F. JOINT DISCUSSION OF COMMISSIONS 29, 35 AND 42 CLOSE BINARIES AND STELLAR EVOLUTION

(Wednesday, August 30, 1967)

Organizing Committee: F. B. Wood (Chairman), R. Kippenhahn, M. Plavec, J. Sahade
Meeting Chairman: F. B. Wood
Secretaries: J. Grygar, S. Kříž
Editors: M. Plavec and J. Grygar

Contents:

https://doi.org/10.1017/S1539299600001532 Published online by Cambridge University Press

OPENING REMARKS

FRANK B. WOOD
(Flower and Cook Observatory, Philadelphia, Pa., U.S.A.)

I should like to open on a personal note recalling the developments that led to my own interest in this topic beginning many years ago. I do this in part because it illustrates how interest in one particular problem sometimes leads one into another quite different one. Actually, there were two apparently unrelated problems which led to my interest in evolution of close binaries.

The first interest arose from a paper published by Raymond Smith Dugan and Frances Wright some 30 years ago (Princeton Contribution 15). This was an investigation of the changes of period of a number of eclipsing binary stars. At about this time we had become aware that rotation of the line of apsides in systems with elliptical orbits depended in part on the internal constitution of the stars and that the determination of the period of this, together with other information derived from the light and velocity curves, could permit calculation of the degree of central condensation of the stars. I suspect that this may have been at least in part the motivation for the work, because in the discussion of almost every system, mention is made of whether or not the changes could have been caused by apsidal rotation.

What Dugan and Wright actually found was that in many cases there were erratic and non-periodic period changes which could not be explained by any known cause. At any rate, this paper made a profound impression upon me, and for years afterwards I speculated upon what physical mechanism might be responsible for sudden period changes.

The second development came in 1946 when I was finding some difficulty in interpreting the system R Canis Majoris – if anyone wants a system hard to interpret I recommend this one. Since only one spectrum could be observed, determination of absolute dimensions presented some difficulty. In an effort to find limiting solutions, it suddenly occurred to me that laws of celestial mechanics applied here also and that an upper limit to the sizes of the components could be set by using the particular solution of the three-body problem in which one component had infinitesimal mass. Fortunately, the difficult mathematics had been worked out long previously and were clearly presented by Moulton in his text on celestial mechanics. Thus, into eclipsing star studies could be introduced the zero-velocity surface which has now become familiar and which I am sure will be referred to in this discussion.

Actually, the first use of this concept in eclipsing stars had been made by Kuiper in 1941 in his study of β Lyrae. In defense of being unaware of a paper in my own field

Perek (ed.), Highlights of Astronomy, 394–395.

published in a professional journal, I can only say that the paper appeared late in 1941. Earlier that year I had gone on active duty with the U.S. Navy and saw no astronomical literature for nearly 5 years. In preparing for publication my dissertation written 5 years earlier, I had reviewed carefully all the stars included, but β Lyrae was not one of these.

These two interests came together in the late 1940's when, still wondering about irregular period variations, I found that, with one exception, and that itself an unusual system, all systems showing such fluctuations had one of the components near the zero-velocity limiting surface and that in systems which for extended intervals had shown no period variation, neither component approached the limiting surface. This led to the suggestion published in 1950 in the *Astrophysical Journal* that we might use this criterium to divide eclipsing systems into two general classes – one in which neither component approached the limiting surface and a second in which at least one component reached it. A few years later a further refinement was suggested by Z. Kopal, who suggested that the second of these be further subdivided and who introduced the terms 'detached' and 'semi-detached' – the term 'contact binary' had, I believe, been used earlier. In the same paper, I suggested mass ejection as the cause of period changes, and noted that, if correct, the period of mass loss would be short compared to the total life of the star.

Time does not permit even a summary of all of the work done in recent years on evolution of close double stars. Su-Shu Huang, one of our speakers today, was the first to compute in some detail the effect of mass loss upon the period, and Crawford first realized what paths of stellar evolution as computed for single stars would mean in a double-star system when one component started from the main sequence to the red giant stage. Many puzzles, of course, still remain. Are the strange departures from the normal mass-luminosity relation caused by conditions existing as the systems form or do they later evolve into this state? What is the final result of continued mass loss? Hopefully, after today's papers and discussions we may be a step or two nearer to the solution of some of the problems involved and will be aware of as yet unasked questions concerned with stellar evolution and close double stars.