SIXTH DISCUSSION SESSION

(Saturday morning; 9 September, 1972)

Chairman: M. G. Fracastoro.

Peters: HR 2142 is a B1Vnne star which periodically displays a short-term shell phase. The period for the recurrent shell phase is 81 d. During the interval 1969–1972, I have observed eleven shell phases of HR 2142. The observations made during 1969–1971 are discussed in a recent paper (Peters, 1972). Only a brief summary of these earlier observations will be presented now.

The shell lines which periodically develop near the centres of the Balmer lines and λ3889 of He I during the shell phase are red-shifted relative to the photospheric features at first and shift toward the centre of them as they strengthen. As an example, consider the Hβ shell line which initially has a radial-velocity shift of 45 km s^{-1} relative to the centre of the emission feature, shows a shift of 30 km s^{-1} at zero phase, (defined as the time of maximum core strength) but a slightly positive shift of 5 km s^{-1} at a phase of $0^{P}.02$. The duration of this sequence is 5–7 d. A variation is observed in the strengths of the shell phases, however, similar shell phases do occur in groups of two to four.

The observations made during the shell phases of 1972 January and April revealed that the shell lines re-appear for a short time after an initial decline in strength. The observations made during 1972, April showed that the shell lines in all observed features except Hβ had disappeared (at 46 Å mm^{-1}) two days after zero phase. Four days later, the shell lines were again present and about 80% of the strength observed at zero phase. These shell lines had completely disappeared two days later. The shell lines which re-appear after zero phase are blue-shifted relative to the photospheric lines. A plate of dispersion 11 Å mm^{-1} on 1972, January 30 revealed a radial velocity shift of -85 km s^{-1} relative to the centre of the emission feature at Hβ.

The two parts of the shell phase are so different in character that I call the first one the primary and the other the secondary shell phase. The duration of the secondary shell phase is about 1–3 d. Previously, these double shell phases were called long-duration shell phases because fragmentary data suggested that the shell lines persisted continuously for 14 d before disappearing.

Spectrograms at 11 Å mm^{-1} obtained at Lick Observatory by D. M. Popper and M. Plavec, covering nine complete cycles, reveal a periodicity in V/R at Hβ with phase. Additional plates acquired since 1971, November confirm the Figure 4 in the paper cited and remove most of the speculative portions of the curve. The strict periodicity of the shell phases and the regular variations in V/R at Hβ suggest that HR 2142 is a binary and that the shell phase appears when the system presents a certain orientation relative to our line of sight. A model currently under consideration by Ronald S. Polidan and myself suggests that material is being transferred to the primary, from a yet undetected secondary of spectral type later than A, in the form

Batten (ed.), Extended Atmospheres and Circumstellar Matter in Spectroscopic Binary Systems, 174–191.

of a thick stream. The shell phase occurs when a portion of the stream passes in front of the primary as we see it (Figure 1). Such a stream would explain the radial-velocity and intensity sequence observed during the primary shell phase. Whereas we look more or less along the stream when the shell lines are weak, we are observing the stream more nearly tangentially just after zero phase (when we view the stream through its thickest portion). Material leaving the secondary star from L_2 could be responsible for the secondary shell phase. Alternatively, this phase could occur when we view along a counter stream. The high negative velocity observed for the secondary shell-phase lines suggests that we are observing a different part of the stream from that seen in the primary shell phase. The weak, permanent core observed in Hβ and Hγ could be formed in an expanding envelope (velocity -30 km s^{-1}) which surrounds the primary star. The rapid rotation of HR 2142 could be a result of angular momentum which has been transferred to the primary as it accreted material from the secondary.

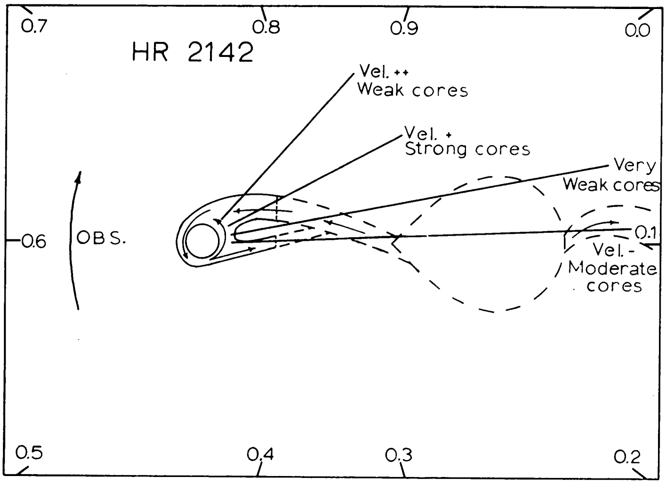

Fig. 1. Model proposed by Peters and Polidan for HR 2142. Figures around the border indicate direction of line of sight at the indicated phases.

Whereas the above qualitative model can explain many of the observed features of the shell sequence, it is difficult to explain the regular V/R variations in Hβ. The short duration of the secondary shell phase suggests that the material responsible for it is rather localized. The orbital inclination of the system must be 70°–80° (an eclipse must just be missed). It is difficult to understand how material from either L_2 or a counter stream could be so localized this far out of the orbital plane.

This system raises the wider question: are some Be stars binaries in the process of mass exchange? Would envelopes around such stars be different in type (density distribution, velocity distribution, turbulence, etc.) from those around single Be stars? What spectral features might distinguish these two possible types?

A number of Be stars do not fit the scheme of emission-line profile versus inclination described in Dr. Boyarchuk's review paper – notably HR 2142 which has a high $V \sin i$, but whose spectrum shows only a very weak permanent core in Hβ and none at all in Hα. Unlike some equator-on Be stars, the spectrum of HR 2142 shows only emission lines of Fe II, and no shell lines of Fe II, Ca II, Ti II, etc. We have observed the Ca II triplet at λ8550 in emission, although few stars show this triplet in emission. We have also observed it in the spectra of π Agr, ϕ Per and υ Sgr. The first two of these also show Fe II in emission. Could the Be stars that display Fe II and the Ca II triplet in emission be binary systems exchanging mass?

Van 't Veer: Do you know the mass ratio of the system?

Peters: No, we can only guess at it.

Van 't Veer: Has the mass transfer only recently been observed – or has it been observed for some years?

Peters: As far as I can tell, the system has been behaving in this manner for the last 50 yrs. McLaughlin, reported by Merrill and Burwell (1949), found that the red component of Hβ was always greater than the blue, and I have seen references to the red shifts of the shell cores, when they are present, but the earlier spectrograms were really not of high enough quality to reveal some of the features that I have found.

Chen: I find this investigation very interesting, and the binary model indeed offers a plausible interpretation of the periodical changes of spectral lines. I still like Struve's suggested model of a single star with circumstellar matter, however. Mr. Tom Morgan, of the University of Florida, has investigated the hydrodynamic flow around a single star (Morgan and Chen, 1972). He found possible periods of variations of the order of days or months. I believe, therefore, that this is an alternative explanation of periodic variations. Are you quite sure of the measurements of radial velocity from the absorption lines in the spectrum of HR 2142?

Peters: I have measured some of our plates for radial velocity and looked for variations. The lines are very diffuse: the star has a high value of $v \sin i$. Only three helium lines have proved suitable for measurement, but they are often asymmetric between the shell phases. The asymmetry could be caused by helium emission from the circumstellar envelope distorting the feature, or it could possibly be absorption lines of a secondary spectrum blending with the primary spectrum.

Smak: How serious could be the contamination of the absorption-line intensities by the variations of the emission-line intensities? Or, to ask a more risky question, is the secondary maximum possibly due to some strange behaviour of the emission component?

Peters: I don't think so.

Plavec: I think that one possible difficulty in this picture of HR 2142 is that the fan of material that is streaming away may be obscured by the fairly big body, so it is difficult to imagine how it can be seen, projected against the body of the only star we observe. In that case, probably what you mention as the 'counter stream' would be more feasible, because, after all, we observe something like it in U Sge, or the ring of β Lyr. We have no explanation for these streams that show violet-shifted lines, but nevertheless they exist. Certainly there is a possibility that we can explain HR 2142 by a single star, but I would like to hear from Dr. Chen exactly how this periodicity arises in a single star.

Chen: Morgan applied Euler's hydrodynamical and continuity equations to the flow around a single star. The main external force is the gravitational attraction of the star. Then, as Dr. Huang indicated, the difficulty is to impose a boundary condition for the flow. He considers the first-order 'perturbation' of Limber's (1964) steady-state solution. The set of resulting time-dependent equations yields periodic solutions.

Hutchings: Has Mrs. Peters estimated $v \sin i$ from the profile?

Peters: Yes, it is 350 km s^{-1}, determined graphically from the He I lines $\lambda\lambda$4026 and 4144.

Oliver: Is there any photoelectric photometry of the system?

Peters: No, but I wish someone would put it on their list.

Cowley: What mass ratio did you assume?

Peters: 0.25.

Cowley: Doesn't this imply a secondary component that should be visible spectroscopically?

Polidan: That value of the mass-ratio makes the secondary something around a middle A-type star. I don't think you could detect it.

Cowley: Not if it is a main-sequence star, rather than an evolved subgiant that could be seen at long wavelengths.

Polidan: If you believe the theoretical calculations, this was originally a fairly massive system, and after mass exchange you should have a B-type star and a middle A-type star.

Underhill: One B0 star with a shell would have a mass about 16 $M_\odot$, so the other one would have a mass of 4 $M_\odot$ – then its spectral type should be B6 and we ought to be able to see the hydrogen lines.

Peters: The spectral type of the proposed secondary is later than B9, so the mass ratio should be lower.

Biermann: If the cooler star is a well developed subgiant, then it has the same luminosity now that it would have had if it had evolved as a single star, because the luminosity of a subgiant is determined not by its total mass, but by its core mass. If this star now has a mass of 4 $M_\odot$, and originally had a mass of 10 $M_\odot$ or 12 $M_\odot$, it is really strange that you don't see it.

Smak: First, I think the mass ratio we are talking about was an assumed one – it could be much different. Secondly, there are many similar systems, with similar mass ratios, in which the secondary spectrum is invisible – so we should study this one in detail.

Fracastoro: Mrs. Peters also wants to read a communication from Dr. Aller, who was prevented by illness from attending this Symposium. The report is on work by S. Heap and Aller.

Peters: The star HD 45166 ($V = 9\overset{\text{m}}{.}88$) may be described best as a 'pseudo-Wolf-Rayet star'. Its spectrum displays intense, very sharp emission lines corresponding to the higher-ionization stages of C, N, O, superposed on a background continuum which shows the Balmer absorption lines. It is not a true Wolf-Rayet star since the emission spetrum changed strongly between 1922 and 1933 (Anger, 1933) the emission lines are very sharp, and include lines of C, O, and N in approximately equal strength. The present investigation includes high-dispersion spectra, energy scans, and filter photometry in the satellite ultra-violet. In brief, HD 45166 appears to be a binary, consisting of a late B-type star and a hot companion responsible for the emission line spectrum; the hot companion differs from true WR stars in having a normal helium abundance and, presumably, a low mass, comparable with that of the Sun.

The width and total intensity of the Pickering lines indicate strong blending with hydrogen. The widths of the lines of C, N, O, etc. indicate velocities of only about 110 km s^{-1} a value characteristic of Of stars rather than WR stars, although the character of the emission spectrum (e.g. strong O lines in emission) differs markedly from that of classical Wolf-Rayet stars. There is no evidence for the Bowen fluorescent mechanism. The absorption spectrum corresponds to a late B, thus setting the system far apart from a normal Wolf-Rayet system; the late B component is either a slow rotator or we are looking at the system pole-on. The emission-line spectrum appears to be about the same as it was when Neubauer and Aller (1948) examined it in 1943–44, but short-term variations are likely. By combining photoelectric energy scans covering the region $\lambda\lambda$3250–7780; (kindly made in 1971 by R. Stone with the Wampler scanner at the Crossley) with ultraviolet photometry from the OAO-2 (using Code's preliminary calibration factors as modified by Code to account for degradation of the filters), the total energy distribution is obtained. Space absorption is estimated by indirect arguments, from data secured by various observers for nearby stars and clusters in this region, as well as the strength of the λ4430 band; the adopted value is $A_v = 0\overset{m}{.}2$.

That HD 45166 is a binary composed of a late B-type star and a hot companion is indicated by the following: (1) superposition of high-excitation emission line spectrum and low excitation absorption line spectrum, (2) relative velocity between emission and absorption lines, (3) veiling of Balmer discontinuity in a late B-type star by the hot companion, and (4) high ultra-violet flux from the hot companion. The companion appears to be responsible for the emission-line spectrum and is referred to as the quasi-Wolf-Rayet star. If it is a close binary involved in mass-exchange, the qWR star has not yet lost its hydrogen-rich envelope. Continuing mass exchange is compatible with observation of long term variations in the emission spectrum.

From the profiles of the Balmer lines it is concluded that at λ4340, the qWR and B8V stars are of about equal luminosity, hence $M_v(\mathrm{qWR}) \sim 0\overset{m}{.}0$. From a distance estimate of the system of 700 parsecs, $M_v(\mathrm{qWR}) \sim 1\overset{m}{.}2$. In any event the quasi-WR component is much fainter than a classical WR star. In this respect HD 45166 seems comparable to the central star of a planetary nebula, e.g. nucleus of NGC 1514 or that of NGC 6543, but HD 45166 is not the nucleus of a planetary nebula.

Paczyński (1967) has suggested that the occurrence of WR stars is a consequence of mass transfer in close binary systems, the process of mass-loss stopping here, evidently, just short of a complete loss of the hydrogen-rich envelope. The remnant has instabilities which produce a WR-type spectrum. The binary system composing HD 45166 does not belong to the same position of the HR diagram as true WR stars. The absorption line component has a later spectral type and lower luminosity than the companions to true WR stars which are usually O and early B giants. The emission-line component is also less luminous than true WR stars, and hence less massive. HD 45166 is an example showing that the atmospheric conditions producing highly excited C, N, O emission lines may arise during the evolution of low-mass stars as well as high mass stars and in stars of normal abundance as well as those of high helium abundance.

Underhill: I've talked a lot about this work with Sally Heap. Although Aller uses the term 'quasi-Wolf-Rayet', I think she feels very strongly that the emission-line object is very similar to the central stars in planetary nebulae – which have sometimes been called 'Wolf-Rayet' although the emission lines in their spectra are much narrower than those in the spectra of Walf-Rayet stars, and the composition of their spectra is quite distinctive. I would suggest that you forget the word 'Wolf-Rayet' in considering HD 45166: it's a hot subdwarf of absolute magnitude around $1\overset{m}{.}0$ or $0\overset{m}{.}0$, combined with a late B-type star.

Fracastoro: We come now to a discussion of period changes. First, Van 't Veer.

Van 't Veer: This is not a prepared paper but merely a comment on the first two days when we heard so many things on gas streams which can last for at least ten or even fifty years, as we have seen this morning. It is important to ask two questions:

(1) What is the influence of this mass transport on the orbital period of the system?

(2) Do we measure period changes which are in agreement with our knowledge on gas streams?

The first point can be roughly treated in the following qualitative way. The matter is flowing from one component to the other so the total mass of the system is conserved. Using Kepler's third law we have $\omega^2 r^3 = \text{const}$, r being the distance between the two components and ω the angular velocity. On the other hand, from the constancy of angular momentum we can deduce that ωr^2 must decrease if the angular momentum of the outflowing matter is increasing. This is the case for a semi-detached system of mass ratio $r \geqslant 1$ with matter outflowing from L_1. The result must be a decreasing period and a decreasing orbital radius with shrinking Roche lobes which can perhaps maintain the mass transport for a reasonable time. If the star filling its Roche lobe has a much lower mass than the primary component, we will obtain the opposite result. So the period changes can give us an important information on some details of the mass transport. Unfortunately the period measured between two successive primary minima not always coincides with the real one, defined as the time elapsed between the corresponding conjunctions. Asymmetries of the light curve during minima are often the cause of erratic fluctuations which may mask the real period changes. I am afraid that we do not possess sufficient observations of semi-detached systems to permit clear conclusions; but I should be glad to hear your opinion on this point. I should like to end this short comment by asking you still two other questions:

(1) What do we know about the mass ratios of the systems with gas streams?

(2) What is the mass of the outflowing matter?

Smak: I have four comments. First, if one assumes that the orbital momentum is conserved, then the absolute dimensions of the Roche lobe around the secondary reach a minimum at a mass-ratio of about 0.8. Secondly, the main reason for the mass outflow is not the shrinking of the Roche lobe, but the expansion of the star, either on the thermal, or nuclear time-scale. My third comment is that the simple picture you have presented is modified, though only slightly, by including the rotational momenta of the components. Finally, the major trouble with the simple star-to-star mass-transfer interpretation of the period changes is that it gives rather short time-scales. The best example is provided by the W UMa systems, which show rather fast period variations, which would imply time-scales of the order of 10^6 yrs for these systems. This is definitely too short, as compared with the large number of these systems observed and their very probable connection with the main-sequence phase of evolution.

Van 't Veer: It is true that I was ignoring the rotational momentum of the individual stars. I think you will agree that this is not more than two or three per cent of the orbital angular momentum of the system. Secondly, on the changes in the Roche lobe, it would be very difficult to maintain a stream if the lobe were expanding.

Plavec: When people working in different fields meet in a conference, it must occur inevitably that discussion turns towards areas which have been discussed in the field for years. Dr. Van 't Veer has my admiration because he managed in a short time to present several problems which have been discussed for years and for example have been described by Kruszewski (1966). So let me concentrate on the problem of how the smaller star can maintain the outward stream not only for 50 y but probably for a much longer period of time. The actual reason is that the star is developing on a nuclear time scale, and because, in this case, it is a subgiant, while it is still burning hydrogen in its core. It has a natural tendency towards expansion but because there is the Roche lobe around it, it must happen that some material is being pushed across the Roche limit just because of internal forces. As a consequence, mass loss can go on for a very long period of time, defined more or less by the main-sequence evolutionary time for a star of one of three solar masses.

Batten: To answer one of Dr. Van 't Veer's questions directly, period changes of the kind you described are known. The prime example is β Lyr. I think most people would agree that the period change in that system is due to mass transfer. I think the case is almost as strong for U Cep, but of course in other systems there are many other factors that we do not understand. The picture of observed period changes is very complicated, and obviously other factors are at work. Dr. Smak recently proposed a model which might account for this although in my review paper, I did express some reservations about it.

Smak: I hope that will be recorded!

Batten: I might add that the increase in period of β Lyr has now been observed for about 200 y, and that of U Cep for nearly 100 yrs. Of course, most systems known to show period changes have fluctuating periods.

Huang: Since Dr. Batten mentioned β Lyr, I would like to add that one of the reasons why I proposed that the secondary component of β Lyr is the more massive component is the period increase, that can be explained as due to the ejection of mass as Dr. Van 't Veer has just described.

Fracastoro: Sometimes we find spurious changes of period due to third-body light-time effects or to apsidal rotation – so the problem is rather complicated. Periods increase and decrease not only in contact binaries, and in dramatic pairs like β Lyr, but in very common semi-detached systems. In order to fit the time scale, one could perhaps think of sporadic phenomena, like prominences in the Sun – not a continuous outflow of matter.

Van 't Veer: So you conclude that no binaries have constant periods?

Fracastoro: Oh, no: that is not my conclusion – although maybe sometimes there are period changes below our ability to detect them.

Oliver: You mentioned contact and semi-detached systems: it is now becoming clear that even detached systems show period changes. Dr. Van 't Veer mentioned that changing asymmetries in the primary of secondary minimum of the light curve can cause apparent period changes. In many systems, this does happen, and it causes confusion. There are many complications that completely obscure the period changes caused by mass transfer. It's a 'fun' problem.

Fracastoro: Now Dr. Herczeg has a contribution, and we will postpone discussion of that until we have also heard from Dr. Hall.

Herczeg: I am going to make two somewhat longer comments. First, a summary of the basic facts that observers have established concerning period changes of eclipsing binaries, then an attempt to interpret the period of β Lyr.

1. Period changes in general

In several cases timings of minima go back 100 yrs or more, although visual estimates (unlike visual measurements by expert observers like Wendell, Dugan, Danjon, Detre and others) are far too inaccurate to reveal more than the crudest features of the (O – C) diagrams. It is only since the mid-1940's, the advent of extended photoelectric studies by the aid of the multiplier phototube, that we can detect rather minute details of the period changes. For the benefit of the theoreticians working on this field I should like to give a set of short statements, not to say axioms, summarizing the experience of decades of observations.

(a) The first of these statements sounds rather trivial. Most eclipsing binaries do show period changes. This applies specifically to so-called typical Algol configurations and W UMa-variables where constant periods are exceptional. We find more constant periods among detached systems although even here we know some surprising cases showing large period changes (e.g. TX Her).

(b) Part of the observed period changes may well be spurious caused by small, random or regular, deformations of the light-curve near minimum. While such effects certainly exist, their influence seems to be relatively small, of the order of, say, $0\overset{\mathrm{d}}{.}01$ as judged from studies like that of Van Woerden (1957) on SV Cam or Kwee (1958) on

VW Cep. Major shifts of the minima up to $0\overset{d}{.}1 - 0\overset{d}{.}2$ may in most cases correspond to real changes of the orbital elements.

(c) However, one possibly important reservation should be mentioned here. Small, random shifts of the epoch of minimum, occurring from cycle to cycle and remaining necessarily undetectable, may sum up (this is a problem of one-dimensional random walk) to produce the observed time residuals. I hardly doubt that in a few cases this type of spurious period changes determines the character of the (O – C) diagram.

(d) We know perhaps a dozen well-defined cases of apsidal motion, possibly 2 or 3 cases of light-time effect and a few binaries with linearly increasing or decreasing period, but the overwhelming majority of the (O – C) diagrams remains quite irregular. With the higher accuracy of the timings, a very frequent, almost typical pattern emerges. Roughly half of the better studied cases show virtually constant or nearly constant periods for years, even decades, then a more or less sudden change occurs resulting in a new period that again remains constant for years or decades. This type of variation makes it improbable that the above mentioned random-walk mechanism would be at work.

(e) These sudden period changes mean in many cases a smooth bending over of the (O – C) curve, extending the transition to the new period to several years. Often, however, the transition is rather fast and we can localize it to a few months or even weeks. In these cases we may speak of veritable discontinuities or jumps of the binary period, accompanied sometimes by observable spectral or light-curve changes, like changes of the spectroscopic behaviour of RCMa about 1914 (Wood, 1957) or the flaring of W UMa observed by Kuhi (1964). This system of VW Cep showed such a discontinuous period change just during the 1959 international campaign.

The $\Delta P/P$ ratio is practically always of the order of 10^{-5}. The sudden changes seem to occur at irregular intervals, with a mean time lapse between them of perhaps 20–25 yrs. Neither the positive nor the negative changes appear to be clearly preponderant; indeed, there is even tendency to exhibit alternating sign sequels: $+ - + -$ etc. Any long term, secular change of the period can be, of course, very effectively masked by this type of irregular, sudden period change.

2. The Period of β Lyr

Although period changes of β Lyr were indicated very early after Goodricke's discovery of the variability, and the non-linearity of the period increase was already extensively discussed by Argelander, all efforts to describe the variation of the period failed hitherto. Usually polynomial expressions were suggested, containing terms up to E^5 in the formulae for the (O – C) values of the timing of minima, but predictions invariably broke down after only 10–12 yrs. The period has been found to be steadily increasing for almost 200 yrs, while the rate of increase has been steadily decreasing all the time. The situation is the more intriguing as the $(O - C)$ curve and the period changes seem to be quite smooth, without conspicuous irregularities. Since polynomials in E failed to represent the variations of the period, I tried to describe them – and this first sounds almost like a scientific joke – by an infinite series. I found a surprisingly good representation by a simple exponential expression of the form

$$P = P_0 (1 - \alpha e^{-\beta x}).$$

This also seems to make more sense from the point of view of physical interpretation than the formal polynomial representations, since a formula like this suggests a slowly but steadily decreasing stellar activity, so to speak, probably a diminishing rate of mass loss from the whole system. The (O – C) curves, following from this representation of the period changes, should correspond to an $ax + c^{-bx}$ type curve which is, indeed, quite manifestly the case. On the other hand, a detailed discussion of the (O – C) curve is made rather difficult, not so much by the irregular nature of the curve itself, as by large and irregular shifts of the epochs of minimum due to occasional strong distortions of the light curve. A comprehensive discussion of the period of β Lyr has been finished recently and I am going to submit the final manuscript for publication within a couple of months.

Hall: I want to propose a model to explain the large cyclical period changes which are observed in virtually all of the semi-detached Algol-like eclipsing binaries. Before beginning I must, in order to lay the proper foundation, make clear a few points.

I am restricting my attention to the semi-detached Algol-like binaries, which are now understood to be remnants of post-main-sequence mass exchange. I am excluding contact, and I am excluding the RS CVn binaries, most of which are definitely detached and are probably in some sort of pre-main-sequence phase of evolution (Hall, 1972). Both the W UMa binaries and the RS CVn binaries also experience period changes of similar size to those in the Algol-like binaries, but probably for a different reason.

Theoretically, we would expect a monotonic (if not uniform) period increase in Algol-like binaries as a consequence of mass transfer from the less massive cooler subgiant to the more massive hotter star.

In contrast, what we see observationally are very large ($\Delta P/P \sim 10^{-5}$) period increases and decreases. There is a tendency for the changes to be comparable in size and to alternate in sign, this often giving the impression of a sinusoidal variation in the (O – C) diagram (a plot versus time of the observed time of primary minimum minus the time computed with some assumed constant period). However, it is quite clear now that very few, if any, of the (O – C) curves are really sinusoidal or even strictly periodic. Earlier attempts to explain the cyclical period changes as a result of apsidal motion or of orbital motion around a third body must, of course, be on the wrong track if the variations are not really periodic. The characteristic time between alternate period changes is between 10 yrs and 100 yrs for different binaries, so 30 yrs is a good number to remember in the discussion which follows. One or two binaries do show the predicted slow monotonic period increase, U Cep being the best example (Batten and Plavec, 1972). But even in U Cep, large alternating variations in period are superimposed on this steady increase. In all of the other binaries, presumably the teady increase is occurring but is completely masked by the larger alternating variations. A few people, Dr. Herczeg for example, have emphasized that there is a marked tendency for the period changes to be abrupt rather than gradual, with the period constant in between these changes. This is a feature which my model should be capable of explaining.

In order to avoid confusion, we must note that, since the very small slow period increase can be neglected, there is an average orbital period P and major semi-axis a which describes each system during any given century or so. Likewise there must be an average amount of mass transfer from one star to the other and (if some of the angular momentum carried over by the mass goes into rotational angular momentum) also an average fraction describing how much goes into rotation. Therefore, we can simply look at fluctuations in these various quantities about their average values.

The fundamental basis of my model is an exchange back and forth between orbital angular momentum, J_{orb}, and the angular momentum stored in the disk around the hot star J_{rot}. Such an exchange was suggested for another reason by Paczyński (1971), and invoked later by Smak (1972), as the only reasonable mechanism to explain the alternate period changes observed in the U Gem binaries. With this exchange as a basis, I want to show that a few relatively elementary considerations make it very natural, in fact unavoidable, that the period should alternate cyclically between values larger than the average and values smaller. A complete explanation of this phenomenon must be extremely complicated, but I think now is the time to take the first step in the right direction. My hope is that this model, although incomplete and unfortunately qualitative in many places, is correct as far as it goes and is, in fact, a step in the right direction.

As Smak has shown must be the case with the U Gem binaries and as my calculations show must also be the case in the Algol-like binaries, about $10^{-5}\ M_{\odot}$ of mass in the rotating disk must be involved in the exchange. This is too much mass to reside exclusively in the optically thin outer parts of the disk and in that sense must be part of the 'interior' of the star, even though its rapid rotation makes it part of the 'disk'. Even more important to note is that $10^{-5}\ M_{\odot}$ is too much mass (by at least one or two orders of magnitude) to be accounted for by the amount of mass which comes over in the stream in the course of 30 yrs. Therefore, the stream must act only as the 'trigger' which controls the exchange between J_{rot} and J_{orb}.

In Figure 2 is a schematic representation of an (O−C) diagram, not meant to be strictly sinusoidal or periodic. At point A the period P is maximum and, by Kepler's law, a is maximum also. Consequently J_{orb} is maximum. Since there must be conservation of total angular momentum in the system, $J\ (\text{total}) = J_{\mathrm{orb}} + J_{\mathrm{rot}}$, it follows that J_{rot} must be minimum. When a is maximum, the Roche lobe around the mass-losing subgiant must have its maximum size. And since it is generally accepted that the rate of mass loss is proportional to the degree to which the contact star overflows its Roche lobe, the rate of mass loss should be minimum. At point B the opposite of each of these statements can be made.

Let us start at point A, where the stream has its minimum strength. If ever the stream increases slightly, it will begin to encourage the disk to rotate more rapidly,

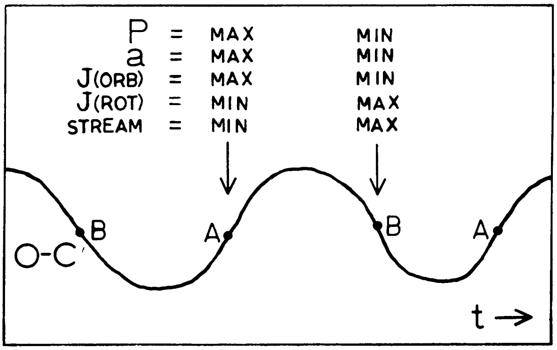

Fig. 2. Schematic (O—C) diagram to illustrate Hall's mechanism for period changes (see text).

probably in order to have the disk velocity match the stream velocity at the point of impact. The disk thus begins to increase its J_{rot}, drawing not so much from the angular momentum in the stream itself but mostly from the J_{orb}. In order for tidal interaction to allow the exchange to take place on such a short time scale, it is clear there must be turbulent viscosity in the disk.

The crucial thing is that, once J_{rot} begins to increase and J_{orb} consequently begins to decrease, the separation a begins decreasing and the stream increases further. Thus the process must be self-accelerating and the period must continue to decrease for some time.

In order for this self-accelerating process not to continue forever, it is necessary either that (1) there exist some maximum amount of J_{rot} which the disk can contain or that (2) there exist some maximum rate of mass loss that the contact subgiant can sustain for any appreciable length of time. The existence of both of these maxima seems reasonable to me (although I cannot prove it) but one of the maxima is probably the critical factor in this situation. It is convenient for my preliminary presentation of this model that the existence of either maximum would be sufficient to prevent the self-accelerating mechanism from continuing forever.

This brings us to point *B*, where the stream has its maximum strength. If now the stream decreases slightly, the disk will be encouraged to begin slowing down. As the disk then transfers J_{rot} back to J_{orb}, the separation a must begin increasing again and the Roche lobe around the mass-losing star must expand. With the stream decreasing further, we now have a self-decelerating mechanism and the period must continue to increase for some time. It is easier to explain why the self-deceleration does not continue forever. The simplest explanation is that the Roche lobe expands sufficiently to shut off the stream entirely; in other words, the minimum rate of mass transfer is zero. It is, however, also possible that the critical factor in this situation is the existence of a minimum J_{rot}, maybe zero. But again it is convenient for my model that either of these minima would be sufficient to stop the self-decelerating mechanism. We are now back at point *A*, and the cycle can begin again.

My model could rather easily account for Herczeg's abrupt period changes. There is no obvious reason why the binary might not remain at point *A* for some time, especially if the stream then is turned off completely. Similarly, it is possible that the binary might remain at point *B* with nearly the maximum amount of mass flow in the stream for a length of time appreciably longer than the time scale involved in the actual self-acceleration or self-deceleration part of the cycle. The appearance of the (O – C) diagram then would be straight line segments at point *A* and nearly straight line segments at point *B*, with the curvature between not always apparent in a typical observed (O – C) diagram.

I have been examining light curves of totally eclipsing Algol-like binaries in an attempt to discover some correlation between asymmetries around totality with the (O – C) diagram. Restricting my attention to light curves obtained in the visual, I tried to decide whether the bottom of totality was symmetrical (either slightly rounded, as in the case in VW Cyg and AQ Peg, or exactly flat) or asymmetrical (either slanted up as in the case of SW Cyg or slanted down as in the case of Walter's light curve of RV Oph). It is premature for me to be certain whether or not a correlation exists, but there is some indication that asymmetries always occur when the (O – C) diagram is between *A* and *B* (when the disk is being speeded up by the steadily increasing stream) and that the totalities are symmetrical between *B* and *A* (when the disk is slowing down and the stream is decreasing). Note that the asymmetries do not occur when the stream itself is a maximum, i.e., on either side of *B* itself. Thus it is probably more relevant to think of the asymmetries in terms of an asymmetry in the distribution of mass in the disk itself, and not in terms of the stream. I did not consider old photographic light curves, whose spectral response often reached into the ultraviolet, because if my experience with SW Cyg is any indication, the *U* light curve can reveal the influence of the gas stream or the hot spot on one side of the hotter star, while at the same time the *V* light curve can be revealing an optically thick accumulation of matter on the other side of the star.

Smak: I have four comments again. First, what Dr. Hall described as slow variations of the period which are hidden in larger, faster, and more irregular ones, corresponds to the systematic effects due to mass-transfer on a nuclear time-scale. It may be worthwhile to mention, however, that there are few cases where we do observe rather fast period variations being apparently due to the rapid mass-transfer on a thermal time-scale. Three examples I wish to mention are: β Lyr, V 367 Cyg (suggested by Plavec in this context), and SV Cen (suggested by Paczyński). As a second comment, I want to suggest that it could be good to check whether the expected variations in the size of the Roche lobe around the secondary are sufficiently large to modify the rate of the mass outflow. My third comment refers to the possibility of a temporary storing of the momentum in the disk, as was first suggested by Paczyński (1967b) in his discussion of WZ Sge. It is quite obvious that in this case, we should try to learn more about the structure of such massive disks, instabilities which are likely to occur in them, and their time scales. Only good models of disks will eventually permit us to discuss this mechanism in a more meaningful way. There has been only one serious attempt in this direction, reported briefly in a paper by Prendergast and Burbidge (1968) and this brings me to my fourth comment. Numerical results by Prendergast and Burbidge imply that due to the transfer of momentum across the disk, there can be a mass outflow from the system. This might be a very powerful mechanism for period changes, although without detailed calculations we cannot make any numerical predictions.

Finally, to clarify one of my earlier remarks, I do realize that the W UMa systems also show alternating period changes. My point is only to indicate that if any such variations were interpreted in terms of secular evolution, that would result in time-scales that were definitely too short.

R. E. Wilson: I would like to ask Dr. Herczeg: what is the multiplier of t in his equation? I am interested in using his formula.

Herczeg: I have only preliminary values, and wanted only to indicate the basic character of the relation. When I have a definitive value, I will send it to you.

Milone: What about these binaries that show a migrating asymmetry in the light curve? As this travels through the minimum, surely it can distort the minimum and thus affect the (O – C) values?

Hall: I think that binaries of the RS CVn type and Algol-like binaries should be considered separately. Migrating asymmetries are found in the light curves of the former type. You are correct in principle – and the RS CVn binaries do show large variations in (O – C). I believe, these are the result of true changes in the orbital period, for two reasons. First, since the origin of the distortion is an uneven distribution of surface brightness on the cooler star, the distortion should only seriously affect the secondary minimum. Second, I think that the observed variations in (O – C) are too large to be explained by distortion of the light curve during eclipse. Maybe this should be checked quantitatively.

Bath: When you have these alternating period changes, is there any difference between the rate at which the period increases, and that at which it decreases?

Hall: I have not looked at this very carefully, but I think there may be a tendency for increases to be larger than decreases. The (O – C) plot looks a little like a Cepheid light curve – it rises more steeply than it descends.

Catalano: Sometimes we find that the (O – C) residuals can be satisfied by two constant periods. If we refer the residuals to some period between these two values, we obtain a nearly sinusoidal pattern, as Dr. Hall showed us.

Hall: Yes, sometimes an abrupt period change can be made to look more like a sinusoidal change by a different choice of mean period.

Herczeg: One interesting object is VW Cep. Kwee has shown that there is a disturbance moving through the light curve in about every two years – and the (O – C) values show just this period. But the amplitude of the disturbance is much smaller than that of the (O – C) curve, so the disturbance certainly cannot be responsible for the whole observed variation. There are only a few cases, however, for which we have direct observations of disturbances in the light curve affecting the times of minima. I agree with Dr. Smak that there may be secular changes of the period that we could easily detect in the absence of complications in the (O – C) diagram. The observed irregularities in the period, unfortunately, make it very difficult to prove secular trends within a reasonable time – say a few decades.

Van 't Veer: I basically agree with what Dr. Herczeg said about the W UMa systems. I looked at seven or eight of the best-observed systems myself. (The others are not well enough observed for us to be able to say anything about their period changes.) In these well observed systems, I have the impression that the period changes behave in two distinct ways. Either you have a continuously decreasing period, suggesting a continuous ejection, or a discontinuously increasing one, suggesting spasmodic ejection. Increasing periods are related to increasing orbital radii and Roche lobes – and conversely – so perhaps we can find a physical relation from these rather vaguely established observational effects.

Devinney: In the (O – C) plot for a given system, it often happens that the upward branches are all parallel, and the downward branches are also parallel to each other. I think one of the Polish astronomers has also noted this. This suggests that some binary systems oscillate between two periods, as Dr. Catalano just said. It is very difficult, however, to interpret this physically, unless there are two modes of mass exchange.

Smak: My impression is that those (O – C) diagrams show such a variety of forms that it is difficult to describe them well, to draw any obvious conclusions, or to say whether that slope is larger than the other slope. Neither do they really follow sinusoidal curves.

Walter: I think it is very difficult, or probably impossible to obtain a clear insight into the nature of period changes unless we understand the structure of the gas streams. These can work in different ways that may have quite different effects on the period. I have the impression that abrupt period changes may be correlated with changes in the structure of a system. I have spoken about the structure of TW And: the period of this system is now fairly constant. I think we should study the structure of systems, and compare it with the behaviour of their periods. I hope that we may learn about this complicated matter by such combined investigations and observations.

Huang: I agree with Dr. Walter. In order to understand the period change, we must first understand the flow pattern – which is not solved. I have discussed period changes (Huang, 1956) but as I grow older, I feel more inadequate to study this problem. In my review, I didn't even touch on this problem of the period change, because it is too involved.

Underhill: The intuitive interpretation of period changes outlined by Hall is very interesting. One of the key components is this disk of gas which is supposed to be changing in characteristics and thereby changing the orbital momentum and the period. I think spectroscopic observations of selected stars would be very useful for determining the physical properties of these disks.

Batten: This is precisely what I'm trying to do, but the observations are not easy and will take time.

Hall: I think Dr. Underhill's suggestion is very good. One fact may complicate the interpretation, however. If the disk contains $10^{-5}\ M_{\odot}$, most of it will lie below the photosphere. It might happen that the outer photospheric layers – which are all you see – will not reflect the average angular momentum of the whole disk.

Underhill: What do you mean "the disk lies below the photosphere"? Is it inside the star?

Hall: Yes. By 'disk' I mean that part of the star and envelope which is rotating rapidly. In my model, I need $10^{-5}\ M_{\odot}$ to be involved in the rotation. You cannot see down through that amount of matter.

Biermann: Dr. Hall's theory is very interesting. I've tried to estimate the time-scale of diffusion of angular momentum across disks. You have to assume a number of parameters, such as velocities, that you can't really check, but if you guess reasonable values of them, Dr. Hall's idea certainly doesn't seem unreasonable.

Smak: Dr. Hall, when the disk in RW Per changed, was there any strange period change?

Hall: I'm not sure that I really believe my own interpretation now of an expanding star or disk in RW Per. I have a complete *U B V* light curve and I am re-examining the older data more carefully, but it is too soon for me to tell you what the proper interpretation is. I looked at RW Per, and a dozen or more Algol-like eclipsing systems, from the point of view of my model for period changes. It seems to fit in with the others.

Plavec: In Algol-systems of short period, the disk itself may not be so terribly important because most of the material that flows from one star probably falls directly on the other one. This of course does not exclude the interpretation suggested by Hall, only you would have to consider the mass receiving star sometimes rather than the disk itself. Otherwise the picture is very attractive and one could think of the changing size of the Roche lobe and changing size of the star, although, as Dr. Smak remarked, it will be necessary to see whether these changes are large enough. Before we are willing to accept the general idea here, I would like very much to hear somebody speak about the period changes observed in Cepheids and RR Lyr stars because about ten years ago, the diagrams produced by Dr. Detre and his group for cepheids and RR Lyr stars, which are single objects, showed very much the same pattern as our picture, and at that time, the suggestion seemed very reasonable to us that this can be represented by the theory of random walk. The problem is whether we have the same situation here or not. Maybe Dr. Herczeg would know more.

Herczeg: I am not sure that the similarity of the period changes is so far-reaching A great many RR Lyr stars show periodic (O – C) curves, many of which indicate very clearly the existence of several periodicities, whereas eclipsing binaries display irregular changes. I would be cautious in drawing analogies.

Goldberg: I would like to consider further this similarity between the period variation in certain single stars like the RR Lyr variables and Cepheids, and those in certain binary systems like the Algols, by discussing the period variations in the single β Cep star BW Vul and by pointing out some possible implications for binary systems.

The period of BW Vul shows the same type of oscillation about a mean period as Hall has described for the Algol-type systems. These variations have been discussed recently by Percy (1971). It is obvious that mass streaming plays no role here since the star is not a binary. There is some evidence to suggest that the oscillations are correlated with variations in the star's pulsational velocity amplitude – a significant result, as a real physical basis for the period oscillations is indicated.

If one assumes that the similarity between the period oscilations displayed by both single stars and binary systems is more than coincidence, the possibility arises that at least one of the components of certain binary stars (in particular the component undergoing mass loss) may be intrinsically variable; and the variability of this component may be the fundamental cause of the oscillation in period of the system. This being true, the role played by the actual stream or the disk surrounding the accreting component in causing period oscillations would not be particularly significant. It would seem worthwhile to investigate some of the questions that I have just discussed by making a thorough comparison of single stars and binary systems that display similar period changes and by making an effort to investigate the intrinsic variability of the components of these binary systems.

Bolton: I was not aware that BW Vul is definitely a single star. It is one of the B-type flare stars that I spoke of when we were discussing Algol. Eggen observed one flare photoelectrically: it was at least $0\overset{m}{.}8$.

Goldberg: I understood that observation was somewhat suspect. There is no evidence at all to suggest that BW Vul is a binary.

Bolton: This has nothing to do with mass transfer, but it does relate to period changes in close binaries. The ellipsoidal variable UU Psc has appeared in the literature several times in recent years with question marks by it. A couple of observers have suggested that it was showing period changes related to apsidal motion. I have recently been reworking the published spectroscopic data and obtaining more of my own. The orbital eccentricity is small, but I believe that the data strongly indicate apsidal motion with a period less than three years. This would make it the shortest apsidal motion period known by about a factor of ten. The stars in the system are about spectral type F0, so that this system would be the latest type system for which apsidal motion would be determined. Because of the short apsidal motion period, it is difficult to obtain the necessary spectroscopic observations. Therefore, I urge the photometrists to time the minima.

References

Anger, C. J.: 1933, *Harvard Bull.* No. 891, 8.
Batten, A. H. and Plavec, M.: 1971, *Sky Telesc.* **42**, 147, 213.
Hall, D. S.: 1972, *Publ. Astron. Soc. Pacific* **84**, 323.
Huang, S.-S.: 1956, *Astron. J.* **61**, 49.
Kruszewski, A.: 1966, *Adv. Astron. Astrophys.* **4**, 233.
Kuhi, L.: 1964, *Publ. Astron. Soc. Pacific* **76**, 430.
Kwee, K. K.: 1958, *Bull. Astron. Inst. Neth.* **14**, 131.
Limber, D. N.: 1964, *Astrophys. J.* **140**, 1391.

Neubauer, F. G. and Aller, L. H.: 1948, *Astrophys. J.* **107**, 281.
Merrill, P. W. and Burwell, C. G.: 1949, *Astrophys. J.* **110**, 387.
Morgan, T. H. and Chen, K.-Y.: 1972, *Bull. Am. Astron. Soc.* **4**, 330.
Paczyński, B.: 1967a, in J. Dommanqet (ed.), 'On the Evolution of Double Stars', *Comm. Obs. Roy. Belgique, Ser. B*, No. 17, p. 111.
Paczyński, B.: 1967b, *Acta Astron.* **17**, 287.
Paczyński, B.: 1971, *Ann. Rev. Astron. Astrophys.* **9**, 183.
Percy, J. R.: 1972, *J. Roy. Astron. Soc. Can.* **65**, 297.
Peters, G. J.: 1972, *Publ. Astron. Soc. Pacific* **84**, 334.
Prendergast, K. H. and Burbidge, G. R.: 1968, *Astrophys. J. Letters* **151**, L83.
Smak, J.: 1972, *Acta Astron.* **22**, 1.
Van Woerden, H.: 1957, *Ann. Sterrew. Leiden* **21**, 1.
Wood, F. B.: 1957, in G. H. Herbig (ed.), 'Non-Stable Stars', *IAU Symp.* **3**, Cambridge Univ. Press, p. 144.