THE RECONCILIATION OF VISUAL AND SPECTROSCOPIC OBSERVATIONS OF BINARY SYSTEMS

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ABSTRACT. The special problems presented to the computer of orbits by radial-velocity observations of visual binaries are discussed under three heads: (i) problems caused by the small range of velocity variation, (ii) problems caused by the long periods, (iii) inconsistency between visual and spectroscopic observations. It is pointed out that radial-velocity observations, even when they are insufficient for independent determinations of orbital elements, impose constraints on possible values of those elements which may in fact be helpful to the computer of visual orbits. In particular, as is illustrated by reference to ADS 8189, even a few observations of the radial-velocity are sufficient to destroy the mutual independence of the elements e, and ω.

I. INTRODUCTION

The topic assigned to me is computational problems in, and techniques for, the determination of orbital elements of spectroscopic binaries. Of the three methods by which binaries can be observed - visual, photometric and spectroscopic - the last-named probably presents fewest problems to the would-be computer of orbital elements. The experienced judgment of a visual observer appears to be necessary to select the reliable visual observations and even, sometimes, to draw the apparent ellipse through the plot of them. To venture on the determination of a visual orbit without that experience is to run a considerable risk of obtaining misleading results. Both the mathematical and physical complexity of the photometric problem make the determination of the orbital elements of eclipsing binaries difficult. Methods of solving that problem, have often proved a rich source of controversy. The mathematical theory of the determination of orbital elements for spectroscopic binaries, however, is fairly simple. Two basic methods - Lehmann-Filhes' for large eccentricities and Sterne's for small - suffice for the overwhelming majority of practical cases that the computer is likely to encounter. Each of these methods lends itself readily to automatic computation and most observatories engaged in this work have programs for them. Provided the available observations are reasonably reliable, an investigator does not need a great deal of experience to obtain useful values of the orbital elements. I am ignoring here the interacting binary systems in which the stellar spectra are distorted by the spectrum of streaming gas. Although this group of systems is important, the problems it presents are of interpretation rather than computation. Moreover, it is not a group of prime interest in the present context.

There are problems, however, in the combination of spectroscopic and visual observations of a given system. They are not all strictly computational, but they must be considered before the computation can be made. They may be divided into three classes:

(i) problems arising from the small amplitude of the observed velocity variations.

(ii) problems arising from long orbital periods.

(iii) problems of inconsistency between spectroscopic and visual data.

This is only a rough classification. Small amplitudes and long periods go together, and the problems encountered could be ascribed to either of these causes. The inconsistency of the two kinds of observation is also often the result of one or both of the other two sources of problems. The threefold scheme, however, provides a useful framework for the present discussion.

II. PROBLEMS OF SMALL AMPLITUDES

A velocity variation of small amplitude is obviously hard to detect, especially if the binary contains two nearly equal components unresolved on the spectrograph slit. At a dispersion of 2.5 A mm^{-1} we can resolve the spectra of such systems when $\Delta V > 10$ km s^{-1} (if the spectra are of solar type). The number of systems bright enough to be observed with medium-sized telescopes at that dispersion is strictly limited. With increasing access to large telescopes and the application of radial-velocity scanners of the general type devised by Griffin (1967), however, spectroscopists may be able to make a larger contribution to visual-binary studies. We can also systematically select binaries with orbits so oriented that even a relatively small amplitude produces a large velocity variation near periastron, or we can choose spectroscopic-visual triples such as Fekel (1981) has studied very carefully.

The first problem, then, is the difficulty of detecting a binary with certainty. For example, Groombridge 1830 is known to have a visual companion and Beardsley et al. (1974) have suggested that both the radial velocities and proper motion indicate a period of about 60 years. Scarfe and I have observed this star for several years and have found a velocity range of about 6 km s^{-1}. At the dispersions we are using (6.5 A mm^{-1} to 15 A mm^{-1}) this is on the borderline of detection. We cannot find a period and are not sure the velocity varies at all. If it does, the period is probably much less than 60 years. The important parameter is obviously the ratio of the amplitude of the velocity variation to the plate-to-plate scatter (external error) of the individual observations. Dramatic reductions in the latter quantity, by methods which have been discussed earlier in this colloquium, obviously will reduce the minimum amplitude of detectable velocity variation, but they will also create new problems. Equipment must remain stable over long intervals of time if long-period velocity variations are to be measured with certainty; the combination of velocities determined at different observatories will also be more difficult at precisions of the order of 10 m s^{-1}. We still do not know if the stars themselves are stable to that level of precision.

The second problem encountered, even at presently attainable levels of precision is this combination of results from different observatories. Just as the visual-orbit computer must allow for the known personal errors of even historically important observers, his spectroscopic counterpart must allow for the differences between the several velocity-systems of various observatories. Wilson's (1953) summary is still the best discussion available of the situation up to that date, but obviously is of no help for assessing work from new observatories. We often have to establish these systematic differences by trial and error. A discussion of the triple system ADS 14893 by Fekel, Scarfe, West and the present writer, for example, is delayed by apparent systematic differences between observatories that are different for each component.

A third problem is not yet often important but may be encountered more frequently if precisions of 10 m s^{-1} are achieved. This is the secular change in radial velocity caused by the proper motion of the star. There is a corresponding secular change in proper motion itself which van de Kamp (1975) considered for Barnard's star. Groombridge 1830, like Barnard's star, has a high proper motion and the secular change is, in principle, already detectable. Radial velocities of Groombridge 1830 have been obtained for about 80 years, and in this time the perspective effect should have made the radial velocity (about -100 km s^{-1}) approximately 0.6 km s^{-1} less negative. Visual binaries are found preferentially amongst stars of large proper-motion, and if very high precision in the measurement of radial velocity is achieved this secular effect may become detectable sooner for a considerable number of such systems.

The foregoing problems arise whether the spectrum of one or both components are visible. If both spectra are visible, as already mentioned, it may be impossible to resolve them at all. For example, 81 Cnc has predicted velocity separations of 14 km s^{-1} and 27 km s^{-1} at the two nodes. At 6.5 A mm^{-1} (it is too faint for higher dispersion Victoria) the spectra are not resolved at the lesser node and no useful observations can be made. We hope to resolve them at the greater node or by observing elsewhere, but even then they may not be completely resolved. The computer should be alert to the possibility that corrections should be applied to the observed velocity differences if the separation of the two spectra is less than about 1.5 times the half-widths of the lines. These corrections have been discussed by Petrie et al. (1967) and Batten and Fletcher (1971). An example of their application is provided by Hans et al. (1979) in the discussion of δ Equ. The correction is precisely analogous to that required to the measured separation of two stellar images very close together. The underestimates of

K_1 and K_2 that would result from uncorrected measures can have an important effect on the derived mass which, of course, depend on $(K_1 + K_2)^3$.

III. PROBLEMS OF LONG PERIODS

The periods of visual binaries are measured in years, decades or even centuries, and many of them are still very uncertain even now, two centuries after the existence of visual binaries was first demonstrated. Thus the spectroscopist's most fundamental difficulty arising from long periods is knowing when to observe. Two binaries (ADS 11060 and 11579) that we have recently followed through periastron passage had orbital elements determined only provisionally. The periods had been rounded off to 20.0 years and 90.0 years respectively. In each case periastron passage was later than expected - by nearly two years for ADS 11579. This is not an important error, of course, in so long a period only provisionally determined, but it indicates the importance of beginning observations well in advance of expected velocity change since the latter may just as likely be earlier than expected and an important opportunity lost for a generation or more. This may have happened for ADS 3588 for which van den Bos (1935) predicted periastron passage in 1982.0 and van Dessel (1977) in 1980.9. While I am still uncertain how to interpret my observations, I believe periastron passage may have been sooner even than van Dessel's prediction.

As remarked above, spectroscopists tend to select systems for observation in which a large velocity difference between components is expected for a very short time. All three of the systems mentioned in the last paragraph are of this type - that is why uncertainty of the period is a problem to the spectroscopist. On the other hand, once a periastron and nodal passage has been successfully observed spectroscopically, we can pass on to the visual observers much more accurate information about the time of periastron passage (and eventually the period) than they can often derive themselves. For example, Batten et al. (1979) quote an uncertainty of $\pm\, 0\overset{\mathrm{d}}{.}19$ in the time of periastron passage in the long-period orbit of ADS 11060, and Morbey (1975) in his rediscussion of Fletcher's (1973) observations of β 1163 quotes an uncertainty of $\pm\, 0\overset{\mathrm{y}}{.}02$. Already from a preliminary (unpublished) solution, Fletcher finds an uncertainty in the time of periastron passage of ADS 11579 of $\pm\, 0\overset{\mathrm{y}}{.}05$ - which will probably be reduced in the definitive solution. Compared with determinations that could be made from visual observations alone, these are virtually free of observational error and should be regarded as fixed in any subsequent solutions that do not include new radial-velocity data. This is not open to the same objections as the old practice amongst spectroscopis of fixing T or ω. That was an arbitrary device, often adopted before Sterne (1941) developed his method, in order to be able to obtain a solution at all for the elements of nearly circular orbits. The values of T and ω thus fixed were usually meaningless. Now, however, I am proposing only that a very well determined quantity might, in some circumstances, be treated as exactly known, in order to increase the weight of the determination of the remaining unknowns. It is unlikely that new determinations of the orbits of ADS 11060 or β 1163 will be attempted before new radial-velocity data are available, but all we may be able to provide for ADS 3588 (period 55 years) is a constraint on possible times of periastron passage which should be taken into account in a new solution. Even the very well determined orbit of 70 Oph (Worth and Heintz 1974) may be improved in a few years time when we shall be able to provide a spectroscopically determined value for T.

As a corollary to the accurate determination of times of periastron passage, it follows that radial-velocity observations can often help to provide more accurate values of the period than can be derived visually. Obviously, however, <u>two</u> periastron passages must be observed spectroscopically, so it will require a considerable elapse of time before spectroscopists can make this contribution. We have already refined the long period of ADS 11060 to 7398 days (with a formal uncertainty of less than a day) but the star must be observed in 1998 to confirm this value. After that, we can be very confident of whatever value we derive since the interval in which the velocity changes rapidly is so small a fraction of the period. The same will be true for β 1163 after 1988: unfortunately, I do not expect to be able to apply for telescope time at the next periastron passage of ADS 11579. Even so long a period as that system has can be refined from observations of one periastron passage since they do convey some information about the mean motion. Nevertheless, for some time to come, computers will have to combine information from visual and spectroscopic observations, in whatever way they think best, to arrive at the best possible estimate for the period of a given system. Since the total mass varies inversely as the square of the adopted period, some attention needs to be given to this.

IV. INCONSISTENCY BETWEEN VISUAL AND SPECTROSCOPIC OBSERVATIONS

In view of the difficulties outlined above, it is not very surprsing that visual and spectroscopic observations sometimes lead to mutually inconsistent results. Sometimes the inconsistency is only apparent and easily removed; at other times it is real. It ought to possible to remove inconsistencies by solving for the orbital elements from all available observations (spectroscopic and visual) simultaneously. Until recently there was no method for doing this. The combination of positional measurements in one plane with velocity measurements in an orthogonal plane is not a trivial problem, especially since a complete solution involves the parallax which may not be known independently of these measures. Morbey (1975) has devised such a method, and his method, so far as I know, is still the only one available. It is based on the principle of maximum likelihood and is probably the most general way of computing orbital elements yet produced. When it is used, inconsistencies cannot develop because the solution must fit all the available data simultaneously. It cannot come into general use, however, until more radial-velocity observations have been systematically collected. Systems for which radial velocities have been determined at only a few isolated times are unlikely to provide suitable examples for the application of the method. Whenever the radial-velocity observations cover the important parts of the velocity-curve well, however, Morbey's method can lead to very accurate values for all orbital elements. Although it is not a disadvantage inherent in the method itself, the experience of a visual observer is needed to sort out which observations are worth including in the orbital solution. Despite the availability of this new method, therefore, partial and separate solutions for visual and spectroscopic orbital elements will probably continue to be made for many systems, at least for some time to come. Sometimes, these will be found to be mutually inconsistent.

An example of apparent inconsistency is provided by ADS 11579, already mentioned. The predicted and observed variations of the velocity difference are shown in Figure 1. Apart from the delay in the observed variation - which is of no great consequence - we see that the observed maximum (negative) velocity difference is somewhat less than predicted. This arises because the dynamical parallax adopted (0".0076) in calculating the predicted value was a little too small. The observations correspond to a parallax of 0".0085 (approximately). The remaining differences between prediction and observation are removed by very minor modifications to Baize's (1950) elements. This system will probably prove to be very suitable for Morbey's method, and after the spectra are no longer resolvable we hope to publish more exact values for the orbital elements. In the meantime, we note that the apparent inconsistency is easily removed because the small change required in the parallax is quite acceptable. The system is too far away for a good trigonometrical determination of the parallax, and since both stars are evolved, there is no strict constraint on the parallax through the masses. We require only that the mass of each star should be at least that of main-sequence star of the same spectral type. In fact, we find about 1.5 $m_\odot$ for each of these (approximately) G5 stars.

It is otherwise with ADS 8189 which I studied several years ago with the late R.M. Petrie (Petrie and Batten 1969). Figure 2 shows the observed velocity variation together with two slightly different predictions based on elements obtained by Muller (1955) and Couteau (1965). There is no appreciable discrepancy in the time of periastron passage; the possibility discussed in the 1969 paper that periastron passage would be later than predicted has not been confirmed by (admittedly rather few) subsequent observations. This system is a spectroscopic-visual triple, so the velocity of the centre of mass of the short-period binary has to be inferred from those of <u>its</u> two components. The velocity differences determined for the visual pair, therefore, are few and of relatively low precision. The system is probably not a good example for the application of Morbey's method, yet the obvious discrepancy between spectroscopic and visual results cries out for attention. The observed maximum velocity difference for the visual pair was

$$V_{max} = 5.8 \pm 0.7 \text{ km s}^{-1}$$

whereas, as is well-known

$$V_{max} = 29.76 \frac{a'' \sin i}{\pi'' P(1-e^2)^{1/2}} (1 + e \cos \omega) \qquad (1)$$

where all symbols have their usual meanings. If values are assumed for the

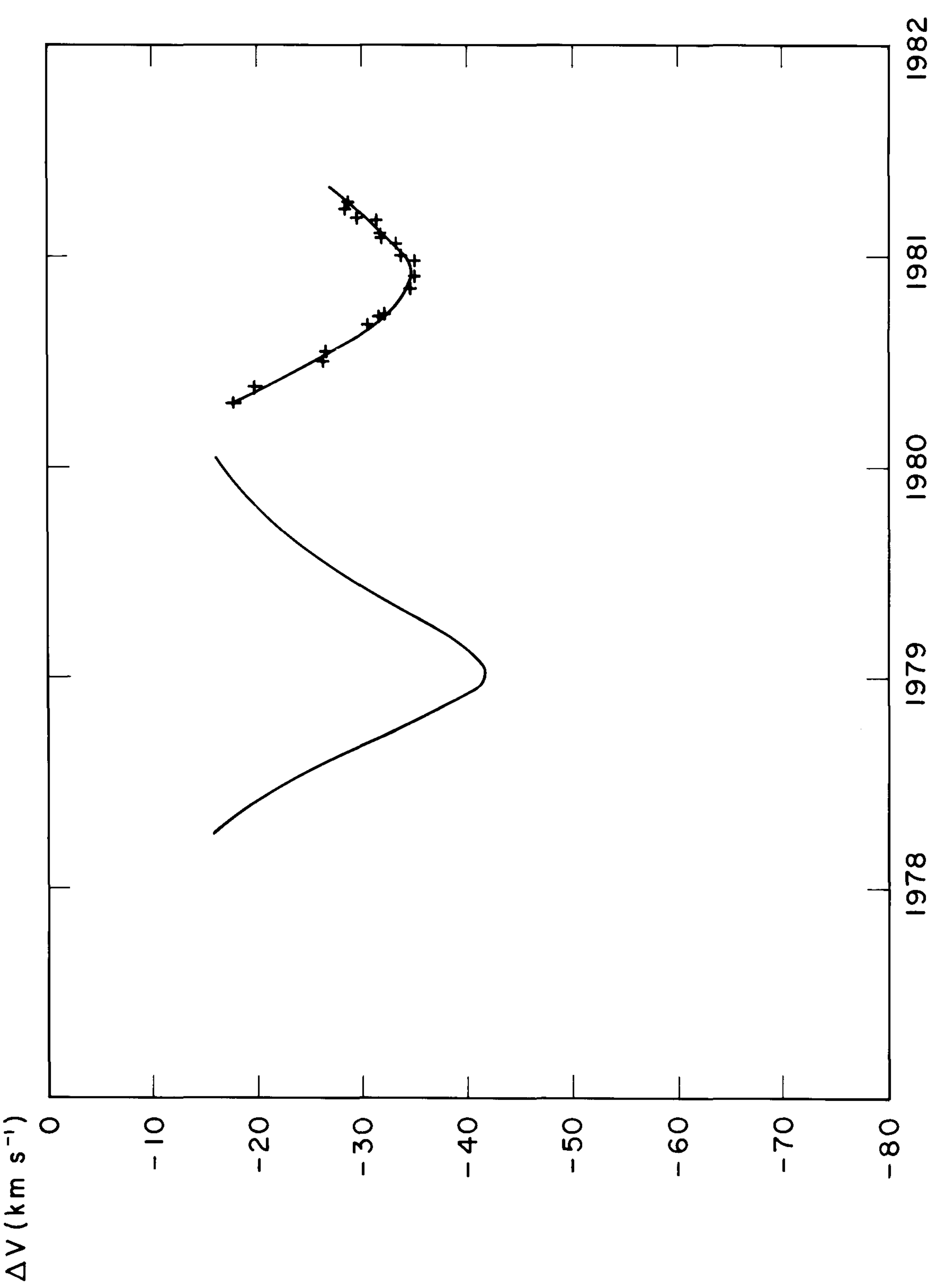

Figure 1. Predicted and observed velocity differences between the components of ADS 11579.

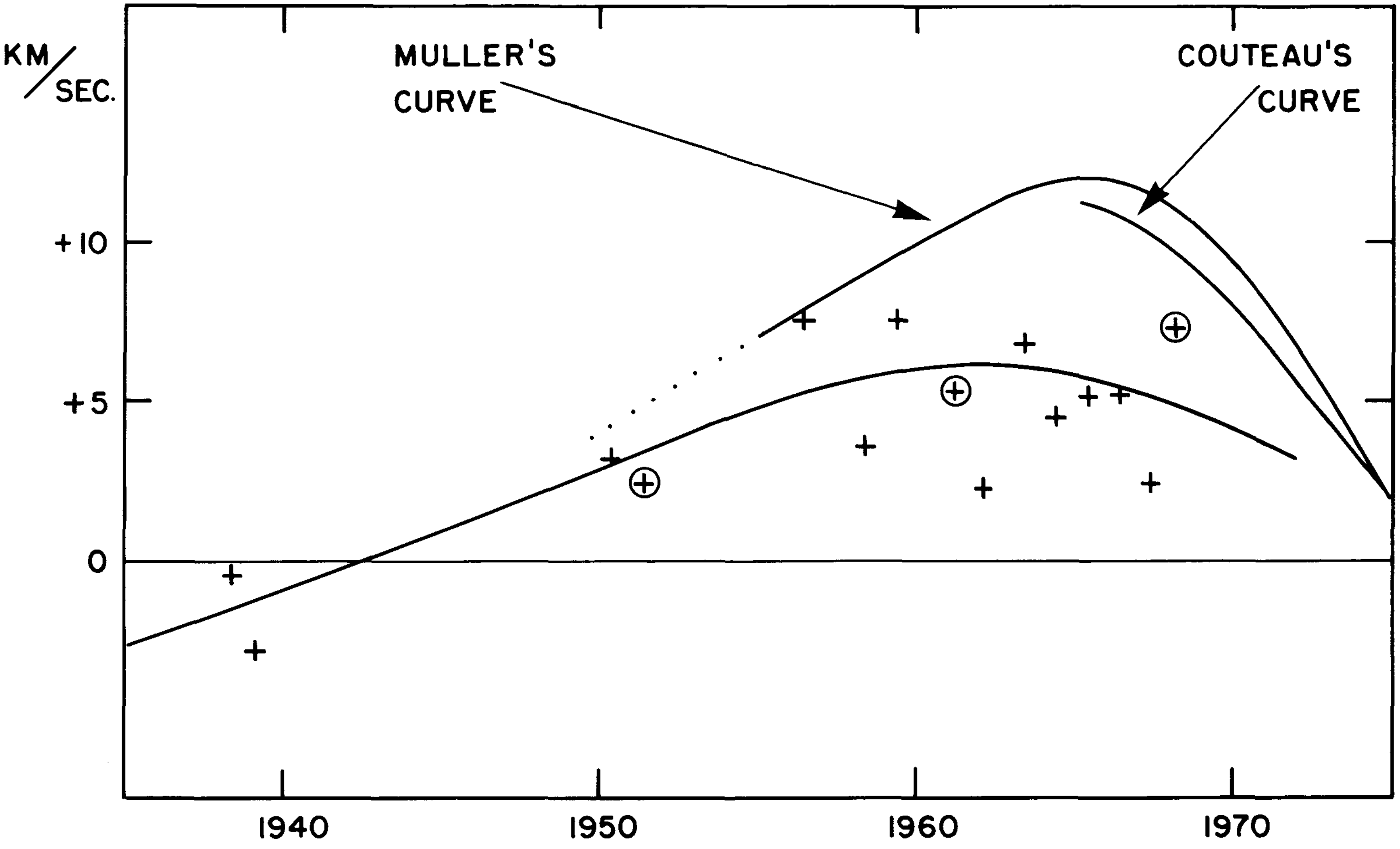

Figure 2. Predicted and observed velocity differences between the components of ADS 8189. Predictions from two different sets of visual orbital elements are shown. The three circled points are those considered best determined.

elements a", e, i, ω and P, therefore, the observation of V_{max} determines the parallax. In what follows, Couteau's values of the orbital elements have been assumed: it makes little difference if Muller's are adopted. The observed value of V_{max} corresponds to π = 0".029. This value appears to agree reasonably well with the trigonometrical parallax of 0".037, but either of these imply, in turn, a value of a (in astronomical units) that leads to a very implausible value, of about 0.4 m☉, for the total mass of the system. Since, to all appearances, each star is a normal main-sequence object of mid-F spectral type, we would expect the total mass to lie between 3 m☉ and 4 m☉, which implies a parallax of around 0".013. The spectroscopic parallax is even lower (about 0".01). The inconsistency cannot be removed by modifying the parallax alone, because unless we have very unusual stars mimicking main-sequence objects we have a strong constraint on the total mass. The observed value of V_{max} is about half that predicted from the likely total mass and the visual orbital elements. Changing the parallax changes the ratio a"/π" - i.e. a expressed in astronomical units - and we cannot find a value of the parallax that does justice both to our observations of V_{max} and our expectations about the mass.

In the 1969 discussion, I tended to assume that V_{max}, π" and a" were the only observables that would seriously modify equation (1), and since a" seemed to be fairly certainly known, the results appeared irreconcilable. The late W.S. Finsen, however, urged me both in correspondence and conversation to consider how much leeway there might be in both the spectroscopic and visual elements. (These urgings were one factor that led to Morbey's development of his method). Differencing equation (1) gives

$$\frac{\Delta V_{max}}{V_{max}} = \frac{\Delta a''}{a''} - \frac{\Delta \pi''}{\pi''} + \cot i \, \Delta i - \frac{\Delta P}{P} + E \Delta e$$

$$- \frac{e \sin}{1+e \cos} \, \Delta \omega ,$$

where $E = \dfrac{e(1+e \cos \omega)^2 - e \sin \omega \, (1-e^2)^{1/2}}{(1+e \cos \omega)\,(1-e^2)}$

Thus, $\Delta V_{max}/V_{max}$ is very sensitive to changes in i if i is small, and to changes in e if e is large. It will be sensitive to changes in ω only if e is large and ω close to 180°. We must, however, consider the effects of changes in these elements on the expected value of V_{max} if we are to reconcile visual and spectroscopic observations of ADS 8189.

If, despite the uncertainties discussed by Petrie and Batten, the observed value of V_{max} for ADS 8189 is accepted at its face value, and if a value (say 0".01 is adopted for the parallax, then equation (1) defines a relation between the otherwise independent elements a", e, i and ω. If Couteau's value of a = 0".41 is also adopted as being unlikely to be much in error, we obtain:

$$\frac{\sin i}{(1-e^2)^{1/2}} \, (1+e \cos \omega) = 0.4109. \qquad (2)$$

Equation (2) defines a volume (which I call the *solution volume*) in (e, i, ω) - space, and the solution for those three elements must lie within that volume. In other words, the observed values of a'' and V_{max}, together with the assumed value of π'', place constraints on the permitted combinations of e, i and ω. Without radial velocity observations there would be no such constraint. Both Muller and Couteau made their solutions before radial velocity observations were available; thus it is no fault of either that neither of their solutions is consistent with the radial velocities. Admittedly the radial-velocity data are of relatively low accuracy, and it is possible to throw all the blame on them. We can not obtain better radial velocities for another 70 years, however, and we should at least try to do justice to the best we have.

It is difficult to visualize the entire solution volume, but we can illustrate its cross-sections with each of the three co-ordinate planes, and these are shown in Figures 3, 4 and 5. Figure 3 shows the e - ω plane, with cos ω plotted rather than itself. The maximum permitted cross-section in this plane, of course, is the rectangle lying between $e = 0$ and $e = 1$ and cos $\omega = \pm 1$. The observed value of V_{max} rules out some combinations of high e and ω close to 180° which would require sin $i > 1$. (The observed value of V_{max} is *too large* to occur at the ascending node of such an orbit, and the node passed through in the mid-1960s is known to be the ascending node of the secondary's orbit.) Formally, any solution within the shaded area is consistent with the observed velocity, but, of course, the run of the positional observations will eliminate many possible combinations. Within the shaded area, the loci of points having a given value for i may be drawn. That for i = 56°7 (Couteau's value) is shown, while the cross marks his solution for e and ω. Their mutual inconsistency is obvious. Formally, a consistent solution can be obtained by changing i alone, but we would have to reduce it to 16°7. This would, of course, have an effect on the value of a'', but a ratio $a''/\pi'' = a$ has been locked into equation (2), so such a solution would be permitted by the expected total mass and observed value of V_{max}, provided it could be made to satisfy the positional measurements.

Figure 4 shows the cross-section of the solution volume with the i - ω plane. Again, cos ω and sin i have been plotted, rather than ω and i themselves. In this plane, only about half of the mathematically possible area is consistent with the observed radial-velocity difference and the assumed value of a''/π''. The actual values of i and ω lie outside the area of consistent solutions. This means that a consistent solution cannot be obtained by altering e alone. Figure 5, in which the cross-section with the e - i plane is displayed, shows that neither can a consistent solution be obtained by altering ω alone.

It is not possible to place such narrow constraints on the elements e, i and ω - at least for this system - that we can solve for the elements directly, but we have narrowed down the ranges in which those elements can lie. I would like to challenge those who are expert in the determination of visual orbits to try again, taking these constraints into account. Presumably the constraints would limit the way in which the initial apparent orbit could be drawn, just as the requirement that it should obey the law of

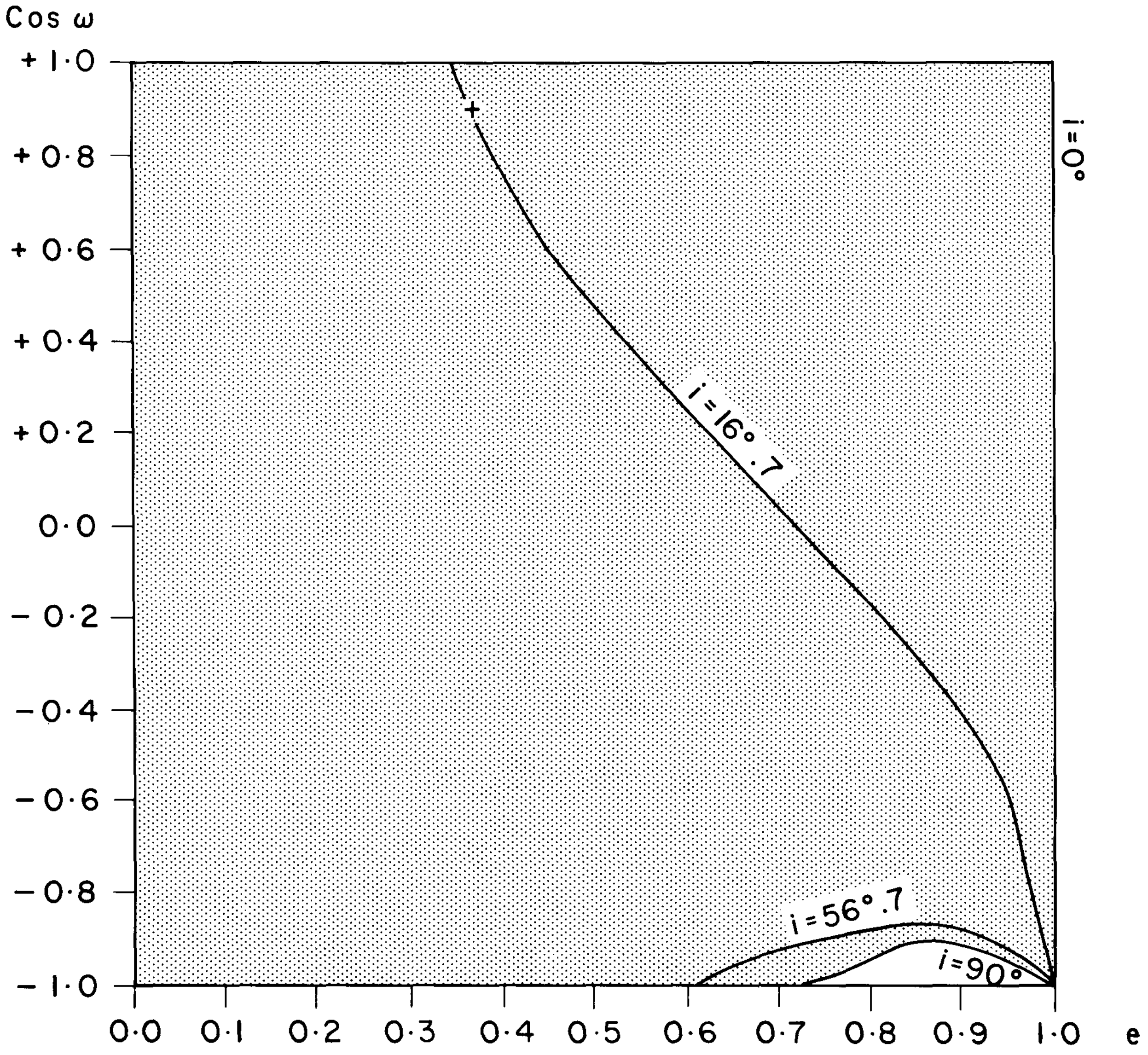

Figure 3. Cross-section of the solution volume for ADS 8189 (see text) with the e - ω plane. Any solution within the shaded area is theoretically consistent with the observed maximum velocity difference. The values of e and ω derived from the visual observations (marked with a cross), however, are not consistent with the value of i and the observed velocity difference.

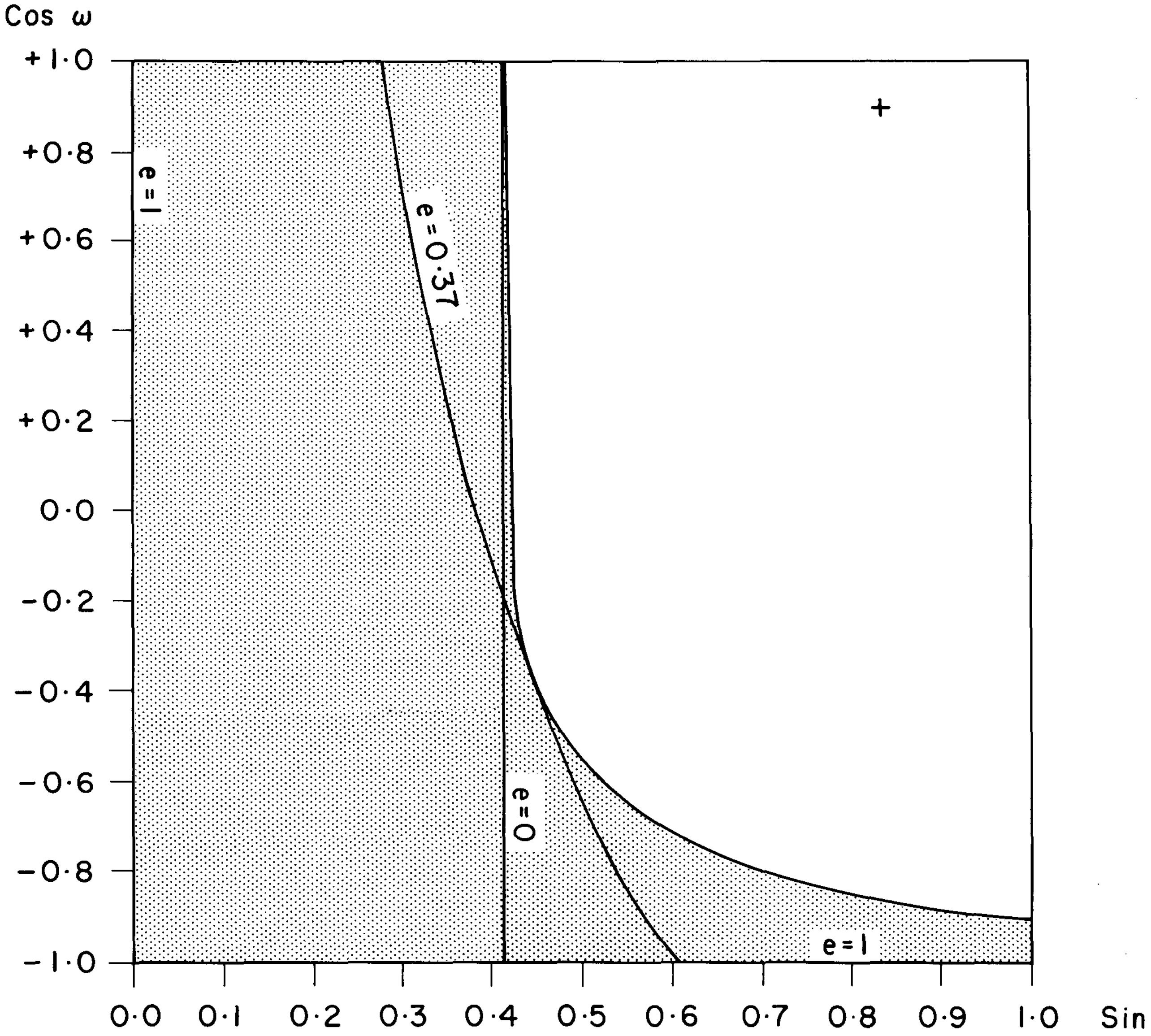

Figure 4. Cross-section of the solution volume for ADS 8189 with the i - ω plane. Again, a consistent solution must lie in the shaded area. No solution is possible by changing e only. (The cross marks the "observed" values of i and ω).

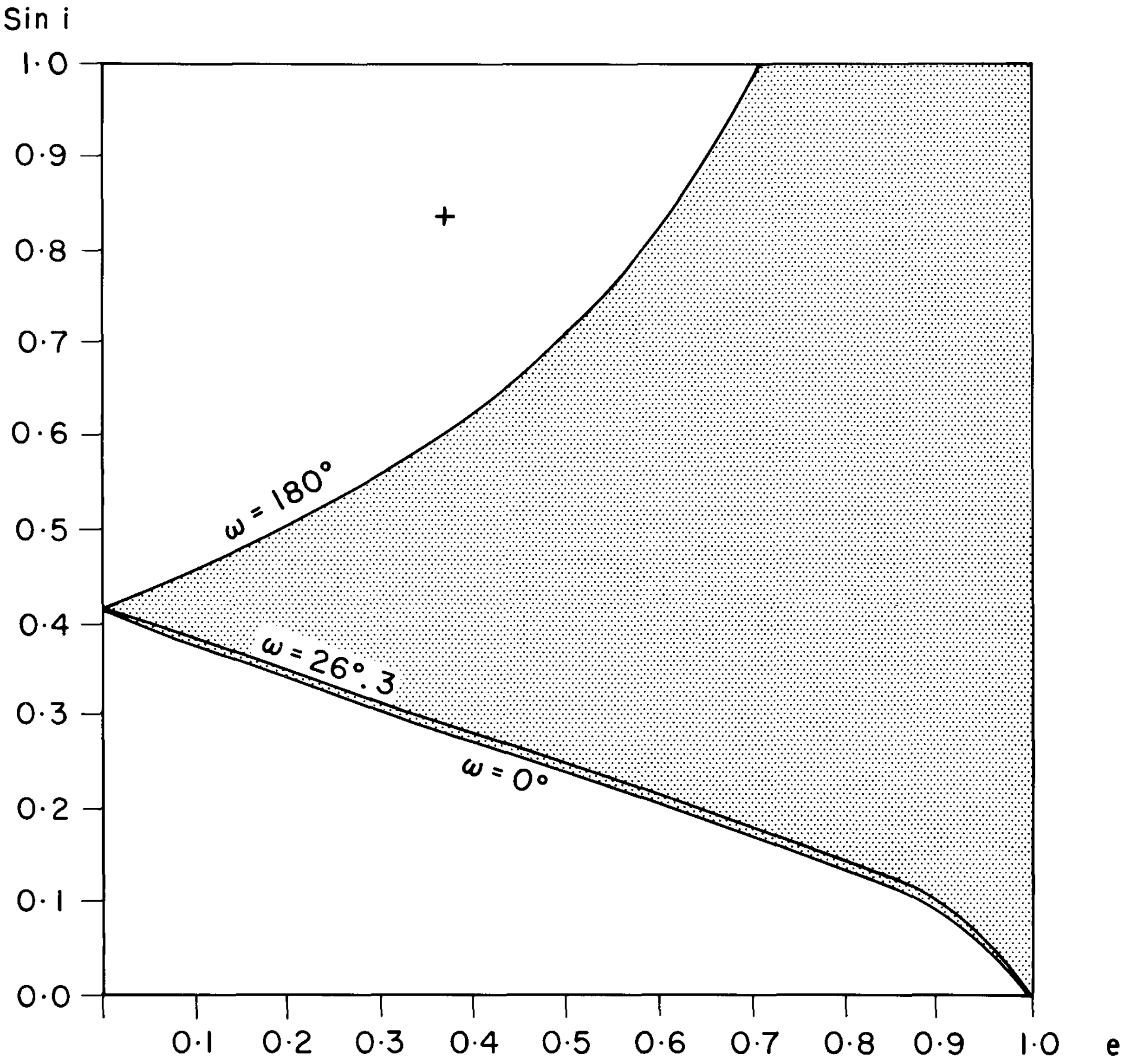

Figure 5. Cross-section of the solution volume for ADS 8189 in the i - e plane.

areas does. Of course, the solution volume does not have a sharp surface: some concession can be made for observational error, but the present solutions lie outside that range.

In summary, the only technique available now for the determination of the orbits of visual binaries that have also been observed spectroscopically is Morbey's. It should be used whenever possible. When, for any reason, that method is not practicable, progress should be made by using the less complete set of data (usually the radial velocities) to place whatever constraints it can on the other set (the positional measurements) and thus to converge to a solution that, as nearly as possible, satisfies all the available observations.

REFERENCES

Baize, P. 1950, J. Observateurs **33**, 125.
Batten, A.H., and Fletcher, J.M. 1971, Astrophys. Space Sc. **11**, 102.
Batten, A.H., Morbey, C.L., Fekel, F.C., and Tomkin, J. 1979, Pub. Astron. Soc. Pacific **91**, 304.
Beardsley, W.R., Gatewood, G., and Kamper, K.W. 1974, Astrophys. J. **194**, 637.
van den Bos, W.H. 1935, Union Obs. Circ. **4**, 148.
Couteau, P. 1965, J. Observateurs **38**, 59.
van Dessel, E.L. 1977, Bull. Astron. Obs. Roy. Belgique **9**, No. 1, 1977.
Fekel, F.C. 1981, Astrophys. J. (in press).
Fletcher, J.M. 1973, J. Roy. Astr. Soc. Canada **67**, 255.
Griffin, R.F. 1967, Astrophys. J. **148**, 465.
Hans, E.M., Scarfe, C.D., Fletcher, J.M., and Morbey, C.L. 1979, Astrophys. J. **229**, 1001.
van de Kamp, P. 1975, Astron. J. **80**, 658.
Morbey, C.L. 1975, Pub. Astron. Soc. Pacific **87**, 689.
Muller, P. 1955, J. des Observateurs **38** 59.
Petrie, R.M., and Batten, A.H. 1969, Pub. Dominion Astrophys. Obs. **13**, 383.
Petrie, R.M., Andrews, D.H., and Scarfe, C.D. 1967, in Determination of Radial Velocities and their Applications (Int. Astr. Un. Symposium No. 30 eds. A.H. Batten and J.F. Heard) London, Academic Press, p. 221.
Sterne, T.E. 1941, Proc. Nat. Acad. Sci. **27**, 175.
Wilson, R.E. 1953, General Catalogue of Radial Velocities, Carnegie Inst. Washington.
Worth, M.D., and Heintz, W.D. 1974, Astrophys. J. **193**, 647.

DISCUSSION

HARRINGTON: Your constraint can be added to the differential correction scheme in the same way I outlined for other constraints. Is not your constraint heavily dependent on the adopted "a" over pi, with an often lousy value of pi?

BATTEN: I don't think the "a" over pi is especially lousy, since it is essentially the total mass of the system, and as far as we can see, the system consists of three perfectly normal mid-F main sequence stars. The possibility exists, of course, that the solution to the problem is that they are very abnormal stars that happen to look like mid-F main sequence stars, but I think if I were to try to defend that possibility, I very soon would be shot down.