(Received 20 October, 1988)

GENERAL DISCUSSION

There were three periods of general discussion, reported here as one. For the most part, discussion centred on major topics, as identified in the paragraph headings. In the third period, four poster papers (selected by secret ballot) were presented and discussed in detail. A few isolated questions, relevant to particular papers, have been transferred from this record of general discussion to the records of the papers concerned.

RZ OPHIUCHI

Smak's objections to the presently accepted model of the system (L.B.G. Knee et al., Astron. Astrophys. **168**, 72, 1986) are (i) the loser does not fill its Roche lobe, (ii) the disk fills or more than fills the gainer's lobe, (iii) the theoretical ellipticity of the loser appears to be too small to account fully for the observed ellipticity and (iv) if the orbital inclination is close to 90° the disk should completely obscure the gainer from our view. (See section 1 of Smak's paper, p.107). He also drew attention to the spectral type and luminosity class (F5 I) ascribed to the gainer with the well determined mass of 6 $m_\odot$ and pointed out that some of the properties of the system could be modified if the secondary spectrum is indeed M3 III, as suggested by Olson (Astr. J. **94**, 1309, 1987) rather than K5.

Plavec replied that he and Scarfe had analyzed UV and optical scans of the spectrum. From the λ 2200 Å dip they found the colour excess to be E(B-V) = 0^{m}30, larger than formerly believed and that the effective temperature is nearly 8,500 K; i.e. the gainer must be appreciably earlier than F5 in type. Scarfe added that the scans showed no sign of TiO absorption in the green spectral region; this supports the K-type classification of the loser, as opposed to the M-type. The higher reddening makes the intrinsic colours consistent with a K-type spectrum for the secondary star. On the other hand, their attempts to obtain the (hot) gainer's visual spectrum by subtracting eclipse scans from out-of-eclipse scans indicate that the loser is brighter at quadratures than in eclipse - i.e. the loser is distorted. Olson added that he had tried to explain the visibility of the hot star through the disk in the system of KU Cyg. If dust obscures the cool star, the central plane of the disk must be cleared of all dust. Even so, the remaining dust probably has some effect on the hot star's light. The reddening found by Plavec and Scarfe should not be assumed to be interstellar in origin.

Andersen emphasized again that the rotational velocity observed for the secondary is precisely what is to be expected for the radius deduced by Knee et al. (see Andersen's comments following Olson's paper p.34). He felt there is no need to worry about the F5 I classification; it refers to the disk, not the star. Indeed, one way out of the puzzle is to assume that the semi-amplitude of the primary has been overestimated simply because the value of K_1 is determined from the disk spectrum. Hall pointed out that if K_1 is in error by only a few km s^{-1}, the system could be semi-detached. Kaitchuck pointed out that cataclysmic

Space Science Reviews **50** (1989), 323–330.

variables are well-known for distortions of their radial-velocity curves and asked if there were phase shifts between the times of eclipses of RZ Oph as expected (from the velocity-curve) and observed. Scarfe replied that primary stellar eclipse did occur at the expected phase, but Olson's observations showed the disk eclipse to be asymmetrical.

Livio emphasized that the problem posed by RZ Oph was not the formation of a disk by a stellar wind undirected by Roche-lobe overflow, but the formation and maintenance of a disk as large as that which seems to be observed. A disk can be formed around the gainer just by accretion from the loser's wind and, once formed, would tend to spread because of its own viscosity. Nevertheless, it is hard to understand how the disk in RZ Oph would stay so large, despite the deposition of material of low specific angular momentum, if the presently accepted picture of the system is correct.

DISKS IN ALGOLS

Chambliss asked what percentage of Algols had disks. There appears to be several hundred known examples of binaries containing a late B-type or early A-type main-sequence primary paired with a cooler subgiant; only for relatively few is there observational evidence of a disk. Are there any spectral signatures (such as the H and K emission lines in the spectra of RS CVn stars) that would reveal at any phase the presence of a disk?

Andersen suggested examining the spectrum for signs of shell lines and checking whether the spectral-type of the primary agrees with that obtained from the mass. A distorted velocity-curve for the primary component is also a significant clue. Batten emphasized that there must be many non-eclipsing Algols which would be indistinguishable from other relatively short-period spectroscopic binaries displaying a single spectrum of late B or early A type. Since the effects of a disk on the spectrum are generally supposed to be confined to the orbital plane, distortions of the line profile or the velocity-curve would probably not be obvious. The latter could not, in any case, be recognized if there were no light-curve with which to compare the velocity-curve. Perhaps non-eclipsing Algols could be recognized by their UV spectra, or if systematic searches were made for faint secondaries, by the methods pioneered by J. Tomkin. Budding remarked that a system could not be considered an Algol unless it was semi-detached, and estimated that about 20 per cent of Algols contained disks. He also asked if there is a fundamental qualitative difference between "classical Algols" and "W Serpentis stars". Replying to this last question, Plavec replied that a practical definition of a W Serpentis star is that the UV emission lines of its spectrum are visible at all phases. He thought that there might be a physical significance underlying this definition. He suspects that W Serpentis stars contain optically thick disks that largely or even completely hide the gainer. It may be possible, however, to detect the gainer, either by radial-velocity observations such as Johannes Andersen is making, or from study of the superionized emission lines, which betray the nature of the gainer.

PERIOD CHANGES

Hall recalled an earlier result of his that most of the systems known to show alternating period changes are Algol systems containing a convective component. He believed that the key to understanding the period changes lay in magnetic cycles associated with these components. He knew of only two systems that show alternating period changes and that do not contain convective components: U Oph and TX Her. He was confident enough of his theory to repeat a prediction he had published some years ago: that each of these systems will be found to have genuinely periodic changes of period caused by either a third body or apsidal motion. (Andersen interjected that both effects were already known to be present in U Oph, see B.-C. Kämper Astrophys. Space Sci **120**, 167, 1986.) Rucinski referred to the recent work of J.-L. Tassoul (Astrophys. J. **322**, 856, 1987) who proposed a hydrodynamical mechanism for synchronization of orbital and rotational motions that would operate whether stars had a radiative or convective envelope (although, as Bolton pointed out, the time-scales in the two types of star are very different). De Greve asked what would be the influence of mass transfer on the chemical composition of the envelope of a loser. At some stage, the mass lost would be rich in helium and this might affect further chromospheric activity in the mass-losing star. Rucinski replied that the computations should be made; perhaps Rolf Mewe in Utrecht could be asked to undertake them. Batten emphasized the difficulty, sometimes, in distinguishing alternating period changes that are only roughly cyclical from the strictly periodic effects of third bodies and apsidal motion - consider, for example, U CrB that Rob van Gent had spoken on earlier. He felt we did not yet understand the relationship between the observed period changes and the detailed evolution of Algols and recalled that F.B. Wood had spoken, as early as 1955, of his "strong conviction" that the study of period changes would be highly significant for understanding the evolution of close binary systems (in Non-Stable Stars, I.A.U. Symp. No. 3, ed. G.H. Herbig, Cambridge, p.144).

R CANIS MAJORIS

Eggleton suggested that R CMa resembles AS Eri which S. Refsdal, M.L. Roth and A. Weigert (Astr. Astrophys. 36, 113, 1974) interpreted as an evolved system that had lost both mass and orbital angular momentum. The luminosity and radius of the cool star seemed to him more consistent with those expected for an evolved, stripped-down star than with those of a low-mass, main-sequence star. It is already clear that RS CVn stars lose both mass and (presumably) angular momentum even before they suffer any Roche-lobe overflow. It ought not, therefore, be too objectionable to a theoretician to introduce mass-loss into evolutionary calculations, but there is not yet any way of estimating how much is needed to account for specific systems. Budding thought that R CMa is an extreme case, with a total mass several tenths of a solar mass less than any known comparable system. It had been suggested that R CMa has a third component, and Budding wondered if this could affect its evolution. Tomkin said that one ground for belief in a third body in the system was the reports of a variable systemic velocity. He had looked into these and believes that, in view of the broad lines and variable line-profiles in the spectrum of R CMa, the evidence for variation in the sytemic velocity is not fully convincing. Guinan said that recent reports were based on a period study. He and P.A. Ianna (Astr. J. **88**, 126, 1983) had shown that R CMa is a high-velocity star with an age estimated to be in the range of 7-10 Gyr. He thought this to be of importance to the interpretation of the system. Hilditch emphasized the importance of making evolutionary calculations of a low-mass system (say 1 $m_{\odot}$ + 0.5 $m_{\odot}$), including full allowance for magnetic braking. Hall stated that period studies of short-period chromospherically active binaries show that these systems have periods that tend to decrease systematically (D.S. Hall and J.M. Kreiner (Acta Astr. **30**, 387, 1980). The decrease would imply a similarly decreasing major axis. Possibly this is a result of magnetic braking and might indicate what the computations advocated by Hilditch would show.

COMMENTS BY S. RUCINSKI

In response to a request that he should amplify remarks made in an unscheduled poster, Rucinski made a number of comments on the general theme of light-curve solutions.

1. Proliferation of light-curve synthesis methods. I realized in Baltimore that new synthesis programs are still being written at the considerable expense of development time. They are claimed to be faster and supposedly better than existing Roche-model codes like the W-D code or LIGHT. However, I feel that we do not need those codes. Comparison of results of different element determinations is already complicated by different conventions used in light-curve codes. In addition, some methods - which are adequate only for detached (slightly distorted) binaries, such as the Russell-Merrill method or WINK - are still used for semi-detached and contact binaries producing clearly unphysical results. Although any method - provided it has enough parameters - can reproduce any light curve, only some methods have a sound physical basis. Since computer speed is no longer an issue, for the sake of easy comparison I propose that we all use one method, for example the W-D code. This code has good physics yet can be used on any existing computer, including the common PC's. I would be the first to abandon my own code and I suggest that others will follow.

2. Solutions for partially eclipsing systems. Solutions of partially eclipsing systems should - in general - not be attempted unless we have a good astrophysical (not personal) reason to do so. Very simply: such light curves contain too little information about the eclipsing system to produce a meaningful set of elements. For semi-detached and contact binaries the spectroscopically determined mass-ratios may help in constraining the solution (through the Roche-geometry). Even in this situation I will refuse to believe in any partial-eclipse solution unless the spectroscopic mass-ratio determination is a very good one.

https://doi.org/10.1017/S0252921100087911 Published online by Cambridge University Press

3. Single-colour light-curves. A single colour light-curve cannot be used to determine temperatures of both components. Relative surface brightness can - of course - be determined but absolute temperatures are indeterminable. Recent preprints of a particularly productive group contain solutions of single-colour curves which include reasonable-looking determinations of temperatures for both components. That it was possible at all is a good proof that our ideas about the limb-darkening and gravity-darkening effects are quite reasonable but we should not use the second-order effects to derive solutions.

4. One cannot replace thinking by any automatic computer method. The same group has recently used the W-D code to map the whole allowable single-colour parameter space generating 20,000 to 80,000 iterations (!) per system claiming good determinacy of elements and their uncertainties. [N.B. Some of the light curves were photographic.] This is a clear waste of computer time. I was told that during the Baltimore General Assembly the Space Telescope Institute computers were totally paralyzed by efforts of some members of the above-mentioned group to solve still more binaries (simultaneously on a few computers). Those (fortunately few) workers of the Institute who did want to use the computers during the meeting were understandably not amused.

5. We must work together not to clutter the literature with unreliable solutions. The theoreticians badly need good data but frequently cannot distinguish which solutions are good and which are bad ones. They seek advice from those of us who have had some experience in this matter. Thus, light-curve solvers who consistently produce physically unreliable data are identified and their (sometimes huge) production labelled as generally untrustworthy. Of course it is a personal matter if one wants to be labelled that way, but the somewhat shaky reputation of the whole field also suffers.

CATALOGUES OF ALGOLS

Wilson returned to the subject of catalogues of Algols. Several had been produced recently that contained many systems that were not Algols. Some systems featured in the catalogues are clearly detached and no-one would consider them to be Algols. This is misleading if the title of the catalogue leads one to expect that it contains only Algols. Budding remarked that he had deliberately called his own catalogue one of Algol candidates. We could perhaps not be entirely sure that any particular system is an Algol, and some systems, thought by some people possibly to be Algols, indeed seem very unlikely to be so. He had carefully divided the stars in the catalogue according to the likelihood of their really being Algols. Wilson replied that a student of his had checked many stars in Budding's catalogue against F.B. Wood's card catalogue and often was unable to find any significant material. It was not clear to them why these stars figured in the catalogue at all. Budding replied that many of these were probably candidates to which he had assigned "0.5" probability. Often the only information about them was the shape

https://doi.org/10.1017/S0252921100087911 Published online by Cambridge University Press

of the light-curve - deep occultation primary minimum and almost no secondary minimum. (Chambliss raised at this point the question of the old-fashioned classification of light-curves - EA, EB and EW - still used in the latest edition of the *General Catalogue of Variable Stars* and which he felt overdue for a change. Leung thought, however, that as a classification of light-curves it was perfectly satisfactory.) Batten recalled that the earlier discussion of catalogues had centred on the question whether or not there might be systematic errors in mean properties derived from collections of data individually subject to large random errors. Provided this possibility was recognized, a catalogue could be useful because it drew attention to systems worthy of study. Plavec defended Budding's catalogue, pointing out, for example, Kaitchuck had found a transient disk in the otherwise neglected system of TZ Eri, and there may be similar neglected but potentially interesting systems in the Catalogue. Of course, such a compilation, if used for statistical work, should be used critically. Wilson agreed, but expressed a wish that some body had the power to vote a system out of the catalogue!

RY PERSEI

Plavec drew attention to inconsistent results of attempts to classify the spectrum of the gainer in RY Per. Optical spectrograms give a spectral type no later than B5, possibly earlier (this is based on both the continuum and absorption lines). The continuum distribution shown on a low-dispersion *IUE* spectrum in the ultraviolet suggests that the star is B9. Although the star rotates rapidly, Plavec could not see that that could account for the discrepancy. Polidan agreed and said that the star had been included in a recent *Voyager* programme in the far UV. Although he had not yet analyzed the data, he believed the star had been successfully detected and that implied a spectral type of B2, since a star of later type, as faint as RY Per, could not be detected.

A NEW MODEL FOR ALGOL (*Richards, et al.*, p.358)

Livio commented that calculations he has made together with N. Soker and R. Dgani (*Astrophys. J.* **305**, 267, 1986) lead him to expect that most turbulence and "splashing" would occur downstream from the impact point. Nevertheless, the stream can "push" matter at the impact point and matter behind that point could flow in to replace the "pushed" matter, thus creating a turbulent region which could correspond to the HRVR referred to in the paper. Richards replied that if the interpretation of the in-eclipse profiles was correct, they may have seen a part of a transient disk (with radius 1.3 times that of the primary star) and the HRVR might be more extensive than she had shown.

In reply to a query from Smak, Richards stated that maximum difference between the radial velocity of the emission and that of the centre of mass of the primary is ±150 km s^{-1}. Similarly, the maximum for the absorption is +150 km s^{-1}. The consistency of the numbers confirms that emission and absorption arise from the same region. In response to Andersen, she stated that the HRVR was seen from December 1976 to March 1977. A large radio outburst and X-ray emission were detected in 1974 and 1975, and Bolton detected CaII K emission in 1972 and 1975. (The full version of this paper is published in Astr. J. **96**, 326, 1988).)

MATTER STREAMS IN ALGOL-LIKE SYSTEMS (Wonnacott, p.375)

Plavec commented on Wonnacott's statement that 90 per cent of the matter in the stream landed on the gainer and asked where the densest part of the stream was to be found. Wonnacott replied that the core of the infalling stream was the densest part and that it followed quite closely the ballistic calculations of S.H. Lubow and F.H. Shu (Astrophys. J. **198**, 383, 1975). In response to a suggestion from Hjellming, Wonnacott said that he had already been asked to compute a model for Algol, to compare with the observations just reported, and he intended to do so.

HIGH-MASS-RATIO CONTACT BINARIES (Hrivnak, p.348)

Scarfe drew attention to the velocity-curve of the secondary component of OO Aql, noting that the measured velocities were almost constant between phases 0.6 and 0.9, and that even the eclipse and proximity corrections Hrivnak had introduced did not satisfactorily represent this feature. Hrivnak agreed but pointed out that the problem was not present in other systems he had studied. Hill suggested that the apparent constancy might be an artifact of the method of measurement, since one tries to match a distorted profile (the secondary component of the cross-correlation function) with a symmetric one. Hrivnak doubted if this were the true explanation, since only one quadrature is affected. Wilson suggested that the local line-strength might be variable (depending on log g or the local T_{eff}) over the surface of the secondary and that this would introduce distortions in the profile that most reduction programs do not take into account, but Hilditch pointed out that most of the lines measured are those of Fe I which are not strongly temperature-dependent in spectra of F and G types. Hilditch also commented on the results for VZ Psc which implied a deep-contact system with $\Delta T \sim 1000K$. Such a result was disturbing; although the early-type system SV Cen was once thought to be in deep contact, it is now known not to be (H. Drechsel et al. (Astr. Astrophys. **110**, 246, 1982) but the neck region between the two stars is very hot and produces X-rays (See also S. Rucinski IAU Symp. No 118, p. 159, 1986). Hrivnak agreed that the implications of his solution were hard to understand physically and stated that his solution is not well determined because the system does not show eclipses. He and Guinan had looked with IUE for evidence of enhanced activity in the Ca II and Mg II lines, but had found nothing dependent on phase. He felt that the system would not easily yield well-determined physical parameters - unless we could observe it from a part of the Galaxy from which eclipses could be seen!

A GENTLE PROCESS FOR THE FORMATION OF ALGOLS (Tout and Eggleton, p.369)

Hrivnak asked if there was spectroscopic evidence for the large rate of mass loss that Tout and Eggleton had predicted for RS CVn stars. Tout replied that he knew of none, although possibly IUE spectra yet to be analyzed might show it. There is some evidence from period changes, but this is hard to interpret because it is related more directly to loss of angular momentum than to loss of mass. Hall stated that a number of papers had been published giving (mostly spectroscopic) evidence in favour of mass loss from RS CVn stars. Hall also spoke about the RS CVn system SS Cam. The more evolved and more massive star fills 95 per cent of its Roche lobe and probably will not be able to avoid the rapid hydrodynamic phase of mass-transfer described by Tout and Eggleton. Tout replied that the possible existence of such systems was predicted by their models.

Plavec commented again (see p.177) on the agreement between the work of Tout and Eggleton, on the one hand, and of Friend and Castor, on the other. They, however, assume not only an enhancement of the stellar wind, but its focussing towards the other star - so that the wind resembles Roche-lobe overflow. He asked how such focussing would affect the evolution of the system. Tout replied that if the less evolved star gains some of the mass lost by its companion, the required reduction of the mass-ratio will be even easier to achieve. If, however, the mechanism of mass loss were magnetic, focussing would be through holes in the field and not necessarily in the direction of the other star. Livio emphasized an important difference between the two hypotheses. The effect discussed by Friend and Castor works for a star that fills more than 90 percent of its Roche lobe, but the model proposed by Tout and Eggleton requires enhanced mass loss when the star fills only half of its lobe.