MASSES, RADII AND LUMINOSITIES FROM ANALYSIS OF ECLIPSING BINARIES

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ABSTRACT. The reliability of values of the fundamental properties of stars derived from eclipsing binary analysis is discussed in terms of general concepts. Since the principles involved in the determination of masses and radii are simple, the heart of the matter is the care and judgment with which the relevant spectrographic and photometric observations are obtained and analyzed. Problems in the evaluation of the radiative properties require special attention.

1. INTRODUCTION

My task is to give you my views on the requirements eclipsing binary systems must satisfy in order to qualify for consideration as standards of reliability of masses, radii, and luminosities. Those views can be stated very simply. The systems must be favorable ones, both spectroscopically and photometrically, the observations must be of high quality, and their analysis carried out with care, understanding, and good judgment. For details, read my papers. That is really the essence of what I have to say, but I'm afraid more is expected of me. Since the concepts I have just stated are elementary, their elaboration is also pretty elementary, for which I apologize.

The topic may be discussed under four headings: Principles, observations and their analysis, results, and choice of stars as standards or calibrators.

2. PRINCIPLES

The basic principles involved in the direct determination of the masses and radii of the components of double-lined eclipsing binaries have been well established for a very long time, namely the dynamics of two-body motion and the geometry of eclipses of limb-darkened stellar discs. The most important complications are gravity brightening and mutual irradiation in the light curves, for which the principles are somewhat less well established. In the most favorable systems - well-separated, nearly spherical stars - these effects are unimportant,

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but can become of great importance in some semi-detached systems and all contact systems. Insofar as luminosities are concerned, the principle is that the surface flux correlates with other radiative properties of a star (e.g., color index) directly obtainable from observations. Radius and surface flux give the luminosity.

The only really new approach introduced into the field in recent decades has been the use of computers for light curve analysis. I mention it here because of its importance, although no new principles are involved, and discussion belongs under analysis of observations.

In making the comment that there have been no new approaches, I am limiting the discussion to the classical problem. Close binary systems with degenerate components - X-ray binaries, binary pulsars, cataclysmic variables - do require and have brought forth new concepts and assumptions (Bahcall 1978; Robinson 1976), as well as their associated sources of uncertainty.

3. ANALYSIS OF OBSERVATIONS

In discussing problems of analyzing spectrographic and photometric data, I shall limit myself primarily to the simplest cases of well-detached systems. It is these that provide the best results, of the highest accuracy, freest of assumptions, and presumably most suitable as standards or calibrators. I use the word "presumably," since, before coming to this meeting, at any rate, I have not had a clear understanding of the concept of calibration of quantities more than one or two steps removed from observation. I'll return to this matter briefly under the choice of stars as standards.

It is, of course, essential in any evaluations based on observational data that careful attention be paid to the determinacy of the results. This is particularly the case for results under consideration as standards. The matter of determinacy or reliability is at the heart of the matter. I presume that I am standing before you because, in my work, I have tried to pay particular attention to those aspects of the observations and their analysis that can, if sufficient care is not taken, lead to untrustworthy results.

I'm afraid much of what I have to say may appear critical or even negative. But the very essence of decision making with respect to standards is critical examination of details. If all details of the observations and their analysis are completely satisfactory, there is little to be said. The more they are unsatisfactory, the more comments are required to show what it is that needs to be improved. It's like refereeing a paper - the better the paper, the shorter the referee's report. Another way of looking at the matter is to point out that there are many more unsatisfactory methods of observation and analysis than satisfactory ones.

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3.1. Spectrographic Considerations

One of my first motivations in looking into problems of close binaries was to try to understand some outstanding discrepancies. In particular, the dissertation of A. B. Wyse (1934) at the Lick Observatory had emphasized differences between the surface flux ratios for a number of binaries evaluated, on the one hand, from the published spectral types and, on the other, from the depths of the two eclipses. As the opportunity became available to me to accumulate photometric and, primarily, spectrographic material to try to understand the problem, it gradually became apparent that the difficulty lay principally in the assignment of the spectral types of the fainter components. It is difficult, at best, to estimate a trustworthy type from weak lines. It is exceptionally difficult if the lines of the seconary are, in fact, too weak to be visible. After all, if a star is known to be a spectroscopic or eclipsing binary, it has ipso facto two components. Moreover, if the period and epoch are known, one knows about where to look for the lines of the second component. It is certain that if one doesn't look for the lines of the secondary, he'll not see them. By "looking" I mean inspection of photographic spectra; study of microdensitometer scans of a plate; examination of the traces on an oscilloscope screen; observation of the output of a Griffin-type or Coravel radial velocity device; analysis of the results of cross-correlating the digital output of a reticon, digicon, CCD, or whatever with that of a standard star, etc., etc. The principles are the same.

There are several possible outcomes of the looking process. 1) The lines of the secondary are not said to be detected. 2) The lines of the secondary are not detected, but are mistakenly thought to be. 3) The lines of the secondary are really detected, but they give systematically incorrect velocities because they are blended with lines of the primary. 4) The lines, or at least some of them, of the secondary are detected and are not blended, so that usable orbits can result, and perhaps spectral types as well. The outcome depends, of course, not only on the true nature of the system and the care of the observer, but also on the resolution and signal/noise of the observations, and the region of the spectrum examined. Only in the fourth case are the results useful. The dividing line between conditions 3) and 4) may not always be clear. Special problems arise in O-B systems where, as demonstrated by Andersen (1975), even apparently well-resolved lines can have overlapping damping wings, giving rise to systematic effects.

On studying both old material and new material of my own, it became evident that the source of the discrepancies in surface flux ratios noted by Wyse was the assignment of spectral types to components falling in one of the categories 2) or 3). Once this situation was realized, it naturally caused one to wonder how securely the masses and radii were established. I must add that the problem of ascribing orbits - and masses and radii - on the basis of inadequately resolved or of nonexistent lines has not completely disappeared from the literature. As a consequence of such considerations, I have taken the position that I must be personally satisfied, from examining my own material or that of others, as to the reality and resolvability of component lines before I consider a system to be satisfactory for the purposes we are discussing.

In that earlier era (before about 1950), the quality of the spectrographic material available was inadequate except for a relatively small number of systems. A fundamental advance was the introduction at some institutions of efficient grating spectrographs with greater spectral resolution and broader spectral coverage, along with more sensitive photographic emulsions. The broader spectral coverage allowed one, for example, to observe at longer wavelengths, where the lines of a cooler secondary component would be enhanced. It was these advances that led to most of the results now considered to be of high quality. The use of oscilloscopic scanning devices has resulted in more efficient measurement of spectrograms.

We are currently in another period of improving spectroscopic technology - digicons, reticons, CCD's, micro-channel plates, radial-velocity meters, cross-correlation techniques, and presumably more to come. The most effective ways of applying these devices to binary star problems is a matter of concern. Perhaps the most striking contribution to date is their ability to obtain results for systems with resolved lines of greater magnitude difference between components than heretofore. But use of the new generation of detectors is no guarantee that the pitfalls referred to earlier will automatically be avoided. And it is not outside the realm of possibility that new and as yet unexpected effects may arise in these advanced techniques that will require special care if they are to be overcome. One new kind of effect that I wonder about is in data handling and reduction. In the old days one <u>looked</u> at his plates, <u>measured</u> them, with his eyes as an essential tool, and perhaps even <u>plotted the results by hand</u> to see what they looked like. Now, with most output in digital form, the tendency is increasingly to feed the observations into a computer and, without having to examine any intermediate steps, to accept the results. I have seen just enough of this procedure in my home environment to realize that the fact that a computer program has been used by many people does not guarantee that it is free of errors that can degrade results. Let me give an example of what I consider to be inadequate data handling. Spectrographic orbits are often published in which the velocities of the two components are combined into a single solution, without solving the two separately. A bad example of what this generally unwise practice can lead to may be seen in a paper soon to appear on V624 Her, a bright, double-lined Am binary (Popper 1984). The old prismatic velocities have been analyzed over the years by three astronomers: the spectroscopic observer, a photometric observer, and a third person. All used the same questionable procedure, and none of them noted that the residuals for the two components differed systematically by 7 km s^{-1}! The reason for the difference is not important in the context of this discussion. Its having been missed is a consequence of poor procedures and of not looking at the results. I have given (1974) a long list of reasons why it is unwise to assume equal systemic velocities for the two components except in special circumstances, and will not repeat it here. To this list may be added the consequences of the cross-correlation technique when the two components differ sufficiently in type that different standards of velocity are used for the two components. The difference in the adopted velocities for the two standards should not be assumed to be precisely equal to the true difference.

3.2. Photometric Considerations

These comments have thus far had to do primarily with the determination of spectroscopic orbits. In some respects the situation is parallel for photometric observations. The great observational advance came, of course, with the replacement of the photographic plate and visual photometer by the photomultiplier, at about the same time as the introduction of efficient grating spectrographs.

A second great advance in photometric studies of eclipsing binaries came with the availability of high-speed, high-capacity digital computers. They are a great help in reducing the observations, though here also one should not underestimate the advantages of looking at the results at various stages of the reduction procedures. But it is in analyzing light curves that the computer's influence is vastly greater than in the spectrographic case. The spectrographic orbit has a simple analytic form, and it makes no difference in the results whether one makes use of a hand-crank desk calculator or a state-of-the-art computer. But the computer has revolutionized the analysis of photometric observations of eclipsing binaries. There being essentially no analytic relations between the observed quantity and the results to be extracted, the only effective procedure is model fitting, involving many numerical integrations. In the simplest cases of spherical or nearly spherical stars, and with 75% or more of each star covered at mid-eclipse, which are just those cases that can give the most determinate masses and radii, the details of the computational procedure may not be critical, but use of a satisfactory computer program leads, in such cases, to objective results and to rational evaluation of their uncertainties. This matter of the non-critical nature of the method of analysis in simple cases is not to say that there may not be pitfalls for the unwary in applying someone's computer program uncritically.

In more distorted systems, both the increase in the number of parameters defining the system and some uncertainties in the model ("reflection," gravity brightening) can cause a solution of the light curve to be less secure. Similarly, systems in which smaller fractions of the stars are covered will give less determinate solutions. Systems in these categories should probably be avoided as standards or calibrators of fundamental stellar properties, although the results may be important for studying specific problems. With respect to reflection and gravity effects, I find a tendency among users of computer programs to assume that the computer model is without uncertainties, so that derived quantities, including mass ratios, are subject only to observational uncertainties. It is my opinion that the modeling of some of these variations is not completely secure, and that one often needs to consider potential effects on the derived results of uncertainties in the models.

Perhaps the greatest potential pitfall is to have too much confidence in a formal photometric solution that is derived from inadequate data or for a system that is intrinsically indeterminate. Use of a computer cannot, despite its great powers and despite heroic efforts of investigators, make determinate that which is not. As is well known, determination of the light ratio of the components from the line spectrum can provide additional information that can rescue an otherwise indeterminate photometric analysis. Use of modern spectral detectors, with linear response and large signal/noise, should improve greatly our ability to determine ratios of line strengths. If the two components differ in temperature and/or surface gravity, the differential use of model atmospheres may be capable of converting ratios of line strengths to a light ratio. I would feel more comfortable about the use of model atmospheres in this problem if there were a body of empirical equivalent widths for a wide range of temperatures and gravities that were found to be in agreement with predictions from model atmospheres. The use of stars in a cluster for such an empirical test would eliminate the uncertainties due to differences in composition, and the differences in gravity and temperature might be placed on a reasonably sound basis.

I have referred to the use of computers in both spectrographic and photometric analyses. Programs have also been developed and employed for combining the two kinds of analysis into one grand program (e.g., Wilson 1979). It is my opinion that only in special circumstance is the use of such a program desirable. The natures of the two kinds of data and of the two kinds of analysis are so different, each with its own special problems and idiosyncracies, that one should always look at each separately to see if everything is consistent. Let me illustrate my point by a couple of examples. A preprint I saw recently made use of a combined program. One assumption made was that the phasing (e.g., epoch of conjunction) was the same for both spectrographic and photometric observations. That is not necessarily a safe assumption if the two kinds of observation were obtained at different epochs because of uncertainties or changes in the period. In the same preprint, the photometric and spectrographic observations of a well-detached system with spherical stars were combined to derive the mass ratio. The authors were surprised to find that the inclusion of photometric data caused a slight change in the mass ratio from the value obtained from the radial velocities alone. They concluded that there was some subtle information hidden in the photometry contributing to the evaluation of the mass ratio. A glance at the photometric observations showed what this "information" was. One deviant point in the light outside eclipses, probably a poor observation, forced the computer to conclude that there was the equivalent of a non-zero coefficient of the cos 2θ term in the light variation outside eclipses - ergo, a mass ratio is evaluated. Such inappropriate mixing of data can readily cause a degradation of the derived results. This is also an example of how necessary it is to examine observations carefully. There are situations where a combined program may be appropriate, for example, in eccentric orbits where significant information on the eccentricity may be contained in both kinds of observation, although separate solutions should always be carried out as well to test for consistency. Another useful case is the combination of photometric observations in two or more wavelength bands, with the assumption of common geometry, although here also separate solutions can reveal unexpected effects.

In both spectrographic and photometric analysis it is essential, if the results are to be taken seriously, that realistic uncertainties of the important quantities be evaluated. This is perhaps the most personal aspect of all, depending on the investigator's judgment as well as on his powers of analysis, so that an objective comparison of the results by different investigators may be difficult to carry out. If the spectrum lines of the components are well resolved in the available material, the formal mean errors of the spectrographic elements may be accepted as realistic. The situation is not so clear in the case of a photometric orbit, where the interplay between the elements that produce the light variation may be complex, subtle, and not simple to understand. Differences between the results by different investigators of the same system show that formal mean errors based on internal agreement tend to be overly optimistic. One needs to be particularly cautious in accepting the formal mean errors that are produced when certain parameters are kept fixed and others allowed to vary. In my own work I have attempted not to underestimate the various sources of error, particularly in photometric solutions, but it is difficult to form objective judgments.

3.3. Radiative Properties

Thus far in our discussion, the focus has been on solutions of spectrographic and photometric orbits, primarily for the purpose of obtaining masses and radii. The radiative properties of the stars are also required. Without specifying them, we don't have a picture of the kind of star we are talking about. They are also required, given the radius of a star, to calculate its luminosity. In evaluating the fundamental properties of stars in binary systems of all kinds, eclipsing, visual, and resolved spectroscopic, I have found the specification of radiative properties the most consistently vexing and difficult of all problems.

The various interrelated radiative properties are color indices, surface fluxes, and effective temperatures. One might also include spectral types.

The flux and temperature scales are discussed by others at this symposium, and I may have comments on their presentations. The flux and temperature scales are usually calibrated in terms of color

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indices, so one observational problem is to obtain the color indices of the two components. It is essential for this purpose that the photometric observations be carried out on one of the best calibrated photometric systems in two or more wavelength bands unless the components are very similar; in that case, the photometric indices need not be evaluated throughout the light curve. In systems where the difference in radiative properties of the components is not great (often the case in double-lined systems), the difference in color index between the components may not be best determined in the usual solutions of the light curves. Two additional, although not completely independent, methods are available for evaluating the individual color indices. If an eclipse is deep enough and is well observed in two (or more) wavelength bands, the color index of the star being eclipsed is just the color index of the light lost (account being taken of the difference in limb darkening in the two bands). The second method employs the relation between color index and surface flux in a standard band (usually V). The flux ratio is usually well determined from the depths of the two minima (for circular orbits) even if the light ratio is not well determined in the solutions. With the color index of the combined light, the flux ratio, and the flux-color index calibration, the individual color indices and fluxes may be obtained. It is a particular complaint of mine that many, if not most, of the computer-generated solutions of light curves that are published do not treat this matter of individual color indices and flux ratios satisfactorily, so that one cannot evaluate the radiative properties well. Beware of results that purport to give temperatures or bolometric light ratios without showing those radiative properties that come most directly from the observations - the flux ratios and light ratios in the observed, carefully calibrated wavelength bands. Then one does not have the available information he needs to obtain absolute fluxes and luminosities. In most computer programs I am aware of, one assigns a temperature to one component and derives the temperature of the other by means of some often unstated assumptions about the relation of surface fluxes to temperatures via black-body curves or model atmospheres. Such temperatures, although perhaps required in some intermediate steps in the program, should never be listed as evaluations. The only satisfactory approach is to apply the relation between a well-calibrated color index and absolute surface flux or temperature. Such relations are a major topic of this symposium.

The situation is more complicated for distorted than for spherical stars. For distorted components, the radiative quantities vary over the surface, and models must be used. But even in these cases, the investigator should give some kind of mean flux ratio and color index, derived from the observations, along with information about the assumed variation of flux over the surface. Distorted stars are, for this as well as other reasons, generally less suitable as standards than nearly spherical stars.

Let me give two examples of the kind of problems one may encounter with respect to radiative properties. I recently received a preprint of a study of a detached eclipsing binary. Both

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radial-velocity and photometric observations were of high quality. The photometry had been carried out in what were presumably standard B, V, and R bands. The two components differ considerably, with a mass ratio, q = 0.55, and a ratio of the radii, k = 0.83. Careful analysis using several indices and MK classification gave the spectral type of the primary A1.5V, or B-V=+0.04. From the photometric solutions in B and V, the difference in color index between the components is Δ(B-V)=0.51, giving for the secondary B-V=+0.55. From the V and R solutions, Δ(V-R)=0.56, giving V-R for the secondary +0.62, which corresponds to B-V=+0.80. And the flux ratio in V, 6.68, corresponds to Δ(B-V)=0.66, giving B-V=+0.70. 0.55, 0.70, 0.80 -which is correct, and why the differences? Perhaps the photometry was a poor match for the standard system. Direct comparisons with standard stars, which should always be carried out, had not been undertaken. Until this matter is resolved, the very good work on the system is not particularly useful, at least insofar as the secondary component is concerned. And it is just such systems, with considerable difference between components, that provide the best tests for evolutionary models.

My second example is taken from recent literature. The spectrum of the primary component of a well-known bright detached eclipsing binary has been classified in several independent studies in the range A2 to A4, and color indices are in agreement. The new study concludes that the true type is B9.5 by the following reasoning. The masses and radii of the components being known, one evaluates the temperature of the secondary (why the secondary I'm not quite sure), an F star, by the strange method of entering "standard" tables relating mass, radius, and temperature. Photometry gives the flux ratio, and from this the temperature difference. The resulting value for the primary is 10600K, and therefore it must be B9.5! We have here a beautiful example of the use of two questionable practices together. The first demonstrates how not to evaluate spectral types or temperatures. The second demonstrates that "standard" tables can be badly misused.

This problem of evaluating the radiative properties is most difficult of all when there is a large magnitude difference between the components. The greater the difference in properties, the better the system is as a test of evolutionary models, since the models must satisfy, with a single age and composition, stars of quite different properties and stages of evolution. While the radiative properties (luminosity, effective temperature) of the primary component may be soundly based, those of the secondary may be only poorly known. As our techniques for observing weak lines of secondaries and of measuring faint components in resolved binaries improve, the demands on the photometric differences between the components increase. Unless the technology for obtaining properly calibrated photometric differences keeps pace with the technology for orbit determinations, the value of the latter will not be fully realized.

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3.4. Additional Considerations

There is a fundamental observational attribute of a star, in addition to its mass, radius, and luminosity (or other radiative quantity). That is its chemical composition. Calibration of composition is also the subject of a presentation here. Such calibrations have not, by and large, been particularly successfully applied to binary stars, nearly all of which are disk or arm objects. The most striking departure among well-analyzed stars is the Am characteristic, and that is generally thought not to reflect interior composition. The tendency has been, rather, to evaluate composition parameters by comparing mass, radius, luminosity to models and come out with age and composition. Direct verification of the results so derived is not easy to obtain.

Throughout this discussion I have presented general, qualitative concepts. I hope no one has been waiting for my views on how spectrograms should be measured, how lines should be chosen, what orbit program is best, whose program for light curve analysis ought to be used, and so on. While I do have ideas on these matters, I consider them less controversial than what has been discussed. If anyone is interested in my views on these details, I recommend again that he read my papers.

Let me conclude this section on analysis of observations by relating a nightmare I occasionally have. I am visiting one of my European friends at his home institution. He investigates eclipsing binaries. "Let's do V5555 Centauri," he says. He sits at his console and starts interacting:

```
ID?       ESO-JA-524
TYPE?     ECLIPSING BINARY
NAME?     V5555 CEN
DATES?    MAY 14-MAY 20  (It is now May 1)
PHOTOM?   STRÖMGREN 4-COLOR
SPECTRO?  CORAVEL
PROGRAM?  W-D COMBINED (Wilson-Devinney photon + spectr)
START?    YES
```

On May 25 my friend returns to his console.

```
ID?       ESO-JA-524
NAME?     V5555 CEN
RESULTS?  YES
```

Immediately there appears on the screen:

```
     M1 2.734 ± 0.022        M2 1.945 ± 0.018
     R1 2.577 ± 0.011        R2 1.850 ± 0.011
LOG TE1 4.032 ± 0.013   LOG TE2 3.940 ± 0.009
 LOG L1 1.900 ± 0.057    LOG L2 1.246 ± 0.045
                 LOG AGE 9.111 ± 0.004
                 Y 0.273 ± 0.013 Z 0.027 ± 0.004
                 VROT    SYNCHRON
```

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"But....but....but..." I stammer. "But....but....but. How good were the nights? How many times was each minimum covered? What standards were used? What does the spectrum look like? How well resolved are the lines? What is the period? On what basis were the temperatures obtained?..?..?..?..?" "Why do you ask all these irrelevant questions? Everything is taken care of in the program and the ± values tell you all you need to know about how good the observations are." "But...but...but... What models were used? What mixing length? What opacity tables?" "Stop asking irrelevant questions. Everything is in the program. We use only the best models. You can trust our results."

To me this is a horrible nightmare. But perhaps to some of you it would be paradise.

4. RESULTS

Values of the masses, radii, and luminosities of the components of eclipsing binaries appear in the astronomical literature from time to time, both in original investigations and in compilations. It is for each scientist who wishes to make use of the results to evaluate the quality of published work. In the preceding sections of this presentation, I have discussed some of the matters that I take into consideration in my own evaluations. My Annual Review article of 1980 (Popper 1980) gives results for those systems that appeared to me at that time to be the most definitive in each of several categories of binaries (including visual and resolved spectroscopic binaries). Improved results are now available for several of those systems, and a number of new ones can, of course, be added. I am discussing with Johannes Andersen the possibility of preparing a supplement to the Annual Reviews compilation. I might say once more that the most vexing single problem, where I consider the results most subject to uncertainty, is in the evaluation of surface fluxes and temperatures, which carry over into the evaluation of the luminosities.

It might be useful at this point to survey the HR diagram briefly from the standpoint of reliable data on stellar masses and radii in particular. Only components in detached systems are relevant. The main sequence band from about B8 to F8 is the most heavily populated with good data. There is a small number of earlier B stars equally well known. But most B-type binaries suffer, for a variety of reasons, from a lack of well-resolved lines. The problem of treating properly lines that are somewhat blended has not been adequately addressed. There are no detached O-type binaries with first-rate results, the best being a couple of what appear to be contact systems. For the main sequence of types G to M, eclipsing binaries have thus far yielded only the two M-type systems, YY Gem and CM Dra, the latter a high-velocity system, although the G8 system, HS Aur, under investigation, should lie in this gap if there are no difficulties with it. It is here that the visual binaries are much more numerous. Whether they can ever be expected to provide masses of comparable accuracy (5%?) to that attainable for eclipsing

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systems is highly problematical. It may be possible to find, among known eclipsing systems of types late F and G, some with fainter, cooler companions with lines measurable with sensitive detectors of high signal/noise. UV Psc, FL Lyr, and RT And are possible candidates. The matter of fluxes and temperatures of the fainter components is a particularly difficult one here for two reasons. First, and already discussed, is the problem of obtaining a good color index because of the domination of the hotter star. Second, and more basic at this time, is the lack of an absolute flux scale for main sequence stars between the Sun and YY Gem (M1). There is no objective evidence that the (V-R)-flux relation ("Barnes-Evans" relation) for giant stars is valid for cool main sequence stars.

As main-sequence stars expand, their possibility of eclipsing increases, and there are a number of detached eclipsing binaries with components in the 1 to 2 $m_{\odot}$ range in which the more massive component appears to have evolved well across the Hertzsprung gap, but not so far as to fall prey to mass exchange. These systems show RS CVn characteristics, and it is not completely clear that their properties have not been affected by mass loss through stellar winds. Radius determination in most of these systems is subject to uncertainties because of their unstable light curves.

Eclipsing binaries have yet to produce good masses and radii for typical cool giants, although the Copenhagen discovery and work on TZ For should give results for a system apparently similar to Capella, the cooler component of which may be a typical giant. The selection against large stars is that if they are close enough to eclipse and to give adequate radial-velocity changes, they become semi-detached as the more massive components expand. The number of supergiant eclipsing systems amenable to analysis has not increased beyond ζ Aur, 31 Cyg and a questionable VV Cep in recent decades. Mass determinations of the most luminous stars of all spectral classes are non-existent. Direct information on the masses of stars of chemical composition clearly different from that of the local population (e.g., halo stars) is also non-existent.

If we wish to extend our discussion to results for semi-detached systems, we find that our supply of velocity curves of the faint Roche-lobe filling secondaries has been significantly augmented in recent years by application of the new generation of detectors. But the masses and radii of the components suffer in their determinacy in most cases because of absorption in the spectrum produced by non-photospheric material, affecting the velocities of the primaries, which are often of small amplitude to begin with. They also suffer because of difficulties with the light curves arising from the large amount of "reflected" light as well as from possible distortions by gas streams.

For stars in contact configurations, the approach to both radial velocities and light curves requires principles and concepts of analysis that are beyond those I have discussed here and that I have not pursued. A critical review of results in this field would take us outside the limits of this discussion. The same comments apply to the analysis of X-ray and cataclysmic binaries, both of which make use, in some degree, of radial velocity and photometric observations.

5. STANDARDS

As the organizers of this Symposium are aware, I have not been completely clear about the choice or even the concept of standards or calibrators among the components of close binaries. One can think of several approaches. First might be a list of the best-determined masses, etc. As noted above, this is basically the approach adopted for the most part in my Annual Review article (Popper 1980). The criterion for inclusion in such lists could be the accuracy with which the properties are thought to be known, e.g., all stars with masses and radii known to within ± 5%. There should also be some criterion for the surface fluxes or luminosities, since a mass is not of a great deal of interest unless one knows what kind of star is being considered.

A second approach could be to tabulate the best systems for each box in the HR diagram. The quality of the data would differ greatly over the diagram, some regions being essentially blank. Tables of this kind could serve as handy references for someone who just wished a general idea. It is possible, of course, to misuse "standard" tables of this kind. I have already given you an example of what I consider to be such misuse from the recent literature. This example illustrates a general problem with "standard" tables. Their application to a particular case assumes a conformity among stars, while it is just their diversity, even among well-behaved objects, that makes them fascinating. Nevertheless, the use of "standard" tables derived from observations is preferable to basing properties on models alone, as must be done for stars of a kind for which no masses are known directly from observations.

A third approach to the selection of standards might be to provide help in understanding some problem of particular interest. For example, the most informative binaries for testing predictions of evolutionary models should be those with the components having quite different masses and other properties. Differences in composition and age are eliminated, and one has the simplest evolutionary problem. One difficulty here is that, from the observational standpoint, the greater the difference in properties of the components, the more difficult the observations are likely to be. As another example, one might be interested in observations to test his prediction of what happens to mass-exchange systems. He might look for results that appear most nearly to match models he has found amenable to theoretical treatment, rather than those with the best-determined properties. This is likely to be a theorist's approach. Other kinds of questions, for which there are no direct answers, might be: what is the mass of a star at the turn-off point in a globular cluster, or what is the mass of the most luminous star in a galaxy, and so on. Thus, the very concept of a "standard" or calibrator requires a determination of the use to which the standards are to be put, a problem addressed in an earlier presentation here.

In all fairness, it must be pointed out the direct determination of stellar masses, in particular, the most fundamental of all the properties of a star, has failed to provide information for many categories of stars, so that the seekers for answers to many questions have, of necessity, had to rely on models rather than on observational material.

As for the future, I have already referred to improvements in instrumental and computational techniques that have played and will continue to play crucial roles in improving and extending our knowledge of the fundamental properties of the components of binary stars. Of equal importance in the realization of these goals will be the existence of astronomers deeply interested in obtaining fundamental data of high quality, as much as a service for our science as for solving specific problems of interest to them; the availability of observing time at major facilities for such general programs with long-range objectives; and finally, the willingness and patience to accumulate enough data, spectroscopic and photometric, and to subject it to the detailed, painstaking analysis required if the results are to be worthy of consideration as standards.

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DISCUSSION

JASCHEK: Two comments on your talk, in which you allude to quite general problems. It was true in the past that most photoelectric observations were made to an accuracy of ±0.01 in an undefined system, so the observations were totally useless. This applies specifically to observations prior to 1955. I thought this had improved, but from your talk I see that this still goes on.

The other situation that you alluded to is the disappearance of the observational material the observations are based on. Instead of plates, data are put on tapes and soon erased. We are on the way of becoming an observational science with just a very thin observational record base!

POPPER: That is a most unfortunate state of affairs.

LACY: I would like to point out that there are two eclipsing binaries (YY Gem and CM Dra) composed of main sequence stars which are used in the determination of the red part of the Barnes-Evans relation and they do not depart significantly from the mean relationship which is determined mainly by red giants. Also, on a different topic, I recognize your example of the misuse of "standard tables" as being that of deLandtsheer's paper on YZ Cas which appeared recently in Astron. Astrophys. I was asked to referee that paper and subsequently recommended strongly on two occasions that the paper should not be published until the data were reanalyzed in a more reliable manner. Nevertheless, the editor chose to ignore my extensive criticisms of the analysis and conclusions, and published the paper essentially unchanged from its initial form. As a result of this editorial lassitude we now have in the literature a bad analysis whose flawed results can now lead unsuspecting workers to erroneous conclusions.

POPPER: The absolute flux scale for main sequence stars from type F to M remains very poorly defined.

FRACASTORO: I wonder whether it would not be useful to add in your list another category of people, namely those who do identify spectral lines in a spectroscopic binary, and then deduce radial velocities, assigning them to pure Keplerian motion of the star. It is rather frequent, instead, that these velocities are altered by the contribution of circumstellar matter. From Batten's Catalogue of Spectroscopic Binaries, a Barr effect results even when orbits having eccentricities e > 0.6 are selected. Therefore, several radial velocities must be spurious.

I have a second point. You have shown some discrepancies resulting when $(B-V)_c$ or temperatures are deduced from observations made in different groups of colors. In my opinion, this might depend on the fact that in a Planckian mode, two colors would be sufficient, whereas the spectral bands U, B ,V etc. have been selected redundantly with the aim of getting the maximum of astrophysical information. This is just the opposite of the Planckian model and may explain the inconsistency of some results, as a consequence of the different viewpoint for laboratory and astronomical photometry.

HEINTZE: Lacy stated that the YZ Cas results Popper showed in his introductory talk may not be used in my poster paper to be presented next Tuesday. However this temperature has been checked carefully. From a high dispersion IUE short wavelength spectrum the metal abundance turns out to be about 10X the solar abundance. From a low dispersion IUE long wavelength spectrum (calibrated) and the Kurucz (1979) models it turns out to be impossible to get a T_{eff} lower than 10000 K.

POPPER: My reason for not referring to YZ Cas by name is that I merely wished to point to what I considered examples of poor practices, rather than to discuss the UV observations.

HEINTZE: I agree with Popper, that the study of detached eclipsing binaries, which at the same time are double line spectroscopic binaries does not give T_{eff} directly but flux ratios as a function of wavelength next to the very precise masses and gravities. However, we desperately need the effective temperatures of these components in order to compare with evolutionary tracks. Modern analysis methods of light curves in any case give a model-effective temperature of one component if that of the other component is assumed or measured. According to me the best way to find T_{eff} for components that obscure one another totally is to observe the energy distribution in a long as possible wavelength region, as has been done by Plavec for SX Cas, U Cep and RW Tau.

POPPER: If wide ranging spectrophotometry is not available, and it usually is not, the T_{eff} is best evaluated through well calibrated indices, such as (b-y) or (B-V). Flux ratios of the components in the V band, for example, are also of importance. My objection was to the custom, by means of programs for light curve analysis, of giving values of T_{eff} rather than of those quantities derived most directly from the light curve, such as flux ratios, light ratios in the observed bands, etc. T_{eff} is <u>not</u> a suitable parameter in light curve analysis.

CHOCHOL: It is not easy to distinguish if a system is detached or semi-detached, especially in the case of early-type binaries. Are you sure that all the binaries used in your work are detached systems?

POPPER: Until the analysis of a light curve has been carried out, one cannot be certain. With one possible exception, I think all the B-type systems, for which I have considered the properties well determined, are detached.