ALGOLS IN VICTORIA, 1988

Frank Bradshaw Wood
Department of Astronomy
University of Florida
Gainesville, Florida 32611
U.S.A.

The chief purposes of this colloquium are to look at the present and (especially) the future state of what the study of Algol type binaries can add to our knowledge of the exciting new astrophysical developments which are occurring with almost overwhelming speed at the present. Therefore, this paper recalls some of the past chiefly in order to set the stage. It will be both general and brief.

A few years ago, Jorge Sahade and I produced a little book (Interacting binary Stars), designed chiefly to spread knowledge of some of the work on eclipsing stars in general to astronomers not working in the field and who in some cases, seemed to be completely unaware of the broader implications being revealed by current work (Sahade and F.B. Wood; 1978 Pergamon Press). In this we arbitrarily divided the work into four general epochs which I will repeat briefly here. These present meetings may well be regarded by future historians as marking our entry into yet a fifth.

The first, as far as systematic study is concerned, goes back to the discovery of the variation of Algol by John Goodricke in 1783 (Phil. Trans. Roy. Soc. London, **71**, 474) and his attribution of the changes either to eclipses by some dark object or (interesting in view of recent developments) the existence of large spotted areas on at least one of the components. In this era, the observations were visual and photographic and almost - not quite - always "estimates" of the stars' brightness as it appeared in the telescope or of its image on a photographic plate. In only a few cases were "measures" available - visually using the polarizing or wedge photometer and photographically by measuring either the size or the density of the star's image. Although relatively crude, these served at least two purposes - they called our attention to some of the unusual cases which merited further attention and they gave us times of minima (which grew increasingly important as time passed) to aid in future studies of period or period changes, since no amount of later observations, no matter what their precision, could tell us what the system was doing at an earlier epoch. This was of importance even when the chief purpose was merely to determine the period more accurately but became of greatly increasing importance when it was recognized that period changes of various sorts

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occur in many systems. (The possibility of ancient knowledge of Algol's variability has most recently been discussed by E. Budding in Southern Stars, **32**, 180, 1988.)

Theoretical treatment of the light curve was extremely difficult and at least one authority stated (in print) that the relation between the elements of the system and the light changes was so complex that no general solution was possible - a statement which H.N. Russell was delighted to quote when he wrote the first of his theoretical papers which so greatly transformed the field. The paper appeared at just about the time when photographic measures were replacing estimates and when a few exceedingly energetic visual observers were doing the same thing by use of polarizing and wedge photometers. Toward the end of this era, a very few photoelectric photometers (with photosensitive surfaces in general which only one man could produce) appeared; the astronomers who could actually make a photoelectric photometer were sometimes regarded as wizards by their contemporaries.

We can then base our treatment of the subject on the first epoch as running from Goodricke to the theoretical papers by Russell (H.N. Russell, Astrophys.J. **35**, 1912); the second beginning with Russell's work showed an increase in the number of astronomers in the field and a marked increase in the number of "solutions" in which the relative sizes and separations of the components could be computed, and the inclination of their orbital planes determined. When spectrographic orbits were available, the "absolute dimensions" - sizes, masses and hence densities in terms of solar units could be computed. Optimism as to the precision of these determinations ran high - we can even find published values giving them to the third decimal place. Not all astronomers agreed with the statement that "only a little geometry" was needed to determine these elements to a precision set only by the accuracy of the observations. However, the efforts to produce more and more precise light curves was continued by an increasing number of observers. The key test was the scatter of the observations. The competition to get the smallest probable error of the individual observation from the theoretical curve was very strong. This led in some cases to the addition to the observations of "night errors" or "seasonal errors" or in some cases even "hour angle" measures - as distinct from normal extinction corrections. The precise amount of these - or even the fact that they had been added - was frequently not mentioned and so the published light curves often appeared more accurate than they in fact were. More seriously, the belief that all light curves should repeat uniformly from epoch to epoch and from season to season may well have caused a smoothing out of the real variations which can and do occur in many interacting systems. (I remember one astronomical meeting, in which I suggested mass loss as a possible cause for changes in orbital period, where early in the discussion one member of the audience announced emphatically that, if there was one thing out of which we could be certain, it was that no star could ever lose mass. This statement would scarcely attract much support today).

There were, however, a few indications of change. One early one was Kuiper's (G.P. Kuiper) paper on β Lyrae (Astrophys.J. **93**, 133, 1941). In this, Kuiper suggested a common envelope for the components and introduced the concept of "contact binaries". Much later Wilson (Astrophys.J. **234**, 1054, 1979) called attention to another type of close binary and introduced the term "double contact".

Kuiper's paper did not attract a great deal of attention. Possibly this was due to the fact that at the time of its appearance, many astronomers were engaged in non-astronomical topics. However, even later, (F.B. Wood - Princeton Contr. No. **21**, 31, 1946) when the Jacobian limiting surface (now frequently called the Roche model or better the zero-velocity surface) was used to reach a limiting solution for R Canis Majoris by using it to set maximum possible sizes for one of the components for various mass ratios, little general attention was given and the same was true later when it was pointed out that with one exception - and that one a notorious maverick - systems showing sudden apparently erratic period changes were those in which one component approached these limits, (F.B. Wood, Astrophys.J. **112**, 196, 1946). The change from the second to the third or modern epoch seems to have been chiefly caused by instrumental advances, chiefly the introduction of the multiplier photocell. The greatly increased sensitivity essentially made big telescopes out of little ones, at least as far as photoelectric photometry was concerned. This of course was also the precursor of the explosion of observational work to exploit almost all regions of the spectrum from γ-rays to radio waves.

An excellent summary of the development of our current idea in this third epoch has been given by Budding (E. Budding, Southern Stars **31**, 125, 1987). Budding also gives a representative list of advances in observational coverage in this interval of inquiry from the 1.6 micron light curve of Algol by Chen and Reuning (Astr. J. **71**, 283, 1966) to the discovery of the eclipse polarization effect by Kemp et al. (Astrophys.J. **273**, L85, 1983). Advances and extensions since then have been too numerous even to mention in a short presentation. However, Mirek Plavec asked me to mention some of my own contributions, or attempted contributions to the field. I must confess that in the first era, (1783-1912), I contributed absolutely nothing. The same was true for most of the second. However, toward the end of it, I did begin observation with the polarizing photometer and my dissertation in 1941 did contain three systems thus observed (one or two had been the number previously accepted for a Ph.D.) and discussion of them on the Russell model as was then customary. However it also contained one system observed by photographic measures, and five by photoelectric observations made with the standard Observatory 36-inch reflector and a photometer designed and built by Franklin Roach. I am not certain, but I think that this was close to a doubling of the photoelectric light curves available to that date.

My chief later contributions were the concept of the "Jacobian" (or Roche) (zero-velocity) surfaces, studies of period changes, the introduction of multi-color observations as in ζ Aurigae, and the commencement of "campaigns" of international co-operation, the production of a number of excellent graduate students, the maintenance of the catalogue of eclipsing binaries since 1950 and the production of "Finding Lists" from them, the establishment of the Flower and Cook

Observatory at Pennsylvania, help in various ways in the establishment of an active observatory in the Southern Hemisphere and in the establishment of an automated telescope now in operation at the South Pole and the encouragement of amateurs in observing and publishing times of minima and aiding in international collaboration whenever I could. I have also managed to contribute a good many photoelectric light curves and solutions.

The complex and expanding work in recent years, treating the stars as active and evolving objects instead of merely disks being eclipsed has been well summarized by Budding in an excellent paper in Southern Stars (already cited) and need not be repeated here.

Finally, this Colloquium and the ideas generated here may well be the beginning of yet another era with exciting suggestions and innovations which may lead who knows where. For a possible view of the fifth era and an indication of what it may bring, we now turn to the remaining papers in this symposium.