POSITIONAL ASTRONOMY AND STELLAR MASSES

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ABSTRACT. In this paper are surveyed the combinations of data needed for mass determinations, the impact of radial-velocity and speckle observations, the 1983 catalog of visual-binary orbits, the current material for main-sequence and red-dwarf masses and the controversial methods and results on low mass objects.

Masses are a delicate link between stellar properties, since they follow from the cube of observed quantities, and in turn enter evolutionary models with a substantial power. As an illustrating although rather unrealistic thought: If the age of a star could be found directly with a precision akin to that of orbits, and we could reason the opposite way, orbit dimensions would be found as a tenth root, with so small an error that there would be no further need for nocturnal gymnastics with micrometers, spectrographs, and the like.

The recent Fourth Catalog of Orbits of Visual Binaries (Worley 1983) contains 847 objects, including 80 for which second solutions are listed. Less than 40% of the entries come from the 1970 catalog; the others are revised or new. We have based selection and grading on computer-generated lists of residuals, and have examined some hundreds of other orbits (including a substantial number before 1970). We have also reobserved almost all the listed pairs during the last decade. Datacenter files were searched for recent photometric information, and published radial velocities were examined.

The total of 847 pairs are only 1% of the Washington Double Star Index (Worley 1984). On the other hand, only 5% of the orbits qualify for good mass determinations, i.e., less than 100 binary components. (This number does not include eclipsing pairs). The evident reason is that several kinds of data and conditions need to be combined, and if just one of them is not pinned down to a few percent, the resulting masses will not be reliable. Even some data of ostensibly good quality will probably be inaccurate owing to the presence of

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undetected subsystems.

The classical case is the combination of an orbit with a trigonometric parallax and a photographic mass ratio; the latter may also give some indication of a possible third mass. Heintz and Borgman (1984) give a list of new mass ratios recently measured at Swarthmore. As always, the parallaxes are generally the weakest data. Masses would be within much better reach if the typical standard error of a parallax went from 5 mas (milli-arcsec) to 3 or 2. However, this will not be easy to accomplish, as it requires - among other things - an adequate time interval as well as good seasonal distribution of measures, lest some random error in the proper-motion term carry over perilously into the smaller parallax. This linkage is probably a major cause for the often large differences between nominal and actual parallax errors.

How reliable are visually measured separations, on which most visual binary orbits and masses are based? The currently active micrometer observers have no significant systematic differences between them, which is reassuring but not strict evidence. Direct comparison with photography reaches only down to 2" or 1".5. Orbits with large changes of angular separation afford a sensitive criterion, since systematic errors at the small separations should show as deviations from the Law of Areas - yet they are rarely noticed. Recently another control has been added by the speckle measures. Lists of standard stars for visual separations have been proposed, for close pairs in particular by P. Couteau (1969), and it is suggested that measures with new techniques include some of these pairs.

Now for the bad news: We have virtually no masses from the southern sky. Most parallaxes are of substandard precision, and no photographic series (except α Cen) is long enough for a mass ratio. Secondly, the visual pairs are very strongly selected in favor of near-equal luminosities, i.e., of fractional masses between 0.4 and 0.5. Statistical incompleteness increases rapidly when the magnitude difference Δm exceeds 2 mags. Speckle is less impeded by large Δm values. There is no reason to believe that lower-mass components in non-interacting binaries behave abnormally, but there is not much evidence in support of their normality either. Finally, the photometry of components is a familiar headache. Earlier work was done visually by wedges and polarization, and much of these results seem to be good and solid; but the speckle CCD will be able to contribute also in this respect. Incidentally, the magnitude estimates by visual observers - as I believe to have seen from numerous comparisons - are mostly better than their reputation. Weed out a few of the estimates, and derive systematic corrections for some others, and then a large body of potentially valuable data reposes in the card file on double stars. The catalog by S. Wierzbinski (1969) was quite good but it could be redone now with much increased material, estimates as well as photometric comparisons.

Radial velocities contribute to the study of visual binaries particularly for many objects outside the range for good parallaxes, and especially in recent years when spectroscopically found pairs can be speckled or vice versa. There have not been too many objects of combined analysis, simply because they are in the spectroscopic long-period range. The observational effort is lengthy, the RV amplitude usually small, and the lines often annoyingly blended. The problem that systematic errors and changes in some earlier spectrographs are not well known compounds the difficulty of combining data from various epochs.

A blessing in the disguise of disappointment lies in the frequent discordance between observed radial velocities and those predicted from visual orbits. The two kinds of observations are complementary in that they strengthen each others weak points; what the positional measures leave poorly determined, may make a large effect in the RV curves. The bearing on the accuracy of masses is obvious. In particular, when part of an orbit cannot be reached visually, or only with difficulty, the uncovered arc could cause errors in the elements e and ω, which transfer via the inclination substantially into the semiaxis major and the mass. An uncertainty of i enters the computed orbit dimension in opposite directions (through cos i and sin i, respectively), and may be revealed by a disagreement of the spectral type with the computed parallax. In brief, good mass results will come from the positional/spectroscopic combination, provided we are aware where potentially harmful uncertainties are. Another outcome of speckle observations is to be mentioned in this context, viz., the negative observations, which reduce the upper limits for small separations more than visual observers can achieve.

It may be time for a new compilation of masses from visual and interferometric pairs, but I have delayed that for several reasons. Some systems are under current investigation; the orbit catalog was not completed, and the Yale parallax catalog (the final version of which is not yet available) may shed light on the question of systematic errors of parallaxes. It was noticed long ago that the inclusion of marginally reliable binary-star parallaxes (0".04 to 0".02) tended to shift the mass-luminosity line slightly toward lower masses; but I am not sure if this is real, and is caused by a systematic error, such as derived in the catalog by Jenkins (1952).

Speaking of calibration, we have to mention the Hyades. Of course, we expect them to match the field stars in that almost every one should be double. Ten orbits are recorded, three of them with spectroscopic subsystems. The objects β 552 and 70 Tau are still under study; but the estimated masses indicate already a difference of their distance moduli by 0.8 or 0.9 mags. Compared with the tangential diameter of the Hyades, it seems that they are really a "globular" cluster.

Generally, however, we cannot afford to be choosy when it comes to mass calibration; we have to grab results where we can get them. Only for the middle main sequence (types A to K) has the mass-luminosity relation not changed in 25 years and seems to be safely pinned down. Exploration of the overluminous, expanding masses is being aided by photometric measuring systems designed for evolutionary classification. The simplest indicator of such stars is the dynamical parallax in comparison with the spectral type. I will not take up the subject of dynamical (or orbital) parallaxes here; they have been overused and over-discussed in the past. They can serve as luminosity indicators, and may show when an object is in some way peculiar. Note that dynamical parallaxes are sensitive to the apparent magnitude - a quantity often not well known for faint binaries.

The mass array for the Population II main sequence seems not within easy reach. Catalogs of proper-motion stars contain numerous high-velocity objects; but it will be a long way to find out which of these are resolvable binaries and are also metal-poor.

The mass-luminosity graph for the red-dwarf segment of the main sequence is reproduced in Fig. 1 (from Heintz 1983). A few revisions and additions in the latest years have not changed the feature of a surprisingly wide spread, which cannot be explained by parallax errors.

Moving further down the mass scale, the orbit catalog lists 23 unresolved pairs, mostly from U.S. Naval Observatory results. Some of them are still shaky as the series of their measurements are short or weak. Most of the unseen companions are probably in the stellar range, but one or another black-dwarf mass should be among them. This subject leads to another part of high-accuracy positional astronomy, and is characterized by two features (not necessarily related): It is still executed by photography, and it has a controversial history.

Unseen companions reported from apparently variable proper or orbital motions have had a very high mortality rate - even more than stars recorded as having variable radial velocities earlier, and found constant upon reinvestigation. Evidently some stronger evidence is required than merely a few consecutive plus or minus residuals of positions, namely a significant correlation between at least two telescopes and/or a significant recurrence of an orbital pattern. With an observed amplitude of 50 mas, one can feel as reasonably sure about the case as one could with a spectroscopic range over 10 km/s; but effects at the levels of 20 mas or of 4 km/s need to be checked more carefully. Unfortunately, the black dwarfs in the expected mass range of 10 to 50 mS (millisuns) will be, with few exceptions, below the 20 mas level, and hypothetical planetary objects (under 5 mS) are well below 10 mas. Much larger instrumental effects being known, it should be standard practice to ascertain beyond reasonable doubt that an alleged feature is not caused by the equipment. But planets are such a crowdpleaser that one can get by with almost anything.

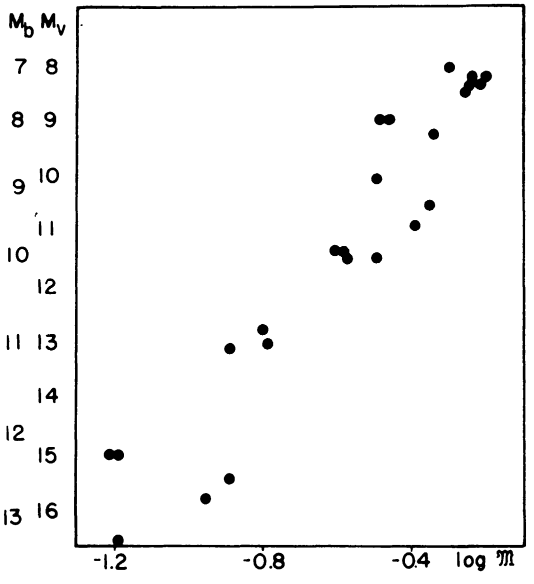

Fig. 1. The lower part of the mass-luminosity diagram.

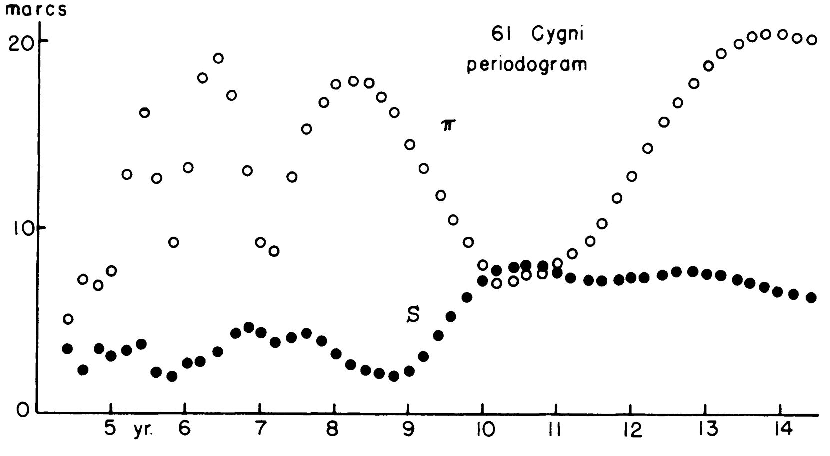

Fig. 2. Periodogram of the relative positions of 61 Cygni measured with the Poulkovo (Π) and Swarthmore (S) refractors.

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Criticism of suspect results need not even necessarily invoke systematic errors. The binomial theorem suffices to show that even a considerable number of random results, say, at the ±30 mas level, need not cancel out to below ±10 mas, but that there is a 10% or 20% chance of a pseudo-systematic effect remaining that level.

In Fig. 2 (presented at a conference in Tübingen, 1978) is shown a periodogram of the often discussed and probably best-known case of suspected planetary objects, 61 Cygni, as a straightforward instance of inadequate study. The Poulkovo refractor had two periods with very serious residuals of over 50 mas, separated by 25 years. The overtones of that time interval show plainly as peaks in the period spectrum. At the Swarthmore refractor (S) the same relative positions of the pair show no period.

It has been argued that photography is excellent, and also, that it is poor and inefficient, needing replacement by other techniques the sooner the better. In this fray one alternative seems to have been largely overlooked, i.e., whether photography has ever been given a fair try by coupling it with adequately general reduction methods - and this over almost a century during which the technique has been in use!

A data-processing method may be useful for parallax reductions but for nothing much else; the study of parallax errors - restricted to a narrow bandpass of 1 yr^{-1} - is not too significant for the analysis of long-term variations that may mimic periods. For instance, the often-heard statement that more than four or five reference stars on the plate do not help the formal precision of the parallax could be generally underwritten (since geometric and magnitude balances of the reference star frame are at least as important), provided it is clearly understood that it applies to the parallax only, that all errors can be treated as random, and that the conclusion does not necessarily apply outside the one-year bandpass in which it has been tested.

The approaches of analysis (cf. Russell 1978) in some ways resemble the problems of complex eclipsing-binary light curves. A rigorous analytical approach may overload and destabilize the solution, wasting computer storage in the process, whereas a synthetic approach is less straightforward to describe and involves more "educated-guess" technique. This may explain why the theorist's delight and the practitioner's preference can disagree - for light curves as well as for astrometric field analyses.

I have not published much on the small-amplitude, low-mass suspects in recent years, but that does not mean satisfaction with the status quo; it had rather extraneous causes related to past history. At the IAU General Assembly in Brighton in 1970 we discussed the alleged discoveries of low-mass objects in the 1960's, the apparent field distortions at the Sproul refractor (qualitatively studied in

1961), and the question whether the neglect of correcting for the latter was related to the former. At that time I did not believe in serious errors, but consented to look into the matter. Reaching an appalling conclusion in July 1972, I indicated the intention of thoroughly reinvestigating the "planets". Within one week administrative decisions occured which had the effect of forestalling the project forthwith. More interesting still, in December 1976 a befuddled administration restored my access to research support, but under the explicit condition that my work not parallel that of the local observatory. Which goes to show that the proof of extrasolar planets requires the use of subtle measures. Since this muzzle was dropped in late 1982, tests by remeasurement of selected material have begun, in order to check on the telescope errors and on the hypothesis I had published in 1976 from limited data. The decade since has elapsed without progress toward better material at this telescope, and some other things have happened: several cases whose reality had been questioned were studied at other instruments, and invariably resulted in an absence of confirming correlations. And the alleged planets have gotten into the propaganda machinery of the Extra-terrestrial Intelligence campaign. To be sure, SETI is a legitimate scientific endeavor, but it can also be misused as an ideology or a pseudo-religion. The way some of the literature distorts or altogether suppresses astronomical evidence smacks of pseudo-scientific UFO fanatism and leaves some room for concern. However, the improvement in data processing gives hope that the reiteration of past truths will not remain an embarrassment to astrometrists much longer. The course of events - slipshod analyses followed by equally inadequate cover-ups - has cost us at least 20 years in learning what actually goes on in high accuracy imaging. More regrettable still, a field analysis is quite laborious, with more than ten times the work and costs of an ordinary parallax; thus I have to maintain a moderate pace, lest the output of other data be impaired. Some effort along this line is needed, however, since issues other than ETI are riding thereon. In particular, there is the question of a continuous mass frequency relation into the range of black dwarfs, and that pertains to the problem whether binary star and planet formation are due to similar processes (there seems to be evidence that they are not). Currently debated among cosmologists is also the issue whether the missing mass can or cannot be explained in terms of very large numbers of stars in the 10 to 100 mS mass range.

The current status of the data on the cases of suspected lowest mass companions (under 25 mS) is thus:

BD + 4°3561 (Barnard's star):
Four series from different telescopes, and no significant correlation between any two of them. Periods had been calculated from limited time spans, but over the whole interval the effect, if any, in the residuals seems aperiodic. Under reinvestigation as a standard test field.

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BD + 36°2147 and AD Leonis:
Found spurious on reinvestigation, and dropped from lists of binaries.

ε Eri:
Disallowed in two reinvestigations, and rated spurious.

EV Lac:
Original orbit (calculated by the author in 1972) erroneous, caused by uncorrected observations. Unclear residuals since, but the reference background on the Swarthmore plates is very poor; reanalysis therefore of doubtful value as only a small part of the material is useful. The star would be of particular interest because of UV flares.

Σ 2398:
New measures at a noise level below the formerly suspected small amplitude, and clearly no evidence for a submotion.

61 Cygni:
Original results from weak data now disallowed.

Stein 2051:
Presumed period not yet covered; reinvestigation due in five or ten years. (The last three cases are distinguished from the former by a better reference frame on our astrometric plates).

Other unresolved objects presumably belong to higher mass ranges.

Perhaps it is still premature to compare output with effort. Father Stein had to measure 2050 rather uninteresting doubles before he hit on a remarkable, red triple of large parallax, which possibly contains the first-known black dwarf. A large number of binaries is still under observation, with more being discovered, but only a fraction of them will be rewarding. Which pair will have an eclipsing or astrometric subsystem, a variable component, or something else worth noting, cannot be foreseen. Many spectroscopic pairs sit in the catalogs with what appear to be garden-variety orbits - until some of them become exciting on reinvestigation fifty years later because of element or line changes.

The progress in mass determination has been slow in quantity, but probably better in quality, considering the smaller error ranges in quite a few instances. The results always have come from large data bases: survey coverage as well as enhanced attention to specific objects. Micrometers and astrometric and spectrographic plates may be phased out by the end of this century; new techniques may be able to further reduce error limits and - as I hope - may improve the tractability of systematic effects. They are likely to increase the demands on large telescope time and the unit cost of observation. Ways will doubtlessly be found to reconcile wide range surveys with the concentration of individual, promising objects. It can be stated as a cosmological principle that exciting results are apt to come from objects which nobody before had time to put on the observing program.

This presentation is dedicated to the memory of our late colleagues and friends F. Zagar and J. Fleckenstein of the Osservatorio di Milano. It was at the Schiaparelli Symposium in 1960, which they had marvelously organized, that I first addressed the topic how to unscramble orbital and proper motions.

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DISCUSSION

STRAND: I agree with the speaker that new technology will bring greater accuracy which will be of importance, but it will not necessarily be useful in certain cases. An example is the determination of the mass-ratio in visual binaries, where observations over half a century, perhaps more, are necessary. For such a problem the archival value of the photographic plates is important.

HEINTZ: Agreed; there is no comparable means of storage. We must be aware, however, of a possible psychological bias that "what is new must be better". Older methods may lose some credence (and support) although they are demonstrably not inferior.

POPPER: In the discussion of parallaxes this morning, we heard of the old story of systematic differences in parallaxes between different institutions. Even though the formal errors in the parallaxes may be small, one might be concerned about their reliability.

In your reference to your new catalogue of visual binaries you mentioned that there were 40 - 50 pairs suitable for mass determination. On what basis was the acceptability of the parallax decided?

HEINTZ: The formal error was under 10%. In borderline cases, consideration was taken of the agreement of multiple parallax determinations, or of confirmation by spectral type.

STRAND: A chapter in "Basic Astronomical Data" lists the visual binaries for which the masses of the components have been determined with a precision of 30% or better, which require the parallaxes to be known to better than 10%.

EVANS: Dr. Bjorn Petersen has done a series of high dispersion high signal-to-noise ratio spectra of AD Leo and EV Lac as flare stars, which may resolve some of their duplicity problems.