a higher rotational velocity than for normal cool giants, because of tidal locking of the rotational and orbital periods. A region similar to the solar transition region might then produce the high ionization emission lines observed, while small variations in the wind from the cool giant could cause large changes in the accretion rate to the compact component, and hence in the nature of any accretion disk. This model was proposed because early IUE observations of Z And suggested that the hot continuum was not hot enough to produce the highest ionization lines in photoionized regions, while some lines at least were formed in a region where high temperature radiation was diluted, that is far from that where the hot continuum was formed, while a certain form of reasoning suggested that this region was thin. The fact that high ionization resonance emission lines of CI Cyg unlike other lines of this star were little or not eclipsed also seemed to support the model. However it now appears that enough high energy radiation is generally present for photoionization. Mikolojewska showed that the high ionization emission lines of CI Cyg had radial velocity variations probably in phase with those of the hot component, and first results on the widths of absorption lines of the cool component of CI Cyg obtained by me in collaboration with several French colleagues, suggest that the rotation of CI Cyg may not be tidally locked to its

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orbital period. The model may also have other problems. However even if effects of increased activity of the cool giant are less important than I thought, the possibility of their presence should not be forgotten in future interpretations. In any case the wind from the cool component seems often to be dominant for emission line formation, event if it is photoionized by radiation from the hot component.

We can now ask whether our meeting has succeeded. Our theoretical models are still very incomplete, so we had to talk a lot about observations on the first day, though the programme was arranged in such a way that we were building up towards theoretical models. The second day was devoted to theory, but when we tried to compare theory with observations of individual objects on the third day, something funny happened. The individual stars did not always seem to like our theories very much, so our models clearly need much improvement. Talks on related objects on the fourth day may however help us in this.

If I try to see what were the highlights of the meeting, the work on orbits seems particularly important. The work on the radial velocity variations of the absorption lines of the cool component has very much strengthened the binary interpretation since 1981; at that time the lack of reliable orbits was a weak point of such an interpretation. I was very impressed by the work on determining orbits from the reflection effect, even for such crazy objects as symbiotic stars where other effects can be strong, one can get reasonable results which even agree with those obtained from radial velocities. Physically, changes in the amount of accretion due to variations of the mass transfer rate associated with an eccentric orbit cannot be important in the amount of radiation emitted by a symbiotic binary for which the method works; either the luminosity directly due to the accretion is small, or the orbit is circular.

The cool component of a symbiotic binary is more "normal', and better understood than the hot one; the talks on the cool component helped us to better understand these components, which are a good starting point in the study of any symbiotic binary. The differences between binaries containing a Mira variable and those containing another type of cool giant were made very clear for us and should be the basis for future classification. Classification into S and D type symbiotics can have traps, as shown by the study of IRAS observations. Symbiotic stars may also make an important contribution to the study of the formation and destruction of circumstellar dust.

At the end of the first day several talks were given on observational methods which can give information about the geometry of symbiotic stars and their nebulae. Radio and optical observations show large deviations from spherical symmetry for the nebulae; jets and bipolar structure is common. Indeed even if an object appears spherically symmetric, it was pointed out that we may be seeing a bipolar structure nearly along its axis ! Polarization methods need perhaps to be further developed before all their possibilities can be realized.

The theoretical talks were on relatively simple physical models that is on photoionization of the cool star's wind, colliding winds, accretion disks including their instabilities and possible formation following accretion from a wind, and on thermonuclear processes resulting from accretion on to a white dwarf. All

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these and probably quite many other effects need to be taken into account in interpretation; each effect does not occur in isolation from the others. A subject where progress has in particular been made in recent years is that of disk formation following accretion from a wind.

The posters were presented towards the end of the second day. I was particularly struck by a poster indicating that the CNO abundancies of symbiotic stars are similar to those of normal M stars (see review by H. Nussbaumer in this volume), one on classification from emission line ratios (winner of the poster prize of IAU colloquium n^{o} 103) and one showing that the visible surface of the active component of PU Vul is getting smaller and hotter. This selection is somewhat preliminary and personnal; reading the final texts in the proceedings of this meeting may show others to have been at least equally important.

As far as the presentations on individual stars are concerned, the fact that the luminosity of the hot component of Z And cannot be due to accretion from a wind is striking, as are the differences in outburst behaviour of Z And and AG Dra observed in the ultraviolet. In the former the very hot source previously seen disappeared (or perhaps rather cooled) as might have been expected from already known optical behaviour, while in the latter it is not clear whether the temperature changed or not. CI Cyg is important to study because of its eclipses, while the interpretation of CH Cyg is extremely uncertain. It is still not clear what is the best model for the excitation of the jet of R Aqr. I am rather surprised about the interpretation of the emission line profiles of RX Pup as produced by rings; similar stationary features are seen in the line profiles of classical novae and interpreted as due to ejection deviating from spherical symmetry, that is ejection of polar caps and equatorial rings. Why cannot such an interpretation be used for RX Pup, especially as it was active some years ago, perhaps somewhat resembling symbiotic novae whose properties were also presented ?

Among the subjects discussed on the last day let me note the interest of ξ Aur/VV Cep systems, which show certain similar phenomena to those of symbiotic stars, but in a less violent way. The detailed interpretation of such phenomena in ξ Aur/VV Cep systems may help us to see what to look for in symbiotic binaries. As far as recurrent novae are concerned, I must admit that I am somewhat sceptical about detailed interpretations. This is because even for classical novae, the measured accretion rate after the end of the explosion is too high compared with theoretical prediction, and it is not certain whether this problem can be solved by the "hibernation" model. Finally progress has been made in understanding the evolution of symbiotic and related stars.

On several occasions classical novae were mentioned and compared with symbiotic novae. Let me, as someone who has worked on novae for many years, point out some differences. Novae are cataclysmic binaries. There is very good evidence that after the initial explosion an optically thick wind is generated, which continues for a considerable time. This wind appears to be probably accelerated by radiation pressure and there are fairly good reasons for supposing the luminosity well above the Eddington limit in this stage. At such a time it is difficult to place the exploded star in the HR diagram as its surface is not seen. Physically this situation is attractively explained if the cool component is considered as then revolving inside the expanded white dwarf; viscous dissipation can produce a super Eddington luminosity, and perhaps also prevent the white dwarf moving further to the right (to cool effective temperatures) in the HR diagram. On the other hand there is as far as I am aware no compelling reason for believing that symbiotic novae and symbiotic stars in general have optically thick winds. The orbital separation appears to be much larger, so one component should never revolve inside an expanded other component, so never producing a super Eddington luminosity in this way. This is a basic diffrerence in the model, which should not be forgotten.

Let me finally turn to what the future holds for us. On the observational side we need more and better orbital data. It is particularly important to determine the orbital eccentricity, in order to see whether in some cases mass transfer variations and hence variations in the accretion rate, can be produced by varying orbital separation. This has a major bearing on certain models for symbiotic stars.

The cool companion needs to be better studied by high resolution spectroscopy in the infrared, and particularly by Fourier transform spectroscopy (FTS). By such methods we can find out to what extent the cool companion is really "normal", obtain better luminosities and determine the abundances of various elements in its atmosphere. In addition parts of the nebular envelope in front of the cool component can absorb line radiation; this geometry is different from that encountered at shorter wavelengths, and can give us new information. For instance a poster presented here by Bensammar and others including me describes FTS observations of CI Cyg in eclipse. A P Cygni absorption feature of the HeI 10830 Å line with a terminal velocity of the order of 150 km s^{-1} was observed, which was not seen during earlier observations at phase 0.5. It is in my opinion premature to interprete this...

Eclipsing symbiotic stars such as CI Cyg and as pointed out the under studied AR Pav, need to be examined in much more detail. The differing eclipses of different spectral features can give us much geometrical information. High spatial resolution methods can be expected to progress. FeII emission lines when seen, can be studied by the self absorption curve method developed by me and Muratorio, which also can give geometrical information. CH Cyg is a good example.

It was also pointed out that we must better observe PU Vul. This star, which is probably a symbiotic nova, could make the transition to the "nebular stage" rather quickly, and we should try to observe the transition.

On the theoretical side we need to better understand accretion disks. The classical Kenyon and Webbink work on the radiation emitted by a symbiotic star containing a disk was done assuming that a disk emits as a sum of black bodies; better assumptions need to be made in future. It was also pointed out that a disk emitting radiation by gravitational dissipation would be hard to detect, if it surrounded a white dwarf undergoing a thermonuclear event (though in fact such a disk should reprocess radiation from the white dwarf). The question is, can theory predict how observers should best be able to detect disks. In addition what kinds of disks are possible ? Our Greek American colleagues have proposed a thick accretion disk model for R Aqr, but as pointed out by one participant, it is not clear whether such a disk is physically possible.

Ionization models need to be further developed, and combined with detailed analysis of emission line profiles and fluxes. It is not sufficient to predict "radial velocities" for the emission lines, as already done profiles must be predicted. Future work should not only take account of the wind of the cool component, but also other regions able to emit line radiation, such as regions associated with accretion disks.

Additional problems are associated with the effect of the hot component on the cool one, on its outer layers, its wind, and on any dust that is present. What kind of wind comes from the hot component and/or any disk that may exist ? Colliding wind theories may need to be further extended, while we do not understand yet the physics of jets and bipolar flows, also seen for many other kinds of object. The last is a general astrophysical problem, studies of symbiotic stars may help in its solution.

It is clear that much work remains to be done. Our subject is still far from dead !