Unsolved Major Problems of Interacting Binaries – A Panel Discussion

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Unsolved Major Problems of Interacting Binaries – A Panel Discussion

Panel Members: P. P. Eggleton, R. H. Koch, Y. Kondo, D. M. Popper, J. I. Smak, and F. B. Wood (Chairman)

A panel of six astronomers was convened to contemplate unsolved major problems of interacting binary stars prior to the review of the symposium by Ed Guinan. Each panelist contributed his thoughts and then discussion was opened to the entire meeting. What follows is the written version of the panelists' comments.

Y. Kondo et al. (eds.), Evolutionary Processes in Interacting Binary Stars, 255–264.

Opening Remarks, Frank Bradshaw Wood

A number of years ago (J. Sahade and F.B. Wood) it was suggested that the studies of close double stars (or perhaps to be more precise, those showing eclipse or light changes caused by proximity effects) might be divided into four general time intervals. We suggested that the fourth era began about 1966 and that this beginning was marked by major emphasis in three fields, one of which was the use of satellites to permit observations is the far UV and x-ray regions. This has been one of a number of factors which have changed our ideas from the older idea of fairly stable systems in which a solution based on a good light curve could be called "definitive" and imply that further observations were unnecessary. The work in the years that have passed since this suggestion have fully confirmed it and have changed our concepts of many to systems which are rapidly evolving and in which one or both components have reached their stability limits - a concept first here introduced in 1950 (ApJ. **112**, 196), - and which has governed much of the work since. Here, I should like to take a minute or two to see how these have changed our concept of many systems and, as an example, I consider U CEPHEI. This is a much studied system, and at one time our knowledge was considered essentially complete, but it now seems that this is no longer the case. From 1913 discovery until 1978, at least 523 publication had been made covering this system, as listed in the card catalogue. One would have thought that little more could be learned. However, a series of papers (many but by no means all) by E. C. Olson and collaborators made it clear that relatively short time changes appear and that "the" light curve of one era was by no means that of another. It would require a much longer publication thus this even to begin to do justice to the observations and discussions which have lead to the changes in interpretation. A major contribution has been the observations from satellites which aided greatly in the visual work. We might call attention to the imagination interpretation of these by Y. Kondo, G. E. McCluskey and collaborators. (see ApJ. **247**, 202, 1981 for details and references to earlier works). Details of hot and cool spots and mass exchange (now invoked for many changes) are giving us details of this complex system. Many similar ones could be considered. We will see what changes the papers at this meeting present.

Finally, I recall that about the time I received my Ph.D. I considered the number of known systems, The rate and Completeness of discovery of new ones, and the number of telescopes and photometers which were available, or soon to be available for this work. I concluded that discovery would be essentially complete and "definitive" solutions made by the time I reached my thirty-fifth birthday, and that I would have to move to some other area of specialization. A glance at the papers presented here shows that this was scarcely the case.

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I will sort my comments into two (Nos. 1 and 2) conceptual ones and two (Nos. 3 and 4) driven by technology.

(1) We all know touchstones of the processes of binary evolution. The stellar bulk parameters, nuclear chemical signatures, and pulsational frequencies can serve here as examples. My first emphasis is that we must think harder about interpretation of our very simplest observational information – the histories of both period stability and variability. These are now quite rich for many close binaries. I will be explicit concerning three challenges. (a) For a long time, we have had at hand the two familiar dynamical interpretations which apply individually to a few dozen systems. For the great majority of variable–period binaries, however, the assertion is made that intrinsic variability must be the cause; we must admit this to be almost an empty statement. We cannot doubt that angular momentum re–distribution, whether the result of neutral fluid or MHD flows, is the only meaningful investigation to pursue. (b) In 1989, C.-H. Kim made me aware that, most probably, we limit ourselves unnecessarily by striving for a single cause for interpreting each binary's period history. Two or even more causes are likely to be in train concurrently and to beat against each other. But what then is to be made, for example, of the contact and quadruple system, XY Leo, for which a light-time interpretation seems (improbably at the present time) to provide a sufficient explanation for the observed variability? (c) More exposure should also be given to the few non-contact, feeble-wind-flow, MS systems which seem to show slow and small period changes. To sum up, I emphasize that we can really claim understanding of binary evolution only when we can interpret these easiest-to-make of all observational evidences.

(2) My second conceptual point is easier to make. By binary–parameter criteria more stringent than luminosity classification, we can now distinguish actively–evolving close binaries from incipiently–evolving systems from true MS ones. We should, therefore, be able to discover and track the beginnings of wind flow and the feeblest of clashing winds. There is no guarantee that the flow processes are initiated and sustained in close binaries in the same manner as they are in single stars of comparable mass.

(3) Analyses of the light and velocity curves for geometrically-deep eclipsing binaries have repaid observational effort many times over in quantification of stellar bulk parameters. In more recent years, these results have been coupled with deconvolved magnitudes and luminosities and all kinds of indices for the individual binary components. This, in turn, has led to deeper understanding of the evolutionary states of the stars. However, at two different times during the General Assembly just past, voice was given to the certainty that all is far from well in the "standarized" values of these magnitudes and indices. Discrepancies significantly greater than the expected accidental errors exist among independently–determined

photometric parameters for single, presumably–constant stars, not only in the *UBV* system but also in intermediate– and narrow–band parameters whether of line indices, conventional color indices, or triplex indices. We cannot doubt that the values assigned to binary star components are similarly afflicted. Commission 25 concerns itself, *inter alia*, with the theory and practice of telescopic photometry and in its scientific session in Buenos Aries showed forceful evidence that the cause of these problems rests in undersampling of the filtered information content in the stellar radiation curves. Further, the beginnings of a prescription to overcome the problems were presented there. This is most significant for the HR diagram positionings and evolutionary interpretations of fast and slow mass–transfering pairs and for atmospherically–eclipsing systems. If photometric parameters are determined to a precision of only 1% or even worse, we should not delude ourselves that modelling to 3 or 4 seemingly–significant figures is meaningful. On the grounds that we wish our assignments of physical processes to be tied as unambiguously as possible to evolutionary status, I make the plea that we try to understand and overcome the technical limitations of our photometric practices.

(4) My last remark is that we pay more attention ourselves and guide our students accordingly to underused opportunities which exist for us now. With even a 1–m telescope it is possible to achieve elegant spectropolarimetry of a number of evolutionarily-important binaries right now. We also need more workers to avail themselves of cm–wavelength observational capabilities with, *e.g.*, the VLA. If we think that the scattering envelopes and magnetospheres of binary components are important manifestations of binary evolutionary processes – and who can doubt this? – it is time to exploit these technologies already waiting for our use.

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One of the oldest unsolved problems in the field of CBS is that of the emission lines. The importance of understanding the mechanisms responsible for their origin is connected with the fact that they can be - and already have been - used as a very powerful diagnostic tool in studying close binaries in general and the circumstellar material in particular.

The relatively simpler case is that of the emission lines from accretion disks, observed in the spectra of CV's, Algols, etc. I am convinced that they are due to irradiation of the outer layers of the disk by the central source (the central star or the boundary layer) and that two mechanisms must be involved: (1) The highly oblique irradiation, producing a temperature inversion in the disk atmosphere, a situation favorable for the formation of emission lines; it turns out (cf. my paper presented at the IAU Colloquium No. 129, Paris 1990) that this mechanism can explain, at least partly, the Balmer lines. (2) The irradiation flux, containing enough high energy quanta, directly ionizing/exciting the atoms; several correlations (cf. Patterson and Raymond, Ap.J., 292, 535 and 550, 1985) clearly suggest that this mechanism must be responsible for the higher ionization/excitation lines such as, for example, He II 4686.

Much more complex and difficult to explain are the high ionization/excitation lines coming from the hot, turbulent circumstellar material, such as the UV lines of carbon, nitrogen, silicon, etc in the spectra of many Algols. Much progress is being made, as we heard during this Symposium, within a model involving the effects of the collisional heating or direct collisional ionization (due to the stream coming from the secondary), but it is a long way to go before we fully understand all these phenomena.

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In the short time allotted, I would like to make four comments. First, I am delighted to have the opportunity to attend a conference honoring my old friend Jorge Sahade. Second, there have been something like 40 oral presentations and 50 poster papers at this symposium. While most demonstrated progress in attacking problems of interacting binaries, most also ended pointing out unsolved problems. So all of you constitute our panel on unsolved problems rather than those of us on this stage. Third, I sit on this panel as a reluctant imposter. Interacting and other complicated binaries have had low priority for the most part in my programs. Finally, I discussed this format with my colleague Mirek Plavec, whose presence here we sorely miss. His insights and incitements have long been major aspects of binary star conferences. His prediction was that each panelist would list as unsolved problems only only those in fields of personal interest to himself. Mirek is seldom wrong in his evaluations.

In order to earn my keep, I suppose I must give some kind of list. A relatively unexplored binary realm, in which both photometric and radial–velocity analyses are difficult, contains systems with periods less than one day in which the components have radiative envelopes. Investigation in this realm has increased markedly in recent years, but no clear picture has emerged. A subtopic not understood is the O'Connell effect (difference in light levels at the two quadratures) for these systems. Invoking spot distributions does not appear reasonable in these cases. These difficult systems could provide a testing ground for comparing different programs for light curve synthesis as well as for comparing cross–correlation schemes in radial–velocity work with results from synthetic binaries.

I agree with Bob Koch that causes of period changes in close binaries of all classes are not really understood. We are still haunted by discrepancies between observation and theory of apsidal rotation in DI Her and AS Cam. With respect to interior models required to understand all aspects of stellar structure and evolution, including those of interacting binaries, we now have, in addition to an unsatisfactory theory of convection, the related question of convective overshoot, the importance of which is supported by considerable circumstantial evidence. Introduction of additional adjustable parameters on an *ad hoc* basis, rather than having a basic physical theory, leaves an unsatisfactory state of affairs.

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There are numerous interesting unsolved problems in the evolutionary processes of interacting binary stars. In my view, the following three are among the major unsolved problems.

(A) **Formation of close binary stars.** Peter Bodenheimer presented a good review of this subject earlier at this symposium. The formation of widely separated (say, hundreds of astronomical units) binary stars may effectively be treated the same way as the formation of single stars. However, the formation of close pairs is another matter. For one thing, there are so many of them that the formation process could not be that which depends entirely on chance encounters of multiple star systems. There are difficulties in forming two stars close to each other. The fission model has yet to be demonstrated to work all the way through; it should not simply be assumed that an instability in a rapidly rotating star would inevitably lead to a fission.

(B) **Mass flow, mass accretion and mass loss.** In the 1960s, the idea of wholly conservative mass flow, in which the matter lost by an evolving component is completely accreted by its companion, had a currency, although there was no strong theoretical basis, let alone observational evidence, to support such an idea. Recent observations, particularly ultraviolet spectra have shown in a number of binary systems that some of the mass lost by an evolving component escapes the system while a fraction is accreted by its companion. Yet, we do not know how much of the matter is being lost from the system and how much is accreted; nor do we know under what circumstances accretion disks are formed and how stable they are. Realistic modeling of mass flow is extremely difficult since the gas flow should really be treated at least as a two-dimensional hydrodynamic problem (with temperatures, pressures and densities varying in the direction of the gas motion as well as in directions lateral to it) but it is difficult enough to do one-dimensional calculations, especially with time-dependent variables. Ideally, of course, it should be treated as a multidimensional, time–dependent, magneto– hydrodynamic gas flow problem.

It is particularly important for us to learn what really happens at the time of dynamic mass flow during which the flow rate is thought to be in the range of 10^{-5} to 10^{-4} solar mass per year. What happens at that time will significantly affect the evolution of the two stars thereafter.

(C) **Formation of collapsed stars in proximity to each other.** We think we know how to form a white dwarf close to its companion. We simply let one component evolve through a giant phase and allow it *gently* to lose its mass. Two white dwarfs in a close orbit could in principle be formed the same way. Under

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certain conditions a neutron star could be formed in a binary system through a Type II supernova without disrupting the system but a pair thus formed is not likely a very close binary; a supergiant progenitor implies a certain minimum separation between the two stars prior to the explosion. The accretion induced collapse of a white dwarf near Chandrasekhar's mass limit could give birth to a neutron star close to its companion. However, it is difficult to see how a close neutron star pair could be formed in this manner. Let us supposed that a neutron star is formed through accretion induced collapse in the proximity of its "normal" companion star, which eventually evolves into a white dwarf. Even if that white dwarf turns out to be close to Chandrasekhar's mass limit, since its companion is a neutron star, there is no source to supply the mass necessary for its accretion induced collapse.

In problems (A) and (C), it is possible that something is being overlooked or there exist physical processes we have yet to discover. In problem (B), first of all we need a great deal of observational data of all sorts at all wavelengths to place appropriate boundary conditions on our models, and we need a vast improvement in theoretical modeling. After listening to the review papers and looking over the poster papers, I feel that gallant efforts are being made toward resolution of these problems. However, I would be pleasantly surprised if a clear answer to any of those questions should manifest itself before the end of this century.

Editors Comments
on The Major Unsolved Problems of Interacting Binary Stars

In the process of assembling these proceedings for publication, particularly the review of manuscripts, we decided that it would be appropriate for the editors to provide an additional commentary on the major unsolved problems of interacting binary stars, based on issues raised directly and indirectly within the symposium contributions. The papers presented in this volume advance our understanding of many issues regarding the physics and evolution of interacting binary stars but there are still many relatively fundamental uncertainties and problems that may profoundly influence our perception of interacting binary stars. It is these "problems" that we wish to mention in this commentary.

One of the basic concepts often applied in the study of interacting binary stars is the relative location, shape and size of the critical Roche (or Jacobian) equipotential surface, often popularly known as the Roche lobe. The surface arises from the solution of the restricted three–body problem, in which two point masses are in a perfectly circular orbit in the absence of any external forces such as radiation pressure or a stellar wind to act upon the mass–less third body. The critical equipotential surface is the one containing the inner Lagrangian (L_1) point but is only one of the *infinite* number of equipotential surfaces. It does NOT hold back the stellar atmosphere if one of the components should expand to that surface through evolution. On the other hand, if that stellar atmosphere should expand beyond the critical surface, the portion of the atmosphere that is outside this surface will have access (celestial–mechanically speaking) to the entire space containing the companion star. Consequently, if the two stars are discretely separate, neither of the components is likely to exceed the Roche surface. Additionally, in modelling contact binaries the Roche representation can provide a useful approximation. To that extent, the Roche concept is quite useful. One must, however, be cautious regarding the detailed use of Roche equipotentials, for clearly no binary system, particularly an interacting system, will fully satisfy the assumptions of this restricted three–body solution.

In understanding evolutionary processes in interacting binary systems we are still faced with the problem of mass and angular momentum loss and its influence on our ability to model the evolution of binary systems. In particular we must address the possibility that there is no "general" solution but rather each binary system evolves, to a detail that we can now distinguish, in a way that is dictated by its specific past history. In addressing this broad problem we must have a better understanding of mass transfer/loss processes and the driving mechanisms, the processes by which mass is accreted, and, of course, the physical and geometrical properties of the structure "between" these phenomena: the accretion disks or layer. The past few years have seen tremendous advances in the theory of accretion disk and boundary layers, enough to know that our earlier models were,

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indeed, simplistic, and that we have a long way to go before we understand how nature functions in detail. Related to this study is the problem of non–equilibrium phenomena in binary systems. The backbone structure of the vast majority of our modelling of interacting binary systems is the assumption that we are looking at an equilibrium phenomena. We must remember that this is indeed just an assumption and that non–equilibrium phenomena have been clearly observed in many types of binary systems. The removal of the equilibrium requirement would put much of what we do into question. There exists a tendency to pursue studies of a specific binary system in great detail and use it as a "Rosetta Stone" for a critical stage of evolution of all, or at least all of a particular subclass, of interacting binary systems. If we concentrate on a flashy but transitory and unimportant stage of evolution, then our perception of binary star evolution will be substantially different from reality. This is true even in the study of an isolated system – we must always ask ourselves whether an observed phenomenon is a global event, fundamental to our understanding of the binary or a random transient that just happened to be in our line of sight at the time of the observation.

Despite all these caveats, we believe there is hope for solutions. Our growing ability to observe simultaneously over a wide range of wavelengths, the advances of new detectors systems, space missions, and computing machinery will likely continue to produce revolutionary advances and allow us to resolve some of the issues and problems raised in this commentary.

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