CONCLUDING REMARKS

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Abstract. This has been a very good meeting. If you cannot read some or all of the papers in full, read at least these remarks. It is an attempt to summarize the highlights, naturally an attempt too imperfect, biased, and highly subjective.

Eight years ago, the topic of the evolution of close binaries was discussed at a conference for the first time. It was the International Colloquium on Double Stars; we were given one halfday session for our topic, and there were half a dozen papers. I remember clearly that I had to explain the concept of the Roche limit: it was unknown outside the small circle of specialists. I am happy to recognize here a few – Paczynski, Batten, Smak – who were with us then and remained faithful to close binary stars (faithful, that is, within the usual wide limits allowed nowadays in other areas where this adjective applies).

I think they will share with me the astonishment at the change. Today, we are concluding a full-sized four-day Symposium where more than fifty papers were presented, although we had excluded all contributions that would deal with too narrow, overspecialized topics. Such a growth of interest surprises even those who are intimately connected with the discipline. I think we must appreciate the fact that the Executive Committee of the International Astronomical Union understands the trends in astronomy, approved this Symposium and by a generous grant helped us to realize it. Another grant came from the Royal Society, to which we also express our thanks. And a substantial grant was made to us by the IBM Corporation. This is almost symbolical, for our discipline was created by computers rather than by telescopes. It is true that close binaries were suspected of having their own specific ways of evolution already at the time when Struve and others discovered their circumstellar material in the early forties, and when Kopal, Crawford, Wood and others recognized the Algol subgiants in the fifties. But how much could be done then on a theory that would explain the phenomena? Morton and Smak demonstrated that true ingenuity can achieve a lot even with a mechanical desk calculator. However, all this was pre-history. The true time for studying the binary star evolution came with the big computer, and Henyey's method.

The past achievements – the explorations of cases (I suspect that the words *modes* is more fitting) A, B, AB, and C, have been amply reviewed and discussed in the literature. Let us quickly survey the present situation as reflected in this Symposium.

One important aspect of the binary star evolution has been conspicuous by its virtual absence: the black holes in binary stars. Yet there was an opportunity here when the black holes almost asked to be invoked as an easy solution. This was when Dr Hutchings presented his list of early-type binaries with one massive but unseen component. Nevertheless, nobody advocated the black holes. This is an interesting change in attitude; I am sure some three years ago, a black hole would have been suspected round every corner. Somehow, they are out of fashion. After a good deal of effort to identify black holes in a number of binary systems, only Cyg X-1 appears as a really good candidate. It seems that the rest of the known binary X-ray sources can do with a neutron star. As to the optical eclipsing binaries, BM Ori is almost definitely out, ϵ Aur is no longer considered as a strong

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candidate, and even β Lyr is very doubtful. The consensus at this moment seems to be in favor of a less sensational model of a first mass transfer of mode *B* for β Lyr. Summarizing, we can say that we are back at the conclusion reached by Thorne and Trimble in 1968, that black holes do not seem to play an important role in known binary systems.

Curiously, this complete reversal of the attitude of some students of close binary systems forces me to reverse my stand, too, albeit superficially, for the basic attitude remains the same. I think that not only ladies of all ages, but scientists as well, are susceptible to fashion. And it is really not surprising that when a sensational new phenomenon is discovered, so many welcome it as a sure remedy and perfect explanation of all the puzzles that plague us. And when the universal new recipe fails to heal all wounds, the tide starts to go the other way; the disenchantment may be as exaggerated as the initial enthusiasm.

Therefore I would like to implore you to continue the search for black holes in binary systems with all energy. Their existence has not been abolished by the theorists. We have heard Dr van den Heuvel estimate that some 20–25 stars in the Bright Star Catalogue should be accompanied by a collapsed object. Black holes may not be frequent enough in binary systems to make the interpretation of many peculiar systems easier. Remember, however, that the identification of just only one safe case would be of tremendous importance for science. Naturally, we know now that each piece of evidence must be examined from all possible aspects before the statement is made. The time when one could say "We can't see the star, hence it's a black hole" is past, and it's just as well.

On the other hand, the fact that a system at least somewhat similar to β Lyr can be generated in the course of a mass transfer of mode B, underlines the importance of this mode. In the early years of the short history of our discipline, mode A was studied in considerable detail. It offered a very plausible explanation of the qualitative properties of the Algols, and whole grids of models could be computed. Gradually, mainly thanks to the limited but illuminating study by Benson, it was realized that in most cases the mass-accreting component will swell to form a contact system. The paper by Ulrich and Burger presented at this meeting indeed shows how greatly oversized and overluminous the accreting component can become if the rate of mass transfer is higher. When the impact energy is included in the calculations, and I think it should be done at least in a crude fashion, the effects will be even larger. Clearly, no future mass-transfer calculation can ignore the behavior of the accreting star. Formation of a contact system may be only transient, and some of the observed semidetached or even detached systems may have 'contact past'. Evidence has accumulated, however, in favor of mode B being most important for the Algols; this has been stressed here again by Ziółkowski.

Let me add that mode B of mass transfer may also be of great importance for our understanding of Be stars and shell stars. Here we deal less with the stars themselves and more with the circumstellar emitting or absorbing disks, rings, or clouds. In order to get a good picture of the circumstellar phenomena, we can no longer be satisfied with the overidealized models where the actual process of mass transfer was ignored. But then the overidealized models computed in the first epoch cannot satisfy us when the stars are concerned, either. Loss of mass from the system, and loss of orbital angular momentum are a very distinct possibility in many cases, and a certainty at least in some cases. How much is lost is the big question.

It is therefore very fortunate that so many theorists are studying now the process of mass transfer, formation of gaseous disks, and the accretion process itself. We had a most exciting time at this Symposium listening to the exchange of arguments and watching the various disk models on the screen. A quick look at the slides does not permit one to see all the differences and similarities. Moreover, the models represent different initial conditions (such as the relative size of the stars, and size of the Roche critical lobe, the latter being determined by the mass ratio) as well as different stages – initial stages of the ring formation as against the steady state. Taking these differences in mind, I was almost tempted to see more similarities than differences. However, the authors have very vehemently assured us that the differences in their philosophies and results are large and substantial. The models by Pringle assume very high viscosity, which transports a considerable amount of angular momentum outwards, leading to mass loss from the system, and which destroys the disk or ring quickly as soon as the mass transfer process stops. In the picture offered by Shu and by Flannery, viscosity is low, material is transported to the surface of the accreting star by shock waves, the process nearly conserves mass and angular momentum in the system, and the disk will persist for a long time after the gas supply is turned off. It makes a lot of difference for our models of Be stars and other objects whose model is accepted. The problem of the viscosity of the accretion disks is very worrisome; however, the discussion was exciting and it is a pity that we could not devote more time to it.

I do not think that it will be necessary for one group of theorists to commit genocide on the other group in order that one model may prevail. I hope that the observers may be judges or at least a jury. Much better spectroscopic and photometric observations of the circumstellar matter are possible now, and their quantitative analysis promises to tell us a good deal. Although this Symposium has been predominantly theoretical, first observational results of this kind did show up in the papers by Batten, Baldwin, and Plavec and Polidan. Of particular importance was the sudden flareup of the emission in U Cep in 1974. We do not know yet what this event signaled to us; it may very well be that the star tried to support the ideas of variable mass transfer advocated here, from different points of view, by Hall and Bath. There are other observational tests. I was very taken by Icke's suggestion that turbulent disks should create coronas, and that coronal lines may under favorable circumstances be observable. Hall's immediate remark that KU Cyg is a good candidate for this phenomenon shows how valuable are meetings between theorists and observers.

Another highlight of the Symposium has been the work reported by R. F. Webbink. No doubt the *genius loci* here in Cambridge deeply influenced him as well as Whelan, Eggleton and others, for their contributions to the theory of close binary stars are truly impressive. When we sit down to read Webbink's contribution, we will probably recognize an overview of the whole subject of mass transfer which marks a new epoch. Here the attention was focused on several interesting specific cases where the mass transfer may become so rapid that the binary system is completely altered or even possibly vanishes. It seems that the generally quiet process of mass transfer can at times produce cataclysmic phenomena, competing with novae.

Speaking of novae, the paper by Starrfield and subsequent discussion showed that even this very difficult problem is subject to concentrated attention, and the effort to explain more and more aspects of the nova phenomenon is certainly to be appreciated. Even more investigators have concentrated on the dwarf novae. In the cataclysmic binary system we observe coexistence of a white dwarf and a cool main-sequence star. Systems where this coexistence is not quite peaceful easily attract our attention. For a certain time, we knew only one quiet system, BD + $16^0$516. Its discoverers, Young and Nelson, spoke here about it as a "well defined anomaly". We will agree with them that thanks to their work the words "well defined" are quite appropriate. We wonder, however, if the system is an anomaly. Recently well-defined rumors circulated that M. Schmidt of the Hale Observatories had discovered another eclipsing pair where the white dwarf is eclipsed by something much cooler.

A striking and puzzling characteristic of these dwarf binaries – cataclysmic or quiet – is the extremely short orbital period. It is hardly possible to imagine a scenario which would lead to such systems without a previous stage of contact or of a common envelope. For some years, it has been surmised that the progenitors were the W UMa systems. We have heard from several independent investigators that this is very unlikely. More probably, the dwarf binaries may be descendants of semidetached binaries where the mass transfer occurs at the red giant stage of the more massive component. At that stage the mass transfer can be so violent that the other star is in fact engulfed by the on-streaming material. Ostriker and Paczynski went even farther and studied the motion of a star in the huge envelope of a giant. The idea, you may recall, appeared already in a paper by Sparks and Stecher. Ostriker and Paczynski maintain that such a process is relatively slow, but releases great amounts of energy and angular momentum. Eventually the whole envelope of the giant may be blown away and form temporarily a planetary nebula with a double nucleus. The nucleus then remains, and we observe two small stars very close together, one of them being the white dwarf representing the core of the original giant.

Although they apparently have lost the importance of being the progenitors of the cataclysmic variables, the W UMa systems are also studied with great zeal. How long ago is it that we believed them to be almost inaccessible to deeper investigations? Sophisticated methods exist now to disentangle their light curves, and since the pioneering work of Lucy in 1968, structural models of these contact binaries have been studied with admirable success. The excellent review paper by Hazlehurst and several papers that followed have shown us how much the work has progressed beyond and above Lucy's model. It appears now that both component stars of the contact systems are perpetually out of thermal equilibrium, and the systems vary in cycles.

This Symposium also stressed the role of the stellar wind in many binary star phenomena. It seems that the wind can be an additional important vehicle for transferring mass, besides the now classical overflowing of the Roche limit. Its inclusion naturally always implies loss of mass of angular momentum from the system.

Just by chance, the stellar wind was invoked in the case where the alternative hypothesis calls for fission as a process to generate a binary star. This association thus brings in mind the origin of binary stars. It is gratifying that some work has been done on the problem of the origin of binary stars, and on statistical surveys of their properties.

Quite naturally, the X-ray binaries remain an extremely attractive subject, and stimulated many papers on the evolution of massive binary stars (although we know now that some binary X-ray sources are of relatively low mass). I will not dare to summarize the talks on the X-ray sources. Rather, I would like to use them to illustrate the trivial truth that theoretical advances are fascinating, but ultimately the driving mechanism in astrophysics are observational discoveries. A few years ago it was worthwhile to speculate why none of the about fifty then known pulsars was a member of a binary system; and there existed plausible hypotheses suggesting that no pulsar can be born in close binaries. This year, Hulse and Tayler discovered a pulsar in a binary system. It is certainly very gratifying that within a few months, we already have several theoretical studies explaining how a pulsar can be born in a binary star. Nevertheless, this story should teach us to be extremely modest in our claims of understanding Nature. It is good to recall sometimes the old wise Faust's Mephisto:

"Grau, teuerer Freund, ist alle Theorie,
und grün des Lebens goldner Baum".

This Symposium has been predominantly theoretical. It appears therefore appropriate to conclude the Concluding Remarks with some comments on the progress in observations. This progress has been truly impressive, too, and poses new questions. Dr B Paczynski correctly pointed out that such a new fascinating discovery is for example the coherence of the very short periods (20–30 s) in dwarf novae, as reported in the excellent review talk by Warner: "Whatever mechanism is responsible, it has a good memory, and preserves its identity over a large number of cycles." Observations, however, do not only pose questions, they also promise answers. It was reported to us by Wu that the recent scientific satellites are able to study the far ultraviolet and soft X-ray radiation in objects as faint as the dwarf novae or symbiotic variables are at minimum light. Paczynski quite justly commented that the complex problems of the hot spots, unstable disks or unstable stars will certainly be better understood in the light of these observations.

It seems to me that the next symposium on close binary stars should probably be more oriented towards the observational aspects. In any case, I am sure that within say three years we will be ready for another good thorough discussion and review of the problems. While we can then organize another good but different symposium, I am less sure we can find a place as good as Cambridge. We appreciate the hospitality offered us here by the Director, Professor D. Lynden-Bell, and the great effort by Drs Whelan, Mitton, Eggleton, and by all their aides; they created a well-functioning machine which kept the meeting going smoothly and at the same time – contrary to many other machines – kept the atmosphere pleasant. Our thanks also go to all participants and contributors, in particular to those who prepared the many excellent review papers.

DISCUSSION

Paczynski: I partly disagree with Professor Plavec. At this Symposium I was most impressed by the observational reports. The first was the most impressive presentation of fast photometry data on novae and dwarf novae by Brian Warner. I would like to emphasise the importance of his finding that the short period oscillations (20^s–30^s or so) observed by him in dwarf novea are *coherent*. In spite of the slow change of the period he can phase the oscillations through the observing run which can be a few hours long. That means that we have not just a peak in the power spectrum, but truly coherent oscillations. There is a long memory in whatever object is oscillating. This will have a great impact on the theory of dwarf novae.

The second impressive report was by Dr Wu. We can observe U Gem at the minimum light all the way down to 1550 Å. I hope we will be able to obtain a light curve and to see how luminous is the hot spot. The theories of Bath and Osaki make different predictions here, so Dr Wu may be able to demonstrate observationally which theory, or in fact which point of view, is correct.

I think I should apologise here for a letter I wrote half a year ago to the Chairman of the Organising Committee, to Professor Plavec. I was afraid at that time that we would invite to Symposium only the dinosaurs, like myself, people who were interested in binaries for decades. I am impressed that, in fact, we managed to bring here so many new people. The medium age of the speakers here was well below my age. It is really encouraging that the field of close binaries is attractive to so many people today.