EVOLUTION OF BINARY SYSTEMS AND THEIR GENERIC RELATIONS

*Vainu Bappu Memorial Lecture**

ZDENĚK KOPAL
Department of Astronomy, University of Manchester, England

(Received 9 August, 1983)

Prologue: I rise to address you today with sadness in my heart – sadness which I trust is shared by all those here present; for as is underlined by its subtitle, our beloved colleague Vainu Bappu – past President of the International Astronomical Union, and life-long student of the subject of our Colloquium – is no longer in this world to take his rightful place among us; and, therefore, my remarks which follow can be dedicated only to his memory.

I feel sad all the more as Vainu Bappu was also once (albeit for a rather short time) a student of mine at Harvard University; and the course he took with me on eclipsing binaries in the spring term of 1948 may have been the beginning of his life-long interest in binary systems, and in problems arising from their existence. Fate separated us by thousands of miles for most part of our subsequent lives; but brought us together in the end again: namely, in the spring of 1982 when Vainu Bappu invited me to give a course of lectures at the Indian Institute for Astrophysics founded by him at Bangalore. Their main topic was to be the Fourier analysis of the light changes of eclipsing variables, and to last several weeks; but the course soon overflowed its originally-intended scope into more general problems connected with the evolution of double and multiple systems of stars.

It was a real pleasure to see Vainu in my audience once more after one-third of a century – looking not a day older than I remembered him from our Harvard days – with his keen interest in the subject undiminished by time; and his brilliant remarks were enjoyed by the audience as much as by myself. But all good things have got to have an end; and this end came when Vainu and other friends took me to the Bangalore airport on the day of my departure. The connecting flight to Bombay was late; but astronomers never waste their time: we immediately started to discuss the problems of evolution of the binary systems, and continued until my flight was called for boarding about an hour later. Of course we did not solve any; but our minds were full of them as we shook hands at the gate, and made another date to discuss them in August at Patras, Greece. Alas, it was not to be; for not many days after the XVIIIth General Assembly of the I.A.U. (over which Vainu was to preside) started, grievous news came from Munich of his untimely passing. Requiescat in pace!

When, therefore, the Organizing Committee of this Colloquium honoured me with an invitation to deliver the Bappu Memorial Lecture at Bandung, which more fitting subject could I choose for it than that we discussed at Bangalore during our last hour together in this world? And so it will be – in the hope that especially the astronomers of the younger generation – both those here present as well as those who may read what I have to say – will be able to advance our subject beyond the limits to which its milestones were carried by astronomers of my age. Not that they will all solve them; this is too much to hope; but step-by-step advances must continue relentlessly, as time goes on, to make us understand better the phenomena which we observe in the sky.

Some 30–40 yr ago – when the world was recuperating from its most recent holocaust, and the problems of the evolution of double and multiple stars began to claim serious attention – the situation facing us could well be described by the words of the Bible: "the harvest is large, but workers are few". Now – thirty years later – this is no longer the case. The number of astronomers interested in these problems has increased

---

* Delivered on 3 June, 1983 at the Lembang-Bamberg IAU Colloquium No. 80 on 'Double Stars: Physical Properties and Generic Relations', held at Bandung, Indonesia.

Astrophysics and Space Science **99** (1984) 3–21. 0004–640X/84/0991–0003$02.85.

by at least one order of magnitude; and brought in its wake such a flood of 'scenarios' aiming to account for what we observe that a casual perusal of contemporary literature on the subject is more likely to be-wilder, rather than enlighten, the student entering this field at the present time. This fact alone reveals – if anything – that many of the problems at issue do not admit as yet of unique solutions; and that the outcome of our attempts at their solutions depends, do not so much on given observational constraints, as on *ad hoc* assumptions introduced to render the problem tractable, or determinate at all. What may, therefore, be a better service to Bappu's memory than to stress what we do *not* know as yet, or what remains still uncertain, rather than to indulge in further proliferation of hypothetical scenarios and pass them off as gospel truth. We must, above all, allow ourselves to be guided by the observations; and this is what I shall endeavour to do in this lecture.

1. Introduction

Before we come to grips with individual aspects of our problem, it befits to define first what we mean by double or multiple systems of the stars. In what follows, we shall consider as such the associations of stars which mutual attraction compels to revolve around the common centre of gravity for a time span that is long in comparison with orbital periods of such configurations. For most systems which we shall have an opportunity to recall in this lecture, this disparity will amount to many orders of magnitude – a fact which will make their gravitational liaison indissoluble – till 'death does them part' in (say) the holocaust of a supernova explosion or by other means of comparable violence.

Our current knowledge of the frequency-distribution of double stars in mutual separation is so far but very incomplete – largely due to observational selection which hampers discovery of pairs within certain ranges of separation, and favours others. The upper limit is set by properties of the fluctuating gravity field in which a binary pair happens to be situated, and which tends to dissolve it (cf. Chandrasekhar, 1944); while the lower limit is given by the dimensions of the constituent stars. In that part of the galactic spiral arm which happens to be our celestial home, this upper limit comes close to half a parsec (cf. Kopal, 1978; p. 10), corresponding to orbital periods of the order of 10^8 yr; while, at the lower limit, the orbital dimensions may amount to 10^4 km (corresponding to the dimensions of white dwarfs), and periods of revolution to only minutes of our time. Moreover, a discovery of pairs still smaller in size – and revolving in seconds rather than minutes – can be expected with confidence in the future.

All such objects constitute a huge reservoir of binary configurations in their own right; and represent probably the major part of stellar population of our Galaxy. If their separations are large, we refer to them as 'wide' (which, in our proximity in space, can manifest themselves as 'visual') binaries; while if this separation becomes comparable with the dimensions of the constituent stars (or does not exceed them by more than one order of magnitude), we speak of 'close' binaries – requiring completely different (spectroscopic, photometric) methods of discovery.

In more recent times, the custom has begun to take root to refer to the latter as 'interacting' binaries; but to me this term does not seem to offer any advantage. For – by definition – the components of *all* binaries are bound to interact; gravitationally alone if they are wide; and gravitationally as well as hydrodynamically (or hydromagnetically) if they are close. To refer only to the latter as 'interacting' is, therefore, illogical – merely calling the same thing by a new and longer name – and as long as no better nomenclature cna be proposed, in what follows we shall continue to refer to the two groups of binaries as 'wide' and 'close', respectively – with the understanding that the components of wide systems interact only gravitationally, whereas in close systems they can interact also hydrodynamically, or even in a more complicated manner. Both belong to the topic of our discussion, and their generic relations should be of equal interest to us.

The *evolution* of binary or multiple systems – be these close or wide – commenced to emerge as one of the central problems of contemporary astrophysics since the latter 1940's, when the general framework of stellar evolution was being placed on a sound physical basis. Much of it was, to be sure, foreshadowed by the earlier work of Eddington and his contemporaries in the first half of this century; but it was not till the work of Bethe and others that the evolution of matter under conditions prevalent in stellar interiors could be described in terms of exothermic nuclear reactions. In particular, it became possible then for the first time to relate the rate of energy production of the stars with their mass (and chemical composition) in a quantitative manner.

As long as a star is single, its mass, chemical composition, and age remain independent parameters which cannot be uniquely deduced from the observations. However, the double (and multiple) star systems – be these close or wide – constitute an extreme type of stellar associations, which remain inseparable for time-intervals exceeding the age of our Galaxy (cf. Chandrasekhar, 1944), and must have originated from pre-existing gaseous substrate so well-mixed (by turbulence) that the chemical composition of the material constituting their components may initially have been indistinguishable. Morever, their formation must have occurred at (very approximately) the same time; so that their present aages must likewise be essentially identical*. Such stars could, therefore, have differed only in their initial mass; and if so, the evolutionary tracks of the components could subsequently begin to differentiate only on account of this fact: the larger the mass, the faster should be the rate of nuclear evolution – with all consequences which this may entail.

When we turn to confront this simple consequence of the theory of nuclear evolution with what we actually observe, we find that the observations verify these theoretical expectations (within the limits of observational errors) *as long as both components remain on the Main Sequence*. In such a case, the more massive component invariably turns out to be the larger and the hotter of the two; and if their mass-ratio is very close to one, the components remain virtual twins.

This, however, continues to be the case only as along as both stars happen to be on the Main Sequence and derive their energy output from a conversion of internal

---

* For massive stars (with, say, $m \gg 3 \odot$) the rate of Kelvin contraction towards the Main Sequence is such that the individual components should have ignited their hydrogen within less than 10^6 yr of each other. In systems with one component very much less massive (say, of the order of $1 \odot$) than its mate, the time interval between their respective births could amount to 10^7 yr or more – but still very short in comparison with the total span of their subsequent evolution.

hydrogen into helium. Once, however, at least one component of the binary pair has departed from the Main Sequence, the theoretical amulet appears suddenly to have lost its charm; for it is no longer the more massive component which continues to lead on the evolutionary track of the pair, but, instead, the lead has passed on to the less massive one – in flagrant contradiction to theoretical expectations. This, moreover, appears to be true of all types of binary systems *regardless of proximity of their components* – visual systems like Sirius or Procyon (in which a typical Main-Sequence star is attended by less massive white dwarfs) are even more pronounced examples of such a situation than (say) Algol or other similar close binaries, in which this phenomenon first happened to attract attention. This perplexing fact earned for itself the name of an *evolutionary paradox*, which began to stare us in the face since about the middle of this century, and has continued to do so ever since. In what follows we wish to describe the present state of this problem, and attempt to foresee the way in which its solution should be sought.

2. Evolutionary Paradox

The first step towards an identification of the cause of the paradox outlined in the preceding paragraphs may appear to be simple, and ascribable to a breakdown of our tacit assumption that the stars evolve along the tracks of constant mass. Indeed, a study of the physical properties of double stars discloses these to be compatible with the observations only as long as their components remain on the Main Sequence; but not necessarily beyond the hydrogen-burning stage. Certainly the existence of such close pairs as Algol – on which a subgiant of gK0 spectrum is 4.7 times less massive than the principal Main-Sequence component of spectrum B8 – or a wide binary like Sirius – in which a Main-Sequence A0-star is attended by a white dwarf 2.3 times less massive – forces us to recognize the fact that this could not be true unless the present secondary (i.e., less massive) components of such systems were once more massive of the two; and attained their present state only after *losing* a large part of their initial mass some time after an incipient shortage of hydrogen forced them to abandon the Main Sequence. Thus far a general agreement exists that (short of abandoning the basic tenets of nuclear evolution of the stars – and this should be considered only as our last resort) the components of binary systems must lose a large part of their initial masses – to enable them eventually to satisfy the Chandrasekhar limit and become white dwarfs.

The question is only: when does it happen and why? Is, moreover, the binary nature pre-requisite for such an act, or is this bound to happen to every star which has reached the necessary stage – even though the phenomenon may become observable only in binary systems? It is on these questions that opinions still differ, and the final answer is not yet in sight; for this answer is intimately connected with the *physics* of the processes causing the loss of mass of the stars, and not – strictly speaking – with the astronomy of binary systems.

Let us attempt to explain why this is the case – and this part of my talk could almost be given a Shakespearian title of 'Comedy full of Errors'. It is as though Nature – that greatest of teasers – has dangled before our eyes many misleading clues to test our intelligence, and to watch with interest how long it may take us to disentangle them.

This story commenced with the work of Schoenberg and Chandrasekhar (1942), demonstrating that when hydrogen exhaustion in the central parts of a star drops below a certain limit (about 12% by mass), its core begins to shrink; and a conservation of the potential energy of the configuration as a whole then requires that this shrinkage of the core of a star must be compensated by an expansion of its outer parts (the 'mirror effect') causing the star to grow in size.

As far as single stars are concerned (and it is these alone that Schoenberg and Chandrasekhar had in mind) this argument remains unanswerable. But not necessarily so in close binary systems; for there it is the total energy of the system as a whole which should be conserved; and dissipative processes (like dynamical tides) allow for an exchange of potential energy between components and the kinetic energy of the system. Both of these processes operate on the Kelvin time-scale; and the efficiency of exchange depends on the viscosity (plasma, or turbulent) of stellar matter. In the absence of its more accurate knowledge, it is impossible to estimate the extent to which the Schoenberg–Chandrasekhar 'mirror effect' should be applicable to the components of binary systems with any assurance; but, for the sake of subsequent discussion, let us assume that this is indeed the case.

If the stars were single (or the component of a wide binary – like Sirius or Procyon), the post-Main Sequence expansion caused by the 'mirror effect' could continue unchecked as long as a shrinkage of the core keeps providing the surplus potential energy for expansion of the outer layers. However, in close binaries, the proximity of the companion will surround the expanding star with an invisible barrier – in the form of the *Roche limit*, defined as the largest *closed* equipotential volume capable of containing the star's mass. If and when a given star has reached this limit (due to the operation of the 'mirror-effect'), its further growth in size may get *arrested*; and a continuing tendency to expand could bring about an actual loss of mass.

That this may indeed be the case was supported by an independent discovery by Crawford (1955) and the present speaker (Kopal, 1954, 1955) that, in a whole group of close binaries (including Algol), the secondary (less massive) component just about fills in its Roche limit while the primary's mass remains well inside this limit. I bestowed on these the name of 'semi-detached' systems (Kopal, 1955); and their existence has become since one of the cornerstones of the modern double-star astronomy, whose physical implications will be discussed below. For the moment, I should like to stress a frequently-overlooked fact that it is virtually impossible to prove whether or not any one particular system is actually semi-detached. In principle, this can be done by a comparison of the fractional dimensions of the 'contact' component (obtainable, for eclipsing variables, from an analysis of their light curves) with the fractional dimensions of its corresponding Roche lobe (obtainable from the spectroscopically-determined mass ratio). Both these quantities are, however, known to us only within certain limits of observational errors; and their coincidence renders a contact nature of the respective star only the more probable, the smaller the range of the respective errors. If, however, the fractional dimensions of the components tend – as they do – to coincide with those of their Roche limits for a whole group of systems independently observed, the probability that such coincidences are not accidental becomes so strengthened as to render the existence of such 'contact' stars tantamount to an observed fact. The question is only about its meaning; but once we raise it, we find ourselves at once in deep waters.

In looking back at the interpretation of this fact when its was discovered 30 yr ago, we cannot but confess that these early attempts amounted to but little more than jumping to conclusions in naive belief that Nature should be comprehensible in the light of such knowledge as we possessed at that time. Alas, clouds soon began to gather in the sky over such a presumption; and Nature soon gave us a salutary lesson for our impatience and loose thinking. In what way did we deserve it?

3. Mass Transfer

Shortly after the existence of contact components was discovered in close binary systems, and coupled with the expected effects of developing hydrogen shortage, Hoyle (1955) put forward an attractive hypothesis of 'mass transfer' between components of such systems, which certainly did not lack merit and can be summarized as follows.

When the hydrogen abundance in the deep interior of a Main-Sequence star drops below 12% by weight, its core begins to shrink and outer layers expand towards the Roche limit; having attained it, the latter begins to 'leak' through the Lagrangian point L_1 (at which the effective gravity vanishes and the equilibrium becomes neutral) to 'overflow' on to the secondary (up to that time, less massive) component, and augment its mass to the extent to which the erstwhile primary may become the secondary star of smaller mass, but evolved from the Main Sequence*. By a further extension of the same argument, Hoyle conjectured ... "a possibility that the predatory star will be forced to make amends for its former behaviour by returning material to the (at present) fainter star, which it robbed of mass so unfeelingly in the past. In the interest of cosmic justice it is to be hoped that this happens; but whether it does or not is unsure" (cf. Hoyle, *op cit.*; p. 200). And – we may add – the doubts on whether or not this entire process is actually operative (or, at least, responsible for a complete explanation of our evolutionary paradox) still continue to be with us almost 30 yr later; for the following reasons.

First, as had been pointed out at the very beginning of our subject (Kopal, 1954, 1955; Crawford, 1955), in every single system known at that time, it was *the less massive component which appeared to fill its Roche limit*; while the fractional dimensions of its more massive mate remained well interior to this limit. The question why we do not observe systems at the immediately preceding stage, in which the (originally) more massive star begins to expand towards its Roche limit and start disgorging mass which

---

* Can – to leave no stone unturned – the secondary components in semi-detached systems still be in the pre-Main-Sequence stage of Kelvin contraction? Scarcely so; for (quite apart from difficulties with the time-scale), no cause is known (cf. Kopal, 1954; p. 685) why their contraction should be arrested at the Roche limit.

would reverse the role of the two components, may have been at least partly answered by Morton (1960) and Smak (1962), who pointed out that the stellar evolution, which could take the system through that stage, unrolls on the Kelvin time-scale – with sufficient rapidity for few if any such stars to be 'caught in the act' of exchanging their roles at any particular time. A fuller discussion of our problem at this stage can be found, e.g., in Plavec (1968) or Paczynski (1971), and need not be repeated in this place.

Such views could have been seriously considered at the time when they were first put forward; for the sample of data then available was limited. In the 24 yr which have elapsed since, the sample of known semi-detached systems has at least trebled – and still no case of transitional stage was caught in the net of our observations – a fact which would require this act to occur all the more rapidly to escape detection. It is this fact which led a predominant majority of theoretical investigators of the respective stage of stellar evolution to choose only *massive* binary systems for their studies – in which the evolution (on the Kelvin as well as nuclear time-scale) can proceed indeed at a sufficiently fast rate. However, such a strategy ignores the fact that systems so massive are *very rare* per unit volume of galactic space (though not so rare in our catalogues of bright stars, as observational selection favours their discovery). As is well known, a large majority of stars in our Galaxy – in fact, some nine-tenths of them – possess masses smaller than that of the Sun; and (as far as we know) a high percentage of such systems form likewise binary systems. Your present speaker pointed out a number of them which appear to have reached semi-detached state (cf. Kopal, 1971) in spite of the smallness of their mass – systems in which the Kelvin time-scale may be longer than that of nuclear time-scale of more massive stars – but their existence has been greeted by most protagonists of the 'mass-exchange' scheme only with an embarrassed silence.

The main weakness of all schemes postulating a mere exchange of mass between the components – such that the total mass of the system remains conserved – is, however, an inadequate physical basis of the processes by which this exchange is to take place. The conventional view that the reason of the mass loss from contact configurations is low gravity prevalent there – so that any hypothetical transfer requires but a minimum amount of energy to make it operative – lands us on the horns of a dilemma. For a low-velocity mass transfer is to be accomplished fast enough to escape detection, the density of mass being so transferred must be very high to accomplish the purpose; and a flux so dense would have to absorb light effectively enough to deform the observed light curves of close eclipsing binaries to an extent which has not been verified by the observations. The same amount of mass can, of course, be transferred by a star at lower densities if the material moves faster; but then not all of it is likely to be acquired by its mate, and some can escape from the system – thus violating the assumption that the total mass of the system should remain constant.

The actual means of mass transfer by gas streams from one star to another will be considered in more detail in the next section, in the light of the constraints imposed upon it by the observations. In doing so we shall find that the principal weakness of most schemes of this type proposed so far was the *assumption* that *mass is only exchanged between components, but none is lost to the system*. From the physical point of view, an initial assumption of this kind is certainly unwarranted; for, if true, it should rather follow as a consequence if the investigation leads to such a conclusion; but to assume it in advance represents, in effect, an undue interference with the physical basis of the problem. It does possess, however, one merit; and this is why it had been so widely adopted in the past: namely, it simplifies computations performed on so restricted a basis.

The question can, of course, be then asked: what meaning can the results of such computations possess, and how legitimate is it to compare their outcome with observations? The reason for such doubts will transpire more fully in subsequent parts of my address; and may explain why we conjectured that William Shakespeare could have been tempted to entitle the contents of this section as a 'Comedy full of Errors'. Indeed, he may have gone further and called it 'Much Ado About Nothing' if the observed facts at the basis of our discussion would not demand explanation. And if, perchance, some readers may have found our comments on the contemporary scene too frivolous, they can only turn some pages more in Shapespeare's *Collected Works* to re-name it: 'As You Like It'.

4. Gas Streams

To paraphrase a witty remark of Lippmann to Poincaré (which concerned the exponential law of error distribution; cf. Poincaré, 1896), "everybody believes in the gas-streams in close binary systems: the observers, because they think that the existence of such streams can be proved by mathematicians; and the mathematicians, because they believe that such streams have been established by the observations". It is certainly true that the ideas exposed in the preceding section would not have been received with such a ready ear by so many investigators in the past, had it not been for the fact that they appeared to derive support from certain observed facts – mainly spectroscopic – which antedated the emergence of our 'evolutionary paradox' and seemed to offer an easy way out of our difficulties: namely, that several close binaries exhibited lines in their spectra whose Doppler shifts did not correspond to the orbital motion of their components, but were seriously at variance with them. The first example of such lines – its so-called B5 lines – in the spectrum of β Lyrae were discovered by Bělopolsky before the end of the last century (cf. Bělopolsky, 1893, 1897); and, somewhat later, recalcitrant metallic lines (mainly of Fe and Mg) whose Doppler shifts did not follow the orbital motion of either component were discovered also in Algol (cf. Barney, 1923). More recently, Carpenter (1930) found that the hydrogen lines in the spectrum of U Cephei – a well-known eclipsing system of virtually circular orbit – exhibited Doppler shifts indicative of highly asymmetric radial-velocity curve of its A0 (later reclassified to B8) component – simulating, in fact, a spurious eccentricity e as large as 0.47! This was certainly a anomalous phenomenon, to which astronomers of that time (led by Henry Norris Russell) preferred to adopt an ostrich attitude – until such a posture was made untenable by the pioneer work of Otto Struve and his school between 1940–1950. For Struve not only fully confirmed the genuine nature of Carpenter's earlier results for U Cephei (cf. Struve, 1944), but detected at least another pair – namely, RZ Scuti (cf. Neubauer and Struve, 1945) where a similar effect was even more conspicuous.

As is well known, Struve and his followers (for a summary of their views, cf., e.g., Struve, 1950) ascribed the origin of the anomalous observed Doppler shifts to *gas streams* within the respective systems; and their views – graphically illustrated by the familiar 'elephant trunks', stretching from the conical point of the contact component to its more massive mate – became for many years almost a trademark for this line of thought. And when, in the mid-1950's we came to face our 'evolutionary paradox', it was only too tempting to identify such streams with the mechanism of mass-transfer between the components described in the preceding section. Yet – as it happens so often in the history of science – a little more patience in the interpretation of the observational evidence could have held us back from premature jumping to conclusions, and made us think whether or not such a view can be justified also on other physical grounds.

In an attempt to answer this question, let us return to the gist of the argument advanced in the preceding section. As the star expands at a sufficiently slow rate, the value of the potential of all forces acting upon the surface remains constant, but its gradient (i.e., the gravitational acceleration) does not. When the star has eventually reached its Roche limit, the surface potential attains a minimum value it can possess for any closed configuration; while the gravitational acceleration – varying over the surface and diminishing in the direction of its mate – actually vanishes at the conical point (identical with the Lagrangian point L_1). This fact, by itself, need not cause any mass to escape; for its equilibrium there is merely neutral, and the Roche limit represents only a *static* configuration. A smallness of gravity in the neighbourhood of L_1) should only make it *easier* for *small* perturbations to remove mass from there than from any other part of the star's surface.

Such considerations prompted in the past several investigators (Kopal, 1956, 1957, 1959; Gould, 1957, 1959; Plavec and Kříž, 1965; and others) to consider a hypothetical outflow of mass from contact configurations as a problem of *particle mechanics*, within the framework of the restricted problem of three bodies. In retrospect, however, all this mechanical approach was doomed to constitute scarcely more than a numerical exercise, with little or no relevance to the physics of our underlying problem (except, perhaps, that the periodic orbits of this type may represent limiting cases of steady-state hydrodynamic flow, obtaining when its density is allowed to approach zero). The reason why this should be so is the fact that an appeal to the restricted problem of three bodies could be physically justified only *if the mean free path of the particles ejected by the expanding star are long in comparison with the scale of their motions;* so that the mutual *collisions* of moving particles (i.e., a *pressure* generated by them) can be *disregarded.*

Can this, however, be true of gas particles (atoms, or ions) which can leave a trace in the observed spectra of binary systems? The equivalent widths of anomalous line profiles in the spectra of U Cep (or RZ Sct) can, in principle, disclose the number of atoms (or ions) along the line of sight capable of producing the observed effects in the spectra. For U Cephei, the deformed lines are essentially those of hydrogen; and absorption properties of hydrogen are well known. In making use of them, Batten (1974) found that the number of hydrogen particles necessary to account for the observed spectroscopic anomalies should be between 10^{12}–10^{13} atoms per cc. However, at such densities *the mean free path of the respective particles would be many orders of magnitude smaller than the dimensions of the respective flow* – i.e., in the ‘elephant trunk’ of mass transfer – a fact which discloses that pressure within it cannot be disregarded; and that, in the absence of any (unknown) force to contain the material in transit to its gravitational pipelines, the respective gas should *disperse* into space before reaching its desired destination.

Byt worse is yet in store for those who may wish to save the simple interpretations at all cost. For spectroscopic anomalies observed by Struve and others in the Balmer lines of hydrogen can, of course, be caused only by *neutral* hydrogen present along the line of sight; but how much of the *total* hydrogen abundance can remain there in neutral state? The answer must be sought in Strömgren’s theory of H II regions, developed and applied extensively to gaseous nebulae and interstellar matter (for their latest presentations, cf., e.g., Osterbrock, 1974; or Spitzer, 1978). A direct transfer of the results presented in these books, and obtained for (say) planetary nebulae to binary systems is not possible, because of a great difference in certain parameters involved in such problems: in planetary nebulae the dimensions are large but densities low (and also the exciting stars hotter); while for circumstellar gas in close binary systems the opposite is the case; and these differences may amount to many orders of magnitude.

Only the first steps to investigate the physical properties prevalent in Strömgren zones of close binaries have so far been made (cf. Kopal, 1981). The results indicate, however, that the bulk of available hydrogen in (say) U Cephei can remain in neutral state only in the atmosphere of its primary component of B8 spectrum, where ample supply of free electrons (provided by ionization of light metals) makes hydrogen recombination practically instantaneous. However, between the two components, a diminishing density (including that of the electrons) will keep an increasing fraction of hydrogen ionized – so that the total amount of hydrogen present in between the stars (let alone outside the system) should be very much *larger* than that of neutral hydrogen alone. The mean free path of its constituents (different as it may be for neutral or charged particles) amounts, in turn, to so tiny a fraction of the flow scale that the absurdity of treating dynamical phenomena in such a medium in terms of particle mechanics – in which collisions are ignored and mean free path considered infinite – is glaringly evident. If so, it follows that the gas envelopes capable of impressing observable features in the composite spectra of such systems are no mere ‘exospheres’ whose particles can move in ballistic trajectories, but genuine ‘extended atmospheres’. Therefore it is hydrodynamics – rather than particle mechanics – to which we should appeal in our efforts to place a study of gas motions in close binary systems on an adequate physical basis.

That this must be so was realized already several years ago; and many investigations attempted to carry out such a programme (cf. Biermann, 1971; Prendergast and Taam, 1974; Sorensen *et al.*, 1975; Lubow and Shu, 1975; Budding, 1981; and others). If, in spite of protracted efforts, the progress in this field has been slow, this is due to inherent difficulties – yet to be overcome before any meaningful comparisons between theory and observations can be attempted.

Let us mention at least a few – if alone as tasks which we must eventually face and accomplish. First, spectroscopic observations indicate that (at least, the neutral component of) gas streams in close binary systems move with velocities of the order of 100 km s^{-1}; while the velocity of sound in hydrogen gas at a temperature of 10^4 deg (a typical ionization temperature, indicated by the spectra) should be close to 12 km s^{-1}. If so, it follows that the gas motions in question should be *hypersonic*, and be characterized by Mach numbers of the order of 10 (or more). This fact rules out, however, any possibility of *linearization* of hydrodynamic motions (inherent in most investigations quoted above); for if we did so, we would rule out possible formation of *shock waves*, which may indeed arise and play an important role in the spectra – especially the bow-shocks!

Secondly, we mentioned already that a considerable fraction of circumstellar gas in close binary systems may be *ionized*; and if so, the *viscosity* μ of the plasma (mainly hydrogen) component should be by several orders of magnitude higher than if the same gas were neutral (cf. Chapman, 1954; or Oster, 1957). As, moreover, the density ρ of the medium in question is probably quite low (10^{13} protons per cc would correspond to a plasma density of only 10^{-11} g cm^{-3}) its *kinematic viscosity* μ/ρ may become enormous; and the terms factored by it may dominate the respective equations of motion.

Add to this the fact that the Reynolds numbers of the flow prove to be so large that such motions are probably *turbulent* as well (and characterized by high turbulent viscosity into the bargain); and we find ourselves confronted with hypersonic flow in viscous turbulent media – about the worst accumulation of attributes one can ascribe to any flow! This flow should now be theoretically treated in three-dimensional space; with time constituting the fourth independent variable (which can be dispensed with only if the flow is steady). No wonder that not much headway has been made with the solution of the equations of motion (subject to appropriate boundary conditions) governing such flows!

Fortunately, this may not be really necessary for the main objective of our inquiry, which is to ascertain the cause of the 'evolutionary paradox' described in the preceding sections; and account for the ways in which evolving components of close binaries can dispense with their mass. The causes of such processes are probably *internal* to each star; and the mechanism of ejection, the 'stellar winds'. To this aspect of our problem we propose to turn in the next section; and to conclude the present we shall return to spectroscopic anomalies concerning asymmetric radial-velocity curves, mentioned already earlier in this section, as observed, e.g., by Carpenter or Struve in the system of U Cephei. For these investigators, the source of such anomalies was to be sought in gas streams between the stars. But whatever their motions, hydrogen in them should be largely ionized and incapable of absorbing in Balmer lines. To localize their origin, we should seek to identify regions where hydrogen can remain predominantly in neutral state; and this is, of course, the atmosphere of the star itself.

As is well known, the observed radial velocities of the stars are measured from the Doppler shifts of spectral lines formed in the atmospheres of the stars in question; and the use of such shifts as indicators of orbital motions entails a tacit assumption that *the* *atmospheric layers in which the measured spectral lines originate are at rest with respect to the star's centre of mass.* This assumption underlies indeed all work on the radial velocities of the stars carried out in the past; but regardless of what may be true if the star is single, can this continue to be the case in binary systems in which their components *irradiate* each other from close proximity?

As is well known, such an irradiation produces *heating* by many hundreds of degrees, which is bound to stir *thermal convection* in the respective atmosphere – steady-state gas motion if axial rotation of the irradiated star is synchronized with orbital motion of the illuminating source, but a non-steady one if this source rises and sets over the illuminated stars on account of asynchronism between rotation and revolution. It is interesting to recall, in this connection, that the principal components of both U Cep and RZ Sct – exhibiting strongly skew-symmetric radial-velocity curves in spite of their circular orbits – rotate much *faster* than they revolve (cf. Struve, 1949); and, therefore, experience conspicuous 'day-and-night' variations of temperature over their surfaces.

The effects of such an irradiation on the buoyancy of atmospheric gas, and the motions arising therefrom, cannot – we repeat – be ignored; the question remains yet only as to their efficiency. Can these represent the main, or contributory, cause of anomalous Doppler shifts exhibited by U Cephei, RZ Scuti and many other similar systems? In spite of some recent work by Kirbiyik and Smith (1976) or Kopal (1980), the quantitative answer is not yet known; but at least a possibility exists that this may be indeed the case.

5. Mass Loss

The principal objections to the mass-exchange scheme as outlined earlier in Section 3 are twofold: namely, the fact that the 'evolutionary paradox' raised in Section 2 is encountered, not only in close binary systems of sufficient mass for the effects of differential evolution to become noticeable in 10^7–10^8 yr; but in all types of binary system regardless of their mass or proximity (i.e., whether these are close or wide). Algol, as well as Sirius, appear to be equally prone to become the victims of it – possessing, as they do, evolved components of mass distinctly smaller than that of their Main-Sequence mates. A good part of the paradoxical nature of such a situation goes back, to be sure, to the restriction that *the systems in question evolve on constant mass;* and that the role of this evolution is limited to a mere *exchange* of mass overflowing from one star to another. We stressed, however, already that such a restriction is wholly arbitrary, and not required by the observations: in fact, the opposite is the case; and the aim of the present section will be to explain why in more detail.

In order to do so, let us first turn our attention to the observed predominance of contact components of low mass, and a virtual absence of systems in which the more massive is being caught in the process of expansion towards its Roche limits. This is equally true of massive systems (whose rate of evolution is sufficiently high to make such a scheme at least a theoretical possibility) as well as of binaries whose total mass is of the order of only one solar mass – in which the Kelvin time-scale of evolution may become much longer than that of nuclear evolution of more massive stars, and thus increase the probability of catching the low-mass systems in the elusive act of mass exchange; and observing in 'slow motion' what happens rapidly in those of greater mass. None were caught in it so far – regardless of whether their mass is large or small; and this is only bound to strenghten our previous doubts on whether such a metamorphosis of the components as envisaged by the mass-exchange scheme takes place at all!

Such doubts are further aggravated by the fact that – as is well known – a star of one solar mass requires not less than 10^{10} yr to exhaust enough hydrogen in its interior to evolve from the Main Sequence. And yet we know eclipsing systems – like V471 Tauri in the Hyades cluster – which (like its parent cluster) is no more than 600 million years old; and within this time the less massive component of this system of total mass of $1.4 \odot$ has already become a white dwarf!

This case (and other similar ones which could be adduced) represent a veritable *reductio ad absurdum* of the process of mass exchange between components which should keep the total mass of the system constant; and the final verdict on such a possibility cannot be too long delayed. But if further arguments are needed, let us recall that the same 'evolutionary paradox' (for the solution of which the whole process of mass-exchange was originally invented) exhibited by Algol-like close binary systems is likewise encountered – and in even more extreme form – in wide systems of the Sirius or Procyon type, in which a typical Main-Sequence star (of luminosity class V) is attended by a white dwarf. And these are no isolated examples; according to a recent catalogue by Agayev *et al.* (1982), over 60 visual binaries in our proximity are known to possess white-dwarfs as less massive components; and no doubt many more will be discovered in the future.

Consider, in particular, once more the system of Sirius whose physical properties are well known. At the present time its white dwarf component represents only 30% of the total mass of this system; 70% being stored in the Main-Sequence A0 star of luminosity class V. These two stars must have formed a dynamical partnership throughout all their past; and their ages be closely the same. If, however, the secondary component could have attained a white-dwarf stage while the primary still lingers on the Main Sequence, the conclusion is inevitable that the present Sirius B must have once been the more massive component of the two – which embarked on its evolution with a good deal more than the present mass of $2.3 \odot$ possessed by Sirius A – but *lost* most part of it at some stage of its subsequent evolution while the present A0 star still continued to linger on the Main Sequence. Indeed, this must have happened if Sirius B managed to pass the Chandrasekhar limit to become a white dwarf.

What had happened to this missing mass? In the case of Sirius (and many other such systems) the velocity of escape from the gravitational field of an evolving star is but marginally different from one which would enable this mass to escape altogether from the system; and the range of velocities for which mass particles could escape from the star but are gravitationally constrained to remain a part of the system (and thus provide material for potential accretion by its mate) is so narrow as to make any transfer of mass in this manner extremely inefficient. In other words, most part of the mass lost by the future white dwarf must have been lost to the system as a whole, and expelled into interstellar space.

What kind of a physical process could have led to such an expulsion? By 1955 – when the 'evolutionary paradox' began to be considered a significant scientific problem – little was known about the ways in which stars can divest themselves of mass: accretion, rather than loss, was then the main object of attention. Since the advent of the space-age in 1957, and a gradual opening of ultraviolet spectra of the stars, the situation began to change drastically. In the years past we learned much about 'stellar winds' and the sources of energy which produce and sustain them. The observations in recent years with the International Ultraviolet Explorer telescope led to a discovery that even seemingly 'normal' stars can expel matter through their coronae with a flux entailing a mass loss equivalent to 10^{-6} to 10^{-5} $\odot$ per annum; in the form of a plasma heated to several million degrees, and escaping with velocities between 10^3–10^4 km s^{-1} (cf., e.g., McCluskey and Kondo, 1981). Moreover, the outflow of this mass appears to be isotropic (cf. Kondo *et al.*, 1982) and barely affected by the proximity of any stellar companion.

Indeed, it begins to appear to us now that the phenomena, known previously (from the optical parts of the spectra) to be characteristic of the stars of the Wolf-Rayet type, may be much more common than had been thought hitherto; and need not be necessarily caused by binary nature of such objects. At least we do know of many single stars exhibiting similar characteristics as the subgiants in semi-detached binary systems, but unhampered by any Roche limits.

Should this mean that an approach to such limits in close binary systems has nothing to do with mass loss by such stars? Not necessarily so; for it is at least possible that an approach to such a limit may give rise to (or stimulate) sub-surface convection which, in turn, feeds energy into the maintenance of powerful stellar winds; the latter representing a sufficient mechanism for mass removal to meet all our needs.

The high velocity of such winds (generally in excess of 1000 km s^{-1}) can account naturally for the escape of the requisite amount of mass from the system per unit time; while the high temperature of the respective plasma (which may attain several million degrees) renders it well-nigh transparent at optical frequencies; and thus does not affect too much the light curves of close eclipsing systems observed through the optical window of our atmosphere or beyond – perhaps as far as the Lyman limit. The difficulties connected with a low-velocity, high-flux mass removal – set forth in the previous sections – completely disappear when hot stellar wind is invoked to replace neutral-gas streams; and the role of the Roche limit may appear to us in a completely different light – as stimulating convection rather than lowering the gravity in the first place (the two may, of course, be connected).

Have we, therefore, arrived at a stage at which we can at last say… 'and so it all ends happily'? Not by a long shot; for our present scheme too contains steps which are hypothetical, and likewise unproven by a more exact analysis. A conclusion that the mass lost by individual components in the course of their post-Main Sequence evolution escapes mostly from the system (and only very little of it can be transferred – let alone exchanged between them) is, in my opinion, so well-founded by existing observational data as to be virtually unanswerable; and the probability that this mass-removal occurs mainly through the medium of high-velocity stellar winds at high temperatures certainly to deserves serious consideration.

The existence in close binary systems of 'contact components' – i.e., of stars which fill the largest closed equipotentials capable of containing their mass – must, however, be likewise accepted as an established fact; but its physical significance remains still not clear. It may be that a sudden onset of convection and consequent stellar wind is indeed the cause of mass removal at the Roche limit; but this has not yet been proven and remains so far a hypothesis (albeit, to my mind, more likely than any other that had been proposed for the purpose). And we should also not lose sight of the fact that a large-scale mass loss must affect, at some stage, also the components of wide binary systems (like Sirius or Procyon) – pairs so wide that none of their components could approach (let alone attain) their respective Roche limits, even if the initial mass of the companion had been several times as large as it is at present*.

And this is, ladies and gentlemen, where we stand today. In grappling with problems mentioned as still open in this lecture, we should keep in mind that the principal reasons of this state is not only the complexity of the underlying equations of our problem (described in the preceding two sections of this paper), but mainly the fact that *the boundary conditions constraining their solutions cannot be specified from observations at our disposal sufficiently to render the solutions of such equations unique*. In order to obtain theoretical models which can be compared with the observations, the deficiency in observed facts must, therefore, be supplemented by intuition of the investigator to fill the gaps; and this where we can find ourselves on very slippery ground.

To give an example of such a situation, consider the case of a 'mass-exchange' mentioned earlier in Section 3 of this address. Even in its simplest physical form (in which the respective gas flow is replaced by a stream of particles which do not interact with each other), the underlying physical problem can be mathematically expressed in terms of three second-order differential equations of the restricted problem of three bodies, governing the positions $x(t)$, $y(t)$, $z(t)$ of such particles as a function of the time t. A unique specification of the motions of particles in three-dimensional space calls, therefore, for a knowledge of three initial positions x, y, z as well as of their velocities $\dot{x}$, $\dot{y}$, $\dot{z}$. If we identify the xy-plane with one tangent to the celestial sphere, and the z-direction with that of the line of sight, the photometric as well as spectroscopic observations can provide some empirical knowledge of the positions x and y, and of the velocity component $\dot{z}$, but cannot furnish any information on the remaining initial values of z, $\dot{x}$, and $\dot{y}$. In order to integrate actual trajectories, these remaining initial

---

* This follows from a well-known integral of the two-body problem with variable mass (cf. Hadjidemetriou, 1963), asserting that if the loss of mass is isotropic (which it will be if it occurs at high speed), a product *of the semi-latus rectum of relative orbit and the total mass of the system must be conserved*. The present semi-major axis of the relative orbit of Sirius is known to be 20.1 astr. units. If, therefore, the mass of the present white dwarf ($0.98 \odot$) was initially (say) 5 times as large – rendering the initial total mass of the system to have been close to $7 \odot$ rather than the present $3 \odot$, this would have reduced the present size of the orbit only to 8.6 AU = 1850 solar radii – still rendering the Roche limits of the (then) more massive star very much larger than a star of mass $5 \odot$ could ever hope to attain in the course of its evolution.

conditions must, therefore, be only guessed at; and the outcome then depends, of course, wholly on the correctness of such a guess. In other words, by choosing these in a suitable manner, we can transfer a mass particle from anywhere to anywhere within the system, and with an arbitrary velocity.

But need the results so obtained have anything to do with reality? Astronomers working on this part of our vineyard in the past would, perhaps, have been less than human if they did not at least try to explore the consequences of such arbitrary assumptions; the trouble only arises if they mistake their assumptions for established facts. To do so may actually become counterproductive, and inhibit further advances rather than contribute to them.

Worse comes, moreover, when such assumptions begin to resonate; and are accepted as truth because somebody else said so before (for 'they cannot all be wrong'!). 'A definite study of the herd instinct of astronomers is yet to be written' remarked recently Fernie (1969) on a similar situation elsewhere in astronomy, "but there are times when we resemble nothing so much as a herd of antelope, heads down in tight parallel formation, thundering with firm determination in a particular direction across the plain. At a given signal from the leader, we whirl about and, with equally firm determination, thunder off in quite a different direction, still in a tight parallel formation." A not very complimentary picture, perhaps, but containing more than a grain of truth. Did we indeed spend too much time in the past running with our heads down, and not thinking enough en route as we thunder across the plains?

Or – to quote another caustic comment which the late Oliver Heaviside (creator of the operational calculus) once made on a similar occasion – "almost everyone agrees with this hypothesis; and, therefore, the hypothesis is almost certainly wrong"; by which he meant that Nature but very seldom discloses her secrets at a first try; and that meticulous attention to every detail of the problem is necessary to force their eventual surrender.

More specific comments on such a situation have already been made (cf., e.g., Kopal, 1978, p. 472; or 1981, pp. 555–557), and need not be repeated at this time. However, the foregoing general remarks should make it perhaps abundantly clear that, in grappling with our current problems, we are still far from being out of the woods. But in the course of this time we have learned to get better acquainted with what remains yet to be done, and to learn from our past mistakes to avoid new pitfalls. Above all, let us strive not to be caught by posterity in practising a 'Procrustean science' – in which by emphasis and omission, disregard of unpleasant facts or accumulation of superfluous hypotheses, we not only may be trying to fit known facts on to the Bed of Procrustes of our preconceived opinions, but also offend the wisdom of 'Occam's razor' that 'entia non sunt multiplicanda praeter necessitatem'. And – above all – whenever our tentative conclusions may (as they did so many times in the past) turn out to be false, let us keep in mind that a quest for truth in science is seldom a monotonous process; and in the case of any disappointment, remember the words which William Shakespeare had Cassius say in his tragedy on Julius Caesar: 'The fault, dear Brutus, lies not in our stars, but in ourselves.'

6. Conclusions

Having progressed so far in our narrative, let us attempt to summarize the present – and not necessarily final – state of our subject in the following terms:

(1) In order to account for the phenomena exhibited by binary stars in terms of presently accepted theories of nuclear evolution, it is necessary to postulate a large-scale loss of mass by their components in post-Main-Sequence stage. Moreover, such a loss must be suffered by all types of binaries – close as well as wide – by systems like Algol as well as Sirius; and no doubt its cause affects all stars – be these single or multiple; in binaries it becomes merely more evident and impossible to ignore.

(2) The mass lost is very probably carried away at high speed – far above the velocity of escape from the gravitational field of the system – essentially isotropically; and only that part of it may be transferred from one component to another as is intercepted by the target surface.

(3) Most of this mass is probably expelled in the form of 'stellar winds' of hydrogen-helium plasma, at velocities between 10^3–10^4 km s^{-1}, and temperatures in the range of 10^6–10^7 degrees. It is this temperature that renders this plasma essentially transparent in the optical domain of the spectrum. The velocity with which the star as a whole may expand towards the Roche limit need not have anything in common with the velocity of ejection beyond this limit by stellar winds – the two may differ by many orders of magnitude.

(4) The energy necessary to raise the flux of corpuscular radiation to the level sufficient for the primary (more massive) and secondary (less massive) components to reverse their roles stems – like for single stars – probably from sub-surface convection. Whether or not an approach of the star's surface to its Roche limit in close binary systems stimulates convection to this extent is as yet uncertain, but remains a distinct possibility. It must, however, be also kept in mind that a mass loss of the same order of magnitude is likely to occur also in wide binaries – like Sirius or Procyon – whose components could scarcely have attained their Roche limits even at the maximum state of expansion permitted by the mirror-effect.

(5) Spectroscopic anomalies which have been observed at optical wavelengths are more likely produced by gas-streams in stellar atmospheres – arising from mutual irradiation of the two stars – rather than by circumstellar gas; for hydrogen in the latter medium should be largely ionized, and incapable of absorption in Balmer lines.

The total amount of gas in between the stars – ionized as well as neutral – may become observable only if its density attains the values at which the mean free path of the constituent particles becomes smaller by many orders of magnitude than the dimensions of the respective system. But if so, its motions must be governed by the laws of hydrodynamics rather than particle mechanics; and the kinematic viscosity of its ionized component (larger by several orders of magnitude than that which the same gas would possess in neutral state) may become so large as to necessitate retention of the Navier–Stokes terms in the respective equations of motion for an adequate description of the reality.

Moreover, since the motion of gas relative to the stars which are embedded in it is likely to be hypersonic (characterized by Mach numbers of the order of 10 or more), shock-wave phenomena – in particular, the bow-shocks in front of the leading hemisphere of less massive components – should give rise to phenomena which may be spectroscopically observable.

(6) If the hydrodynamical phenomena likely to occur in close binary systems remain still but incompletely investigated from the theoretical point of view, the principal reason is not only the complexity of fundamental equations which should control such phenomena, but – above all – the fact that the boundary conditions which constrain the solutions of such equations are *not* specified by the observations sufficiently to render such solutions *unique*. And as long as this is the case, it must be kept in mind that the scenarios postulating various 'shells', 'rings', 'accretion discs', etc. invoked by many less patient confrères to account for the few observed facts at our disposal are wholly dependent on the assumptions by which our intuition (or imagination) must supplement these facts to make any comparison between theory and observations possible at all.

That we often do this is, of course, inevitable; for this is how science advances in the long run. But, in doing so, we should not proclaim such contraptions as gospel truth. Indeed, we must constantly keep in mind that the scenarios so conjured up remain constructions of our own making until the underlying assumptions can be verified by independent evidence, and shown to be consistent with basic physics. And always keep in the back of your mind the unpleasant thought that while any (finite) number of observed facts which may be in agreement with a given hypothesis does not yet establish its validity (they can only increase its probability), a single well-established fact which is definitely at variance with such a hypothesis is sufficient to disprove it!

References

Agayev, A. G., Guseinov, O. H., and Novruzova, H. I.: 1982, *Astrophys. Space Sci.* **81**, 5.
Barney, I. W.: 1923, *Astron. J.* **35**, 95.
Batten, A. H.: 1974, *Publ. Dominion Astrophys. Obs.* **14**, 191.
Bělopolsky, A. A.: 1893, *Mem. Soc. Spettr. Ital.* **22**, 101.
Bělopolsky, A. A.: 1897, *Mem. Soc. Spettr. Ital.* **26**, 135.
Biermann, P.: 1971, *Astron. Astrophys.* **10**, 205.
Budding, E.: 1981, in F. D. Kahn (ed.), *Investigating the Universe*, D. Reidel Publ. Co., Dordrecht, Holland, p. 271.
Carpenter, F. M.: 1930, *Astrophys. J.* **72**, 205.
Chandrasekhar, S.: 1944, *Astrophys. J.* **99**, 54.
Chapman, S.: 1954, *Astrophys. J.* **120**, 151.
Crawford, J. A.: 1955, *Astrophys. J.* **121**, 71.
Fernie, J. D.: 1969, *Publ. Astron. Soc. Pacific* **81**, 707.
Gould, N. L.: 1957, *Publ. Astron. Soc. Pacific* **69**, 541.
Gould, N. L.: 1959, *Astron. J.* **64**, 136.
Hadjidemetriou, J.: 1963, *Icarus* **2**, 440.
Hoyle, F.: 1955, *Frontiers of Astronomy*, Heinemann, London, p. 195.
Kirbiyik, H. and Smith, R. C.: 1976, *Monthly Notices Roy. Astron. Soc.* **176**, 103.
Kondo, Y., Feibelman, W. A., and West, D. K.: 1982, *Astrophys. J.* **252**, 208.
Kopal, Z.: 1954, *Mém. Roy. Soc. Liège* **15**(4), 684.
Kopal, Z.: 1955, *Ann. Astrophys.* **18**, 375.

Kopal, Z.: 1956, *Ann. Astrophys.* **19**, 298.
Kopal, Z.: 1957, in G. H. Herbig (ed.), 'Non-Stable Stars', *IAU Symp.* **3**, 123ff.
Kopal, Z.: 1959, *Close Binary Systems*, Chapman-Hall and John Wiley, London and New York, Chapter VII.
Kopal, Z.: 1971, *Publ. Astron. Soc. Pacific* **83**, 521.
Kopal, Z.: 1978, *Dynamics of Close Binary Systems*, D. Reidel Publ. Co., Dordrecht, Holland, p. 10.
Kopal, Z.: 1980, *Astrophys. Space Sci.* **70**, 329; **71**, 65.
Kopal, Z.: 1981, in E. B. Carling and Z. Kopal (eds.), *Photometric and Spectroscopic Binary Systems*, D. Reidel Publ. Co., Dordrecht, Holland, p. 535.
Lubow, S. H. and Shu, F. H.: 1975, *Astrophys. J.* **198**, 383.
McCluskey, G. E. and Kondo, Y.: 1981, *Astrophys. J.* **246**, 464.
Morton, D. C.: 1960, *Astrophys. J.* **132**, 146.
Neubauer, F. J. and Struve, O.: 1945, *Astrophys. J.* **101**, 240.
Oster, L.: 1957, *Z. Astrophys.* **42**, 228.
Osterbrock, D. E.: 1974, *Astrophysics of Gaseous Nebulae*, Freeman and Co., San Francisco.
Paczynski, B.: 1971, *Ann. Rev. Astron. Astrophys.* **9**, 183.
Plavec, M.: 1968, *Adv. Astron. Astrophys.* **6**, 202.
Plavec, M. and Kříž, S.: 1965, *Bull. Astron. Inst. Czech.* **16**, 297.
Poincaré, H.: 1896, *Calcul des Probabilités*, Paris, p. 149.
Prendergast, K. H. and Taam, R. E.: 1974, *Astrophys. J.* **189**, 125.
Schoenberg, M. and Chandrasekhar, S.: 1942, *Astrophys. J.* **96**, 161.
Smak, J.: 1962, *Acta Astron.* **12**, 28.
Sorensen, S. A., Matsuda, T., and Sakurai, T.: 1975, *Astrophys. Space Sci.* **33**, 465.
Spitzer, L.: 1978, *Physical Processes in Interstellar Medium*, John Wiley, New York.
Struve, O.: 1944, *Astrophys. J.* **99**, 222.
Struve, O.: 1949, *Monthly Notices Roy. Astron. Soc.* **109**, 487.
Struve, O.: 1950, *Stellar Evolution*, Princeton Univ. Press, London.

Prendergast, K. H. and Taam, R. E.: 1974, *Astrophys. J.* **189**, 125.
Schoenberg, M. and Chandrasekhar, S.: 1942, *Astrophys. J.* **96**, 161.
Smak, J.: 1962, *Acta Astron.* **12**, 28.
Sorensen, S. A., Matsuda, T., and Sakurai, T.: 1975, *Astrophys. Space Sci.* **33**, 465.
Spitzer, L.: 1978, *Physical Processes in Interstellar Medium*, John Wiley, New York.
Struve, O.: 1944, *Astrophys. J.* **99**, 222.
Struve, O.: 1949, *Monthly Notices Roy. Astron. Soc.* **109**, 487.
Struve, O.: 1950, *Stellar Evolution*, Princeton Univ. Press, London.