were concerned with was *astronomy* and the rôle of women in it. There was no mention of feminist or gender theory, and as several people commented, this made the meeting both refreshing and stimulating in so far as no one was attempting to bog things down in sociological quagmires, or alienate the audience by the use of jargon words and phrases. — ALLAN CHAPMAN

SPECTROSCOPIC BINARY ORBITS FROM PHOTOELECTRIC RADIAL VELOCITIES

PAPER 142: ξ URSAE MAJORIS

By R. F. Griffin
Cambridge Observatories

ξ Ursae Majoris is a well-known multiple star system consisting of a 60-year visual binary each of whose components is a single-lined spectroscopic binary; one of the sub-systems is also observed as an astrometric perturbation of the visual orbit. All three orbits (one visual, one spectroscopic, and one observed by both techniques) were quite well determined long ago. Recent interest in the system has, however, cast doubt upon the orbit that depends only upon spectroscopy and has indicated a need to refine the period of the other spectroscopic orbit in order to provide accurate comparison of the current phasing with that of its astrometric counterpart. Here the salient literature is reviewed and summarized in a 'user-friendly' fashion, and various apparent problems (including several not previously recognized) are identified and where possible corrected. By those means, and with the help of new and more precise radial velocities which by good fortune are taken near a periastron passage in the 60-year orbit, the orbits are confirmed and improved. This is the first paper ever to be based upon radial-velocity measurements that span more than 100 years. The velocity amplitudes in the visual orbit are determined for the first time, but reasons are given for doubting their formal accuracy. Although it is evident that this paper is far from being the last word on the subject, it does place our understanding of the system on a fairly satisfactory footing. Observers are offered two invitations: to look for eclipses in ξ UMa A, and to look carefully at the *K* line for signs of ξ UMa Bb.

Introduction — ξ Ursae Majoris as a visual binary star

ξ Ursae Majoris was discovered to be a visual double star by Sir William Herschel[1] on 1780 May 2*. He catalogued it[1] as H I 2, and helpfully described it (in Latin) as being in the Bear's right hind foot! Even *that* has been challenged: Admiral Smyth[3] asserts that ξ is in the Bear's *left* hind paw! Among the various problems that we shall show to beset ξ UMa, that is one upon which we can adjudicate with assurance: Bayer[4], whose star maps depict the constellation figures and carry his original designations of the Greek letters to the principal stars, shows beyond cavil that ξ is in the Great Bear's *left* hind paw. So Sir William is found guilty of an inexactitude!

Herschel reserved the category I of his double stars for very close pairs, and within each category he numbered them in order of discovery; the one previous class I star was ε Boötis. It was the re-observation, after a lapse of about 20 years, of the many pairs that he had measured around 1780 that convinced Herschel[5] that the relative movements seen in quite a number of them were of an orbital nature and that double stars were in most cases actual physical systems, constrained by the same law of gravitation as operated in the Solar System. In a further paper[6] on the same subject, Sir William explicitly discussed the case of ξ UMa, among others.

His son, Sir John, was one of the first people to compute orbits for double stars, and ξ UMa was the system that he selected for the initial demonstration[7] of his method, in 1831. Priority in that field, however, must go to Savary, who gave, in the *Connaissance des Tems* [*sic*] for 1830 (which according to its title page was published in 1827), a description[8] of a method of calculating orbits for double stars. In a supplementary paper[9] in the same volume, he opens by saying (in French, of course: the following is my translation), "It has occurred to me that it could be useful to add to [the formulae for computing orbits] their numerical application to a specific example. So I will suppose the following data, without attributing any reality to them:" — and he lists four dates with associated separations and position angles, all highly reminiscent of, but not identical with, actual observations of ξ UMa. Five pages later, at the conclusion of some stiff mathematics, he ingenuously remarks, "The data from which we started in the preceding calculations are very close to four observations of the double star ξ of the great Bear"; he goes on to tabulate all 11 of the observations available at the time, and to show that the computed orbit satisfies them very well. In fact Sir John Herschel[7], whose orbit was, on his own admission, not nearly such a good match, complained with good reason that Savary had cheated — not Sir John's word: that is my précis of the gentlemanly circumlocution that occupies nearly a whole page (his paragraph 64) — by choosing to solve data that were only *approximately* the observed ones!

Since ξ UMa is a bright and fairly equal system, with a period of manageable length (60 years), and not terribly close, the pair attracted the attention of numerous observers and 'personal computers' in the nineteenth century, and so many orbits were published of it that in 1906 Burnham was able to chronicle no fewer than 18 successive ones in his great catalogue[10] of double stars. The system is listed as Σ 1523 by Struve[11], BDS 5734 by Burnham, and ADS 8119 by Aitken[12].

Just too late for inclusion among the orbits cited by Burnham was one published in 1905 by Nørlund[13], who found that there was a tiny periodic fluc-

*The date is either corrected to, or more probably misprinted as, 1780 May 20 in Sir John Herschel's consolidated catalogue[2] of his father's double-star discoveries.

tuation in the residuals, with a period of 1·8 years. Such a fluctuation would find a natural explanation in an additional, unseen, component of the system. Dark components had already been proposed in a number of other systems; a few of them have turned out to be real, but more have proved to result from over-interpretation of what are actually observational errors. Indeed, See, who computed an orbit for ξ UMa in 1896 and published it both in the *Astronomische Nachrichten*[14] and also in a book[15], remarked that the orbit was already very well defined and accurate but that careful observations should continue because they "will be valuable in throwing light upon the question of the existence of dark bodies or other disturbing influences, ...". In the same year, in the *Astronomical Journal*[16] as well as in his book[15], See himself subscribed to the idea of there being a dark third component — whose existence few would believe nowadays — in the binary system 70 Ophiuchi. In the case of ξ UMa, however, Nørlund was absolutely correct, and deserves much credit for recognizing such a small and rapid perturbation; one reason that it had escaped attention previously (quite apart from its amplitude being only 0″·05) was that the usual scheme of combining the plentiful observations into seasonal 'normal points' for the convenience of calculation largely smoothed the perturbation out, and in any case left fewer than two points per cycle.

Nørlund pointed out that radial velocities would be of value in corroborating the short-period motion; he was evidently unaware that Wright, at the Lick Observatory, had already found the visual primary star to exhibit radial-velocity changes[17,18] on much too short a time-scale to be explicable in terms of the 60-year orbit. Quite soon, in 1908, Wright was in a position to publish a brief note[19] confirming the 1·8-year periodicity. With a view to determining good astrometric elements for the short-period orbit, Hertzsprung began a series of photographic observations of ξ UMa with the long-focus (20-inch *f*/25) Potsdam refractor in the inauspicious year of 1914. They proved to be much more accurate than visual measurements. Hertzsprung gave a preliminary result[20] in 1918, but the observations continued until 1923. In that year they were placed at the disposal of van den Bos, who undertook the heavy task of measuring them all (including those already measured by Hertzsprung). Van den Bos was also furnished with the Lick radial-velocity measurements, of which there were 42 of the visual primary. Armed, therefore, with all the best material, both astrometric and spectroscopic, he did it justice in an exhaustive treatment that he published[21] in 1928 in a Danish scientific journal that is none too easy now to find. His spectroscopic orbit of the primary is still the currently adopted[22] one; in fact, as will become clear below, there has unfortunately been no systematic radial-velocity work done on ξ UMa since shortly after the time of van den Bos's publication.

The astrometric orbits were re-discussed in 1995 by Mason *et al.*[23], who included a number of measurements made comparatively recently by speckle interferometry. As expected, those data (like the photographic ones) were substantially more accurate than the visual positions: Mason *et al.*'s Fig. 4 convinces the reader that the 1·8-year orbit is demonstrated directly by the individual observations rather than merely statistically. Even so, Mason *et al.* found it best to impose upon their orbital solution for the short-period subsystem the values of the period and eccentricity found spectroscopically seventy years ago by van den Bos; and in the light of the plot that they show of the residuals upon which the corresponding astrometric orbit is based (their Fig. 4) one cannot but marvel at the perspicacity of Nørlund in detecting the perturbation in the first place.

Mason *et al.* discovered an additional 'visual' component of the ξ UMa system; they associated it with the secondary star. It was detected in only *one* of their 27 speckle observations, of which 8 appear to have been made with 4-m-class telescopes. The observation that provided the lone detection was made at Kitt Peak (according to Tables 1 and 4 and §2 of the paper[23]) and at CFHT (according to §3·3). The newly discovered object seems never to have had any effect whatsoever upon the astrometric or radial-velocity behaviour of the observable components whose existence is reliably established.

Mason *et al.*'s paper[23] did not include, or even refer to, Hertzsprung's photographic work, whose precision appears to be comparable with that of the best of their own speckle interferometry. Reference to Hertzsprung's[20] Figs. 4a and 4b, and to van den Bos's[21] unnumbered Figure (exactly analogous to Mason *et al.*'s Fig. 4) on the fourteenth page of his paper, shows that the short-period motion is just as convincingly demonstrated by the photographic data as by the speckle measurements — and the former are more numerous. A discussion including the observations of all types was really needed, and was promptly supplied (albeit with characteristic terseness) by Heintz[24], who also pointed out a number of additional shortcomings in Mason *et al.*'s paper. (His criticism of Mason *et al.*'s Fig. 5 must really have been directed against their Fig. 4.) Heintz somewhat consistently[25,24], but in my view confusingly, adopts for the various components of the multiple star a nomenclature that differs from the one I regard as normal, which for avoidance of doubt I spell out here. The visual components are A (the brighter) and B, and they consist respectively of subcomponents Aa and Ab, and Ba and Bb. Heintz usually uses A to mean Aa, a to mean Ab, and Aa to mean either A or the orbit Aa–Ab!

It has long been appreciated that the orbits in a multiple star system are subject to perturbations, *i.e.*, the motion in each orbit causes secular changes, which can accumulate to large values, in the other(s). The case of ξ UMa was explicitly discussed long ago by Brown[26], and has been treated again in three subsequent papers, those of Heintz[25,24] (already mentioned) and of Ling, Docobo & Abad[27]. The last shows that the most rapid change is in the longitude of periastron of the 1·8-year orbit, which is expected to decrease by several degrees per century. It is a disappointment to discover in retrospect that the inclination of the orbit of the 1·8-year A sub-system to the line of sight ought, in theory, to have risen through the value of 90° about 50 years ago, when the components should have undergone a series of mutual eclipses, enabling the nature and luminosity of the unseen object Ab to be established. The actual determinations of the inclination hardly corroborate that expectation: van den Bos[21] found the inclination already to be significantly above 90° (95°·5 ± 2°·4) from data taken about 1920, whereas Heintz[25] in 1967 gave $i = 84°·5$ and in his recent paper[24] gives $i = 91°$ from an ensemble of observations whose weighted mean date must be much later. Heintz seems concerned with perturbations only as regards the outer orbit, whose variations he finds to be approximated by discontinuous changes at each periastron passage. Graphs presented by Ling *et al.*[27] to illustrate the variations of orbital elements with time also show the changes to occur mainly at periastron in a rather stepwise fashion. There seems to be enough uncertainty as to the exact value of the present inclination to warrant the note, in the discussion below, of forthcoming dates of conjunction, when eclipses might be sought.

Photometry

It may cause wry amusement that the magnitudes of ξ UMa A and B in the *Bright Star Catalogue*[28] ($4^{m}·41$ and $4^{m}·87$ respectively) are exactly those that appeared in the fore-runner of that *Catalogue*, the *Harvard Revised Photometry*[29], published in 1908! Not only that, but even *those* magnitudes were quoted from still earlier works: the integrated magnitude of the pair was determined by repeated measurements with the successive meridian photometers at Harvard[30,31], while the magnitude difference of $0^{m}\cdot46 \pm 0^{m}\cdot10$ was the mean value obtained by Pickering, Searle & Upton[32] with the Harvard double-image photometer in 1879. In that year Crossley, Gledhill & Wilson[33] gave the magnitudes of ξ UMa as $7^{m}\cdot3$ and $8^{m}\cdot2$, so the Harvard magnitudes were certainly an improvement!

The integrated magnitude found for the pair at Harvard was $3^{m}\cdot86$; there are now several *UBV* photoelectric measurements[34–40], which are in close agreement with $V = 3^{m}\cdot79$, $(B-V) = 0^{m}\cdot58$, $(U-B) = 0^{m}\cdot03$, with the exception of one by Nikonov *et al.*[35], who gave $V = 3^{m}\cdot89$. When the *Photoelectric Catalogue*[41] was compiled, that discrepancy was conspicuous, and was duly noted[42] by one of the compilers of the *Catalogue*. As a further result, ξ UMa then featured (as NSV 05165) in the *New Catalogue of Suspected Variable Stars*[43]. Macroscopic variability of ξ UMa had also been suggested by Jackisch in a paper[44] whose reliability the present writer would not care to certify.

Strassmeier *et al.*[45] reported on a photometric monitoring campaign, spanning three consecutive observing seasons, with an automated telescope; the integrated light of ξ UMa appeared to show slow variations amounting to a few hundredths of a magnitude. The authors attributed the variations to movement of spots on the surface of the fainter visual component, which is known to have an active chromosphere. They proposed a period of 810 days on the basis of a periodogram, but they gave no reason to suppose that the slight and seemingly capricious variation would repeat with that (or any) period, nor were they able to identify any rotational modulation associated with the postulated spots. If the secondary star, which contributes about 2/5 of the total light of the system, was indeed the seat of the apparent changes in the integrated light, its own variations must reach at least $0^{m}\cdot1$ peak-to-peak. Strassmeier *et al.*'s paper carried conviction that ξ UMa is a variable star, which accordingly featured (as ξ UMa!) in the *70th Name List of Variable Stars*[46]; it is there *said* to have a *V* magnitude of $4^{m}\cdot38$ and a range of variation of only $0^{m}\cdot01$ and to be of type 'RS'. A note calls attention to the fact that it is a visual binary and suggests that it is the B component that is variable. Miss Hoffleit[47] subsequently included ξ UMa in a *Catalogue of Bright Eclipsing and Ellipsoidal Variables*: the system is listed as a whole, as a variable of type RS, and the individual visual components, designated ξ^1 and ξ^2, each feature separately. No type of variability is given for ξ^1, while ξ^2 is decribed, rather surprisingly, as an ellipsoidal variable with a period of 810: days and as having an angular separation of $0''\cdot056$. (That angle is actually the semi-axis of the orbit of the *other* visual component, Aa, around the centre of gravity of itself plus its unseen companion Ab.)

The determination of magnitude differences for double stars has been a notoriously difficult problem. Both Burnham[10] and Aitken[12] listed the magnitudes of the ξ UMa pair as $4^{m}\cdot0$ and $4^{m}\cdot9$, so they considered the difference to be twice as great as the Harvard observers found by measurement. Hertzsprung[48] performed photometry on two of his astrometric plates, taken in 'visual' light, and found that the magnitude difference was $0^{m}\cdot393$. In a paper reporting a substantial empirical investigation of systematic errors, as a function of angular separation, of estimates of the magnitude differentials of double stars, Baize[49] asserted that Burnham tended to exaggerate magnitude differences; he (Baize) concluded that Δm for ξ UMa is $0^{m}\cdot43$. He also quoted Kuiper as finding a value of $0^{m}\cdot43$ by his objective-grating procedure[50], and Muller as obtaining

0ᵐ·46 with his double-image prism. In fact Muller[51] not only obtained $\Delta m = 0^m{\cdot}46 \pm 0^m{\cdot}02$ as the mean of 13 measurements by that technique, but he also attempted to measure magnitude differences in different colours; for ξ UMa he found $0^m{\cdot}46 \pm 0^m{\cdot}02$ through a filter centred at λ5400 Å and $0^m{\cdot}41 \pm 0^m{\cdot}03$ through one centred at λ6100 Å. Wallenquist[52] considered that the mean of the values found by the Harvard observers[32], and Hertzsprung[48], Muller[51], and Strand (the last unpublished) is $0^m{\cdot}48 \pm 0^m{\cdot}01$. That is exactly the value later found by van Herk[53] by Muller's method. Rakos *et al.*[54] measured Δm in U, B and V with their 'area scanners', which could be expected to give reliable results. Observations on one night gave $\Delta V = 0^m{\cdot}52$ and the colours of the stars similar to one another, and on another $0^m{\cdot}63$, with the secondary star somewhat redder. Internal evidence in Rakos *et al.*'s table of results for more than 200 stars arouses misgivings such that the entries for ξ UMa are regarded here with reserve.

It seems, therefore, in summary of the above discussion, that there is a reasonably good consensus on a magnitude difference very near to $0^m{\cdot}46$ between the two stars, and if B is indeed slightly variable we cannot hope to get a value that will always remain perfectly accurate. Application, then, of that value of Δm to the integrated magnitude of $3^m{\cdot}79$ yields the separate magnitudes of $4^m{\cdot}34$ and $4^m{\cdot}80$ as the best that can be offered at present for the component stars. One catalogue[55] (a compilation, not original data) that gives the magnitudes as $3^m{\cdot}79$ and $3^m{\cdot}80$ is evidently mistaken.

HIPPARCOS[56] has made a splendid contribution to the Δm problem by providing accurate values for nearly all 'visual' double stars brighter than about 8^m and having separations above about $0''{\cdot}25$. Unfortunately it got so confused by the curious epicyclic motion that it presumably saw for ξ UMa A that it seems to have thrown in the towel altogether: it declined to divulge any astrometric data at all, or even to furnish the magnitude difference! Of course it is true that orbital motion, especially in systems having periods of the order of a year, will tend to confuse parallax meaurements. That was pointed out by Wright[57] in 1904, and further emphasized subsequently both by him[58] and by Vinter Hansen[59], as well as being mentioned in the *HIPPARCOS*[56] introductory text (vol. 1, section 1·4·2·(2)(b)). In his second paper[58] Wright remarked that "Academically speaking, the problem is of course susceptible to simultaneous solution for the parallax and the dimensions of the orbit, but in view of the relatively large errors of observations it seems probable that particular methods of attack, adapted to one case or another, will be found more satisfactory". Two-thirds of a century later, with the benefit of generous numbers of observations of much increased accuracy, *HIPPARCOS* has realized in very many instances exactly the possibility that Wright so clearly foresaw, but ξ UMa, with its two superimposed orbital motions, proved too much for it. Stearns[60], however, already in 1924 managed to disentangle the orbital motions from the parallactic ones, and actually determined the elements of the 1·8-year orbit in the course of his derivation of ξ UMa's parallax. Doubtless the 88 observations that *HIPPARCOS* admits to having made of ξ UMa would produce a fine orbit for the astrometric sub-system — to say nothing of Δm — so a specific effort to work up the raw *HIPPARCOS* data on ξ UMa might well be a rewarding project. Presumably, photometry is in principle available for the independent stars, so the origin of any variability could be attributed with certainty. The published range (ref. 56, 7, p. 1111) of the joint brightness offers little scope, however, for any variations.

Spectral types

Simbad has a long list of spectral classifications, *all* of which are G0 V, for both stars; unfortunately *Simbad* does not distinguish between papers in which actual classifications are made and those that just quote types given by other sources. That shortcoming is particularly acute in the case of ξ UMa, where many citations are incorrectly listed as classifications but all the more recent and more perceptive classifications are omitted. The original *MKK Atlas*[61] of 1943 listed ξ UMa as a standard for type G0 V, noting that the type referred to the integrated light of the system. It is not clear to what extent subsequent classifications can be regarded as independent, at least where they reproduce the classification for which ξ UMa is a standard; but for what it is worth we note that the G0 V classification was given by Miss Roman[62], and later by Cowley, Hiltner & Witt[63] who specifically assigned it to each of the visual components individually. On the other hand Wilson[64] certainly took an independent line, classifying both components as G5 V. Shortly afterwards, however, in 1964, he[65] said that "Spectrograms in the blue show a slight difference in spectral type in the sense consistent with the apparent magnitudes". The earlier paper actually has a half-tone picture that includes reproductions of spectrograms of the two stars; but it is hard to judge small differences in type between them, because they are not well matched for photographic density.

Later, it became more generally agreed that the components are not, in fact, of identical types: the fainter one is also later in type, as Wilson said and as is to be expected if both are on the main sequence. That is clearly shown in the spectrum tracings published in 1987 by Bopp[66], who considered the types to be G0 V and G5 V, and by analogous ones subsequently published by de Strobel *et al.*[67]. It was, however, already known in 1983 to Soderblom[68], who quoted types of F8·5 V and G0 V (for components A and B respectively) privately communicated by Keenan, and of F9·5 V and G2 V by Harlan. Keenan & Yorka[69] then published the G0 V type for B alone, saying nothing about A; later they[70] revised it to G2 V and gave F8·5 V for A, types that were repeated in Keenan & McNeil's[71] catalogue of standards. In the *Eighth Catalogue*[22] of spectroscopic orbits Bopp's types of G0 and G5 V are selected, but unfortunately their assignments to the respective components are reversed.

Interestingly, the difference in spectral type had been appreciated long before MK types were invented. Thus Adams & Joy[72] initially classified the A and B components in 1917 as F9 and G2 respectively; but in the paper[73] in which they first published spectroscopic parallaxes from Mount Wilson, they gave them as F9 and G1, with absolute magnitudes of $5^{m}\cdot0$ and $5^{m}\cdot6$. In 1923 they[74] revised the types back to F9 and G2 and the luminosities to $4^{m}\cdot9$ and $5^{m}\cdot3$; but in 1935, without obtaining any new spectra — at least, not as far as Abt[75] was able to discover — they[76] reverted to calling both stars G0, and gave their spectroscopic absolute magnitudes as $4^{m}\cdot3$ and $4^{m}\cdot6$. In each of the three spectroscopic-parallax papers they said they were quoting the Harvard apparent magnitudes, but they did not do so very reliably: in 1917 they gave $4^{m}\cdot0$ and $4^{m}\cdot9$, in 1923 $4^{m}\cdot4$ and $4^{m}\cdot9$ (the correct values), but in 1935 they unaccountably*

*They may have been hoping to 'correct' the observed magnitudes to take account of light they attributed to the spectroscopic companions accompanying each visual component; if so, they grossly *over*-corrected, since there is no evidence (or, at least in the case of A, likelihood) that either of those secondaries contributes appreciable luminosity.

gave $5^m.0$ and $5^m.6$, thereby deriving a spectroscopic parallax that was considerably too small.

Kuiper[77] seized on the seemingly rather modest discrepancy between that parallax and the trigonometrical one, which he quoted as 0″·138 with the optimistically small 'probable error' of 0″·006, to assert that ξ UMa is a 'sub-dwarf'. The system was of course included in the *Catalogue of Nearby Stars* (as Gl 423) by Gliese[78, 79], who gave[79] the trigonometrical parallax as 0″·127 with a more realistic 'probable error' of 0″·017.

Mount Wilson was not the only observatory with an interest in spectral classification and luminosity determination. Young & Harper[80], writing from Victoria in 1924, gave the types of ξ UMa A and B as F8 and G2 and their absolute magnitudes as $5^m·1$ and $4^m·8$ respectively. At the Norman Lockyer Observatory, Rimmer[81] found an absolute magnitude of $4^m·6$ for A, which he rather confusingly called B (for *Brighter*!).

The literature that refers to the spectral types and luminosities of the stars, then, though exhibiting minor discrepancies in detail, unanimously affirms that ξ UMa consists of a somewhat unequal pair of stars having luminosities that would put them close to the main sequence, the primary being somewhat earlier than solar type and the secondary close to solar. We note that the apparent V magnitudes of $4^m·34$ and $4^m·80$ are very close to the *absolute* magnitudes normally attributed to stars of the types concerned, so a spectroscopist is led to expect the distance of the system to be very near to 10 pc ($\pi \sim$ 0″·100). The most recent trigonometrical parallax determination[82] offers support to such a value, being 0″·101 ± 0″·005.

One hardly needs to be clairvoyant to understand how it came about that the difference in the spectral types became 'forgotten' in the 1930s (Morgan, in the preliminary paper[83] in which he explained the luminosity-classification nomenclature for the first time, went back to a joint type of F9+V in 1938), not to be recalled again for decades: changing angular separation. Between about 1904 and 1924 ξ UMa AB was near apastron, the components separated by 2″·5–3″, so people who were optimistic and/or observed from sites with reasonably good seeing could observe them separately. In the 1930s the separation was only 1″–1″·5: even at the Lick 36-inch refractor, where observers such as Hussey, Burnham, and Aitken regularly discovered and measured double stars of amazingly small separations, Berman[84] lamented that as from 1929 May 30 the components of ξ UMa could not safely be separated on the spectrograph slit and were not likely to be again before 1940. After periastron passage there is a long interval when the separation hovers in the 1″·5–2″ régime, but at last in 1964–1984 there arrived another time of comparatively wide separation — whereupon separate classifications came once again to the fore.

Published radial velocities and orbits

Radial-velocity observations of the brighter component of ξ UMa began at Lick early in 1897, very soon after the commissioning of the *Original Mills Spectrograph*[85] on the 36-inch refractor. By 1900 the reality of velocity variations was established and was duly announced[17,18]. The tiny astrometric perturbation discovered in 1905 by Nørlund[13] was found[19] by 1908 to correspond with the radial-velocity changes, the period being about 1·8 years. The spectroscopic orbit was determined, on the basis of the accumulating Lick data, by Abetti[86] in 1918; as noted in the *Introduction* above, it was improved on the basis of a comprehensive discussion of extensive astrometry as well as an increased number of radial velocities by van den Bos[21], who in 1928 had what is still regarded[22] as the last word on the radial-velocity side of the matter. He utilized 42 velocities from Lick and four published by Küstner[87] from Bonn. Unlike Abetti, in his solution of the 1·8-year orbit he accounted for the variation of velocity attributable to motion in the 60-year orbit; that variation was, however, not great during the time interval covered by the radial velocities, which was around apastron in the outer orbit.

Lick velocities of the fainter component did not begin until 1902. Although a substantial discordance arose already at the second observation, made only a fortnight after the first, and a still more glaring one occurred in 1909, it was not until 1918, after six plates had been taken in a single observing season and had thereby almost doubled the total number available, that the variability was announced[88]. A period of "about 10 days" was suggested in Moore's *Third Catalogue of Spectroscopic Binaries*[89] in 1924, even though the observational material available at that time does not appear adequate to support any such estimate; van den Bos[21] referred in 1928 to the period as being "nearly 9 days". The true period of just under four days was established by Berman, on the basis of 47 Lick velocities that included a spell of rather intensive observations in 1927. Berman presented his work in summary form[90] in 1930 and in detail[84] in 1931; his orbit is the only one that has ever been published for ξ UMa B.

An idiosyncrasy in the expression of the orbital elements, coupled with insufficient vigilance on the part of catalogue compilers, has caused some confusion regarding that orbit. Berman adopted a circular solution, and instead of using the usual convention of giving as the epoch an instant T_0 of maximum velocity, he fixed the epoch at a round number of Julian days and specified the *phase* at that epoch as if it were a longitude of periastron, ω. The successive catalogues of binary orbits[91–94,22] have omitted to quote that important quantity, although each has a note (separated by many pages from the actual catalogue entry) that the epoch is arbitrarily fixed. In one case[93] only, the note offers a derived value of T_0.

Berman appears to have made an error that has not been remarked upon by authors who have referred to his work hitherto. His perception of the nodes of the orbit is the reverse of the present writer's: indeed, whereas the text of his paper (§II A) says that the B system was *approaching* us throughout the interval 1897–1931, his Table 1 shows (as does Fig. 2(a) below) that the γ-velocity of B was above the systemic velocity of the whole system ξ UMa AB throughout the observations of 1902–1929 — so B was actually *receding* all the time. It follows that all the corrections to times of observation for the light-time across the 60-year orbit are applied with the wrong sign. Fortunately, the small amplitude of the 4-day velocity variation renders the error of little consequence.

The radial-velocity values utilized by Berman[84] are very similar to, but not identical with, those published in 1928 by Campbell & Moore in the great compendium[95] of Lick velocities. The differences, typically of 0·2 km s^{-1}, appear to represent corrections discussed in their introductory text by Campbell & Moore and applied systematically by them to all velocities. The corrections include a systematic one for G-type stars, and one[96] for flexure of the telescope tube, both of which Berman specifically says that he has disregarded. Van den Bos's[21] data differ considerably more from the corresponding Campbell & Moore entries, the latter being more negative by as much as 1 km s^{-1} in several instances. It seems likely that the Campbell & Moore values are the authoritative 'final' values, and they are the ones used in the discussion below. It may be mentioned here that the civil date in the third entry of van den Bos's

Table 1 is misprinted — it should be 1899 Febr. *22*. On the other hand, van den Bos appears to have corrected a discordance that exists in Campbell & Moore between the civil and Julian dates of the observation that both attribute to 1909 Feb. 25.

The pre-1930 Lick velocities used to such good purpose by van den Bos[21] and Berman[84] still constitute the large majority of the data available on ξ UMa, but occasional values have been published from other observatories from time to time. In several cases it is not obvious whether the object was observed as if it were a single star, or whether the measurement refers to one component alone; and in the latter case, it is not always certain *which* component is intended. It is slightly counter-intuitive that the fainter component has the earlier HR and HD numbers (4374 and 98230 respectively), the brighter having the next one in each case. It is clear that certain authors[97–100] have inadvertently used the designation of the secondary when they intended the primary or the unresolved system; in all too many other cases it is *not* clear what is intended. One author[101] even refers to ξ UMa B when the context shows that he means ζ UMa (*i.e.* Mizar) B; others[102] use ζ UMa to mean ξ.

Küstner's[87] four velocities were published in 1914. They were followed by two from Ottawa[103] (wisely omitted from consideration by van den Bos). Harper[104] published two velocities of ξ UMa from Victoria in 1934; they are assigned to HD 98231 and the magnitude is given as $4^{m}\cdot41$, which seems to confirm that they are measures of the A component alone. Difficult though it may be to believe that the ξ UMa pair could have been observed separately at Victoria, the fact remains that in 1924 Young & Harper[80] actually published separate line strengths and spectral types for them — so they *must* have been! The two plates were taken in 1921 and 1922, when the system was close to apastron and almost 3″ apart. A careful reading of the introductory texts of the 1924 and 1934 papers[80,104] does nothing to dispel the idea that the plates measured for velocity[104] were probably the selfsame ones, of dispersion 30 Å mm^{-1} at Hγ, as had been taken for the earlier spectroscopic work[80]; but that leaves open the question as to why, then, Harper did not measure the plate(s) that supposedly must have existed of the B component as well.

Radial velocities measured from the plates taken at Mount Wilson for the spectroscopic-parallax work of Adams & Joy and their collaborators[73,74,76] were published after a lapse of 30 years by (R.E.) Wilson & Joy[105] — but even then only as a mean value. A further 20 years passed before Abt[75] provided their individual details. At Mount Wilson there would of course have been no difficulty in observing the two stars separately; even in the 1950s, when the separation was only 2″, Struve & Zebergs obtained (and published[106] velocities from) 5-Å mm^{-1} spectra of both components individually at the 100-inch coudé.

Beavers & Eitter[97] made 17 measurements of ξ UMa with the radial-velocity spectrometer at the Erwin W. Fick Observatory of Iowa State University at Ames. The HR and HD numbers of the object to which they are attributed are those of B, but a standard footnote shows that the authors were aware that more than one star was on the entrance slit during the observations. The seeing at Ames must be altogether astounding, because Beavers & Eitter specifically draw attention to contamination of their observations by the unwanted components in the cases of ζ Piscium, Mizar, and γ Delphini, double stars whose angular separations are 24″, 14″ and 10″ respectively!

Duquennoy, Mayor & Halbwachs[107] gave eight radial velocities obtained with the Haute-Provence *Coravel*. It is not possible to observe the components separately with that instrument, and in no case were the twin velocities sufficiently disparate to permit resolution of the 'dip' into its two components.

Four velocities were measured from Hα reticon spectra obtained at the Pine Bluff Observatory of the University of Wisconsin by Eker, Hall & Anderson[99], who attributed them quite specifically to ξ UMa B. They complained that their measurements did not fit Berman's[84] orbit of that star, and noticed that Beavers & Eitter's[97] velocities do not fit it either. The Hα profiles look more like that of A than of B; indeed, the authors themselves say that the profiles "appear normal". It will be demonstrated below (Table III) that Eker *et al.*'s velocities fit the orbit of A somewhat less badly than that of B; they are evidently derived from blended spectra of *both* the ξ UMa stars, as indeed Beavers & Eitter admit *theirs* to be. Since neither set of velocities is actually of B, the incompatibility with the orbit — which, as we shall show below, is still very good – of that star is not altogether surprising. The inescapable conclusion is that the proximity of A to the star of interest to them must have escaped the notice of the observers altogether. A relatively minor problem is that the civil dates given for the observations are one day out of register with the Julian dates that are also tabulated. Eker *et al.* say that it is clear that a new period analysis of the orbit of B is needed, and that they are undertaking a study to that end.

New radial velocities

Early in 1993 Dr. H. A. McAlister, who had been making speckle-interferometric observations of ξ UMa, invited me to obtain radial velocities of the A component in order to establish the phasing of the 1·8-year spectroscopic orbit more accurately than it could be extrapolated from the elements given in 1928 by van den Bos[21]. Unfortunately I was unable to comply with that request within the time-scale on which Dr. McAlister's group wished to publish their own ideas about ξ UMa, so this present paper should be seen as a belated 'make-weight' to theirs[23]. Patience would, however, have had *some* virtue: they settled in their paper for an epoch of periastron — the quantity that they asked me to establish — that differs from the one found below by fifty standard deviations! A mistake upon which Heintz[24] forbore to comment concerns the size of telescope that would be needed to resolve the A sub-system in the infrared, where the Δm would be somewhat less unfavourable than in the visible. The angular distance that Mason *et al.*[23] treat as the separation is actually the distance of the component Aa from its centre of gravity with Ab. The mass function together with the known high inclination demonstrates that Ab has only approximately $^2/_5$ of the mass of Aa, so it is $^5/_2$ times as far from the centre of gravity. The angular separation is therefore about $3^1/_2$ times as great as Mason *et al.* think, so the system could be resolved by a telescope whose aperture is smaller than they say by that factor.

At the time that Dr. McAlister made his request I was wholly reliant on the *Coravel* at Haute-Provence for observations, so I observed it there; and being unable to measure A alone I necessarily obtained traces blended with the B component. In view of the known 4-day period of B, I tried to measure the system on four consecutive nights in each run, in the hope that that might facilitate the reduction of the traces to give the individual velocities of the two stars. Also, after the first occasion when the seeing was sufficiently good to enable the duplicity of the object to be seen in the finding eyepiece, the *Coravel* was always rotated so as to align the components *along* the slit in the hope of ensuring that the same fraction of the light of each would be transmitted and the blended dips in the resulting traces could be assumed always to have the same relative strengths. In that way a total of 56 new measurements was obtained. Dr. S. Udry, who kindly performed the reductions of the observations, was able to resolve 30 of those traces (together with one of the eight previously published[107] as a blend) to give the radial velocities of the components individually. Because that was only practicable when the twin velocities differed sufficiently ($\Delta v \gtrsim 7$ km s^{-1}), the results do not provide uniform phase coverage of the 1·8-year orbit of the A sub-system.

In the 1996/7 observing season, the Cambridge *Coravel* was sometimes available, and 14 measurements of ξ UMa were made with it. The seeing at the Cambridge 36-inch reflector is normally good, and on most nights the components of ξ UMa, which at that time had an angular separation of about 1″·5, could be observed separately. On occasions when they could not, no observation was made, so no question arose of having to disentangle blended traces. Although there was therefore no instrumental restriction on phase coverage of the 1·8-year A orbit, the Cambridge observations are bunched in phase because they were all taken within a comparatively short interval (the principal concern then being with component B); they are, however, near the opposite node of A to the one best covered by the Haute-Provence data.

The *Coravel* observations are set out in Table I. To save space, none of the previously published observations is tabulated. Such negligence may perhaps be excused on the grounds that (*i*) the new treatment largely confirms the orbits long since derived from the Lick observations, so the tables in the relevant papers[21,84] may serve nearly as well, and (*ii*) all the other data[87,97,99,103–106] are so fragmentary, and in so many cases refer knowingly or otherwise to blended spectra, that they have not been deemed useful for present purposes. Even the Haute-Provence *Coravel* observations that could not be reduced to give the separate velocities for the components are not tabulated, because — as is explained more carefully in the next section — there is little of value that can be extracted from unresolved traces. In several cases the velocities computed for the two components at the time of an unresolvable observation are identical within 1 km s^{-1}, and it is tempting to include the observation, just as it stands, in both orbital solutions; but it has been concluded, with some regret, that there would be significant impropriety in thus reinforcing solutions 'retrospectively'.

It is an unfortunate but seemingly unavoidable fact that radial velocities from different observatories tend to exhibit mutual discrepancies of zero-point. In the determination of orbits, such discrepancies can usually be removed quite accurately through evidence gained from the orbital solution itself — empirical adjustment of the zero-point of one set of velocities costs one degree of freedom, which is a negligible price to pay for homogeneity, although of course the procedure does call into question the external accuracy of the resulting γ-velocity. In the present case, the fact that the three different sets of data cover different ranges of phase in the outer orbit creates unusual uncertainty, as also does the provenance of one set from a source for which there is little experience to act as a guide. The writer's best judgement has led him to adjust the Haute-Provence velocities by +0·4 km s^{-1} from the values supplied from Geneva, and the Cambridge observations, which were initially reduced differentially against the Cambridge reference stars in the same way[108] as with the original spectrometer, by −1·0 km s^{-1}, the intention being to make both sources homogeneous with the Lick data.

TABLE I

Coravel radial velocities of ξ Ursae Majoris A and B

Date	*HMJD (B)*†	*Velocity* A	*Velocity* B	*Phases* Outer	*Phases* A	*Phases* B	*(O − C)* A	*(O − C)* B
		km s⁻¹	*km s⁻¹*				*km s⁻¹*	*km s⁻¹*
1986 Dec. 2·23	46766·21	−6·6	−17·6	0·864	0·071	0·492	+0·3	+0·1
1993 Dec. 25·11	49346·08	−14·5	−21·9	0·982	3·920	648·619	−0·1	−0·3
28·08	349·05	−14·5	−20·4	·982	·924	649·365	−0·4	+0·9
29·10	350·07	−13·8	−22·2	·982	·926	·621	+0·1	−0·6
1994 Jan. 1·22	49353·19	−13·5	−22·2	0·983	3·930	650·405	0·0	−0·1
Feb. 16·10	399·07	−1·9	−14·0	·985	·999	661·931	+0·5	−0·1
17·07	400·04	−1·9	−16·0	·985	4·000	662·175	+0·3	+0·1
18·13	401·10	−1·7	−22·1	·985	·002	·441	+0·2	+0·7
19·08	402·05	−1·4	−19·6	·985	·003	·680	+0·3	+0·7
19·90	402·87	−1·0	−14·8	·985	·005	·886	+0·6	−0·2
21·02	403·99	−2·2	−18·0	·985	·006	663·167	−0·9	−2·1
Apr. 29·92	471·90	−2·1	−15·1	·988	·108	680·226	+0·9	+2·6
30·89	472·87	−2·6	−22·3	·988	·109	·469	+0·5	+0·9
May 1·92	473·90	−2·8	−18·4	·988	·111	·728	+0·4	+0·7
2·88	474·86	−3·6	−15·4	·988	·112	·969	−0·4	−1·6
Dec. 10·23	696·21	−12·9	−23·9	·998	·442	736·578	0·0	−0·5
13·15	699·13	−12·7	−21·6	·998	·447	737·312	+0·2	−0·6
1995 Jan. 3·08	49720·06	−12·2	−23·2	0·999	4·478	742·570	+1·2	+0·3
June 2·89	870·87	−15·7	−24·6	1·006	·703	780·458	−0·1	−0·4
Dec. 24·13	50075·11	+0·5	−19·5	·016	5·008	831·769	−0·3	−0·1
27·12	078·10	+1·0	−25·0	·016	·012	832·520	−0·3	−0·2
1996 Jan. 1·14	50083·12	+1·7	−19·1	1·016	5·020	833·781	−0·2	0·0
Mar. 28·98	170·96	−4·1	−18·6	·020	·151	855·849	−0·6	−1·2
30·01	171·99	−3·7	−17·0	·020	·152	856·108	−0·1	−0·6
30·94	172·92	−3·7	−22·9	·020	·154	·341	−0·1	−0·1
31·98	173·96	−3·8	−24·2	·020	·155	·603	−0·1	−0·2
Apr. 24·93	197·91	−5·0	−23·7	·021	·191	862·619	+0·2	+0·1
25·84	198·82	−5·4	−18·5	·021	·192	·848	−0·1	−1·1
Nov. 21·22*	408·21	−12·7	−25·2	·031	·505	915·450	−0·5	−0·1
Dec. 15·22	432·21	−13·0	−26·4	·032	·540	921·480	−0·3	−1·0
16·14	433·13	−12·9	−22·6	·032	·542	·711	−0·2	−0·9
1997 Jan. 23·07	50471·06	−13·4	−20·9	1·034	5·598	931·240	0·0	−0·6
Feb. 8·09*	487·08	−13·4	–	·034	·622	935·265	+0·2	–
Mar. 3·05*	510·04	−13·5	−15·2	·035	·656	941·033	+0·4	+0·7
29·94*	536·93	−14·6	−18·4	·037	·697	947·788	−0·3	+1·1
30·96*	537·95	−14·8	−16·1	·037	·698	948·045	−0·5	−0·1
Apr. 1·00*	538·99	−14·7	−21·9	·037	·700	·306	−0·4	+0·4
10·00*	547·99	−13·9	−25·6	·037	·713	950·567	+0·5	−0·5
10·99*	548·98	−14·3	−18·2	·037	·715	·816	+0·1	+0·6
13·91*	551·90	−14·2	−25·2	·037	·719	951·549	+0·2	+0·1
14·99*	552·98	−14·6	−18·0	·037	·721	·821	−0·2	+0·6
15·97*	553·96	−14·3	−16·3	·037	·722	952·067	+0·1	0·0
17·97*	555·96	−13·7	−25·6	·038	·725	·569	+0·8	−0·5
30·90*	568·89	−14·9	−18·4	·038	·744	955·818	−0·3	+0·3
May 3·93*	571·92	−15·1	−25·4	·038	·749	956·579	−0·5	−0·4
10·88*	578·87	−14·6	−22·5	·039	·759	958·325	0·0	+0·4

†Dates correspond to observations of component B.
*Observed with Cambridge *Coravel*.
All other observations made at Haute-Provence.

Orbits

The three orbits (those of A, B, and AB) have been investigated by means of a computer program modified from one developed by Mr. A. P. Cornell[109] to solve orbits for systems that consist of three stars but exhibit only one spectrum, which moves with two different periods; the solutions are set out in Table II. In principle the proper way to deal with ξ UMa would be by a program that deals simultaneously with the complete quadruple system. In practice, though, that would scarcely offer any advantage over treating the visual components as separate systems as is done here, because the 60-year orbit is much more accurately defined by the nearly 200 years of systematic visual and quasi-visual observations than by the desultory radial-velocity coverage. Accordingly, the elements P, T, e and ω have been imposed from the latest visual orbit[24], whereas the gamma-velocity of the complete system (denoted here by Γ), and the velocity amplitude in the outer orbit, have been left as free parameters in each solution, together with the elements of the relevant inner orbit.

The one criticism that could be levelled at that treatment is that it does not align the Γ-velocities which are determined for A and B separately, whereas an integrated solution would require them to be identical. In the event, however, such criticism has little force, since the individually determined Γ-velocities differ only by 0·6 km s^{-1}, and if we felt strongly about it we could average them and impose the result on both solutions — it would make practically no difference to the solutions of the inner orbits, because the changes would be accommodated by the outer ones. The problem is a serious one, as becomes apparent in the discussion below; but it would merely be concealed rather than solved by an integrated solution or the imposition of an averaged Γ, so it has been left open to view. The solutions are set out in Table II. To detect any secular changes such as have been predicted theoretically for the orbital elements would require much more and better material than is presently at our disposal, so the elements have necessarily been assumed constant.

On the basis of the r.m.s. residuals, the Lick velocities have been weighted 0·1 in the case of A and 0·25 in the case of B. The Cambridge and Haute-

TABLE II

Orbital elements for the quadruple system ξ Ursae Majoris

Parameter	*ξ UMa A*	*ξ UMa B*
*Outer orbit:		
Γ (km s^{-1})	−14·87 ± 0·14	−15·50 ± 0·08
K (km s^{-1})	4·85 ± 0·14	4·33 ± 0·09
Inner orbits:		
P (days)	670·24 ± 0·09	3·980507 ± 0·000006
T or T_0 (MJD)	47389·1 ± 2·2	42441·421 ± 0·019
K (km s^{-1})	8·95 ± 0·12	4·83 ± 0·14
e	0·532 ± 0·008	0·0
ω (degrees)	314·1 ± 2·1	undefined
$a_1 \sin i$ (Gm)	69·8 ± 1·0	0·264 ± 0·008
$f(m)$ ($M_\odot$)	0·0303 ± 0·0013	0·000046 ± 0·00004
R.m.s. residual (wt. 1) (km s^{-1})	0·43	0·62

*The other elements have been adopted from the visual orbit[24]:
P = 21857 days, T = MJD 49735, e = 0·412, ω_1 = 307°·3.

Provence data merit equal weight in the orbit of A; for B the Haute-Provence ones have substantially larger residuals and have been accorded half weight. It is worth pointing out, however, that velocities that have been disentangled from blends, as the Haute-Provence ones have, are in principle unbiassed, at least if the observations are made with both visual components simultaneously centred on the spectrometer slit. Measures in which the individual stars are observed separately are compromised by *any* blending from the light of the other component, whose effect will be to drag the measured velocity towards that of that component. Also, systematic guiding errors may be introduced by the inevitable concern on the part of the observer to keep the image of the unwanted star off the slit, rather than to keep the image of the wanted one accurately centred. Exactly the same type of errors may arise, however, when both stars are observed simultaneously but not aligned along the slit, even if the pair appears visually unresolved.

As Table II shows, the inner orbit of B has been supposed to be exactly circular. Berman[84] came to the same conclusion: he started with the eccentricity set to 0·02 among the preliminary elements, and his paper says that a least-squares solution "rendered its value so nearly equal to zero that the orbit was assumed to be circular". (Berman's solution was performed on 'normal points'; a modern solution in which all the observations enter individually gives $e \sim 0{\cdot}03$, though still without any significance.) A solution of the present ensemble of data, with the eccentricity of the inner orbit left free, yields $e = 0{\cdot}056 \pm 0{\cdot}023$ (with $\omega = 221° \pm 23°$), whose significance appears marginal. The sum of the weighted squares of the residuals of the 92 observations is reduced from 35·03 to 33·15 km^2 s^{-2} by allowing e and ω to float and thereby reducing the number of degrees of freedom from 87 to 85. Bassett's[110] second statistical test, comparing the mean square residual per degree of freedom for the 85 and for the extra 2 degrees, gives $F_{2,85} = 2{\cdot}38$, which is *just* significant at the 10% level ($F = 2{\cdot}37$). In the absence, therefore, of strong evidence for non-zero eccentricity, it seems justified to accept the circular solution for this short-period orbit.

The orbits are illustrated in Figs. 1 and 2. In each case, the velocities plotted for the outer orbits are the observed values *minus* the respective computed velocities in the inner ones, and *vice versa*. That implies that it would be useless and indeed misleading to portray velocities which are admitted, or from sources that are suspected by me, to be blends, even in Figs. 1a and 2a: owing to the motions attributable to the inner orbits, blending does not (even in *those* Figures) necessarily drag the observed velocities *towards* the Γ-velocity, as it would in the case of a normal double-lined system. The only thing that can usefully be done with a blended datum, in fact, is to compare it with the computed velocities of each component; that has been done for the published data from every source not utilized in the solutions (Table III) and has assisted in the assessments of identity and blending, but the results are otherwise unedifying and it would serve no purpose to present them *in extenso* here. The one case which gave a reasonable result was that of blended velocities from the Haute-Provence *Coravel*: when the residuals from that source were plotted against the computed radial-velocity differences between the components, the points fell agreeably close to straight lines of slope 0·4 for component A and 0·6 for component B. Exactly the same test has been made on the Lick and Cambridge velocities, each of which is nominally of one component only and therefore is expected to be independent of the relative velocity of the other. Both sources appear to pass

TABLE III

Variances of the series of radial-velocity observations not used in the orbits

The mean square residual from the adopted solution is computed for each source as if it were intended to be, first, of component A, and secondly of component B. It is considered that if the residuals are much smaller when regarded as belonging to one particular component, that is probably the one observed; where they are comparably bad for both, the corresponding observations probably are *of* both.

Approx. Date	*Observatory*	*Ref.*	*n*	*Ostensible source*	*Mean square residual as A*	*as B*	*Probable source*
1912	Bonn	87	4	A	5·4	33·6	A
1917	Mt. Wilson	75	{ 4	A	8·5	9·1	} Mixed up?
			3	B	6·8	23·7	
1918	Ottawa	103	2	A	28·3	138·3	A
1921	Victoria	104	2	A	10·0	87·7	A
1953	Mt. Wilson	106	{ 1	A	0·0	19·2	A
			1	B	16·8	0·0	B
1978	Fick	97	17	Both	14·6	29·6	Both
1984	Wisconsin	99	4	B	44·9	58·6	Both
1994	Haute-Provence	Here	33	Both	1·4	3·8	Both

that check, yielding relationships whose slope is sensibly zero, but in the light of the discussion below it seems that the sensitivity of the test is inadequate to validate the data at the level of stringency demanded by the derivation of the dynamical parallax.

Table III could be read as authenticating Küstner's[87] observations from Bonn, and Harper's[104] from Victoria, as being genuinely of the primary star, as those authors say. The single observation made of each component by Struve & Zebergs[106] at Mount Wilson has a residual of 0·0 km s^{-1} from the cited component — an astonishing result! — and the only reason that those velocities are not incorporated in the orbital solutions is that there are too few of them for positive assurance of their systematic homogeneity with the rest of the data. The three small sets of velocities just mentioned have been portrayed in Figs. 1 and 2, even though they were zero-weighted in the solutions. Among the early Mount Wilson observations published by Abt[75] long after they were made, *one* of each component has a fairly 'wild' residual but would fit reasonably well with the other component. There is no doubt that the (60-inch) telescope and the seeing on Mount Wilson would easily allow the components to be observed separately at the relevant time, which was near apastron; a book-keeping error might be suspected. Cannon's[103] two Ottawa observations are evidently of the correct (brighter) star, but are too few and far from accurate enough to help with the orbit. The Beavers & Eitter[97] Fick measurements are evidently blends, as those authors suspected, although it is odd that they were ascribed mainly to the secondary star. The velocities given by Eker *et al.*[99] from the Wisconsin Pine Bluff Observatory are clearly blends, although published as measures of B. Included in the Table are the 33 closely blended observations made with the Haute-Provence *Coravel* (26 of them by the author, seven by others and already published[107]). Since the reason why they cannot be resolved is that they were obtained at times when the twin velocities were closely the same, it is no surprise to see that they agree quite well with the computed velocities of *both* components.

Discussion

We first compare the orbital elements of the two sub-systems with the only previous determinations, published 70 and 67 years ago by van den Bos[21] and Berman[84] for A and B respectively. The period of A is found here to be just over one day longer than that given by van den Bos, about 1·5 times his standard deviation. The epochs T, brought up to the present by addition of appropriate numbers of the respective periods, differ by 38 days or ·057 of a period, van den Bos's being 'too early' by that amount, which is determined within a little over 2 days. (Mason *et al.*[23] give a T 109 days too early!) At the van den Bos epoch, the discrepancy in T is 13 days, about 1·4σ, and has the opposite sign; it is linked to a 6° (1σ) difference in the longitudes of periastron. The eccentricity found now is practically identical with the old value; the only considerable change in an element is an increase of K by 1 km s^{-1}, about 2·5 of van den Bos's standard deviations.

The only elements of B to be compared are P, T_0 and K. The new determination of the period differs from Berman's value, which was given only to four places of decimals, by a mere seven millionths of a day (six tenths of a second, scarcely more than the new standard error). Berman's epoch (MJD 24998·84, when the idiosyncrasy noted previously is taken into account), brought up to date, differs by only 0·04 of a day, or one hundredth of the period, from the new value. The K values are the same within their standard errors. According to this discussion, therefore, Berman's orbit is for all practical purposes still perfectly valid, despite the doubt cast upon it by Eker, Hall & Anderson[99]; it will be interesting to see in due course whether those authors' promised new period analysis also corroborates it.

The radial-velocity amplitudes in the 60-year orbit are determined here for the first time. In conjunction with the inclination, which is quite accurately known from the visual orbit[24], they can be expected to permit immediate derivation of the dimensions of the system and thereby of its distance and the masses of the separate binary systems A and B. (An analogous calculation was carried out by Berman[84] on the basis of the velocity difference between A and B at a single epoch, but those data permitted only the *total* mass to be calculated.) Unfortunately that exercise produces results that are in unwelcome disagreement with what prejudice, as well as the most recent parallax determination[82], would lead the author to expect: the masses of the two sub-systems come out at 1·04 and 1·20 $M_\odot$ for A and B respectively, the distance being about 7·9 pc ($\pi \sim 0''\cdot126$).

The mass function of the A sub-system, together with the assurance from astrometry that the inclination is so near to 90° that we can incur negligible error by adopting sin i and even sin^3 i as unity, allows the total mass to be partitioned uniquely between the components, whose masses are then Aa 0·72, Ab 0·32 $M_\odot$. At 1·20 $M_\odot$ the B sub-system is the more massive; that is the opposite of the conclusion of most previous authors, who have found (*adopted* might be a more accurate expression for some cases) values of the 'fractional mass' ($=M_B/(M_A+M_B)$) ranging from 0·42[86] to 0·50[111], until in the most recent determination Heintz[24] has obtained 0·506 ± 0·02. As has frequently been remarked by others, the mass function of B is notably small and must imply a low inclination: there is no possibility at present of assigning masses to the individual stars from purely observational considerations.

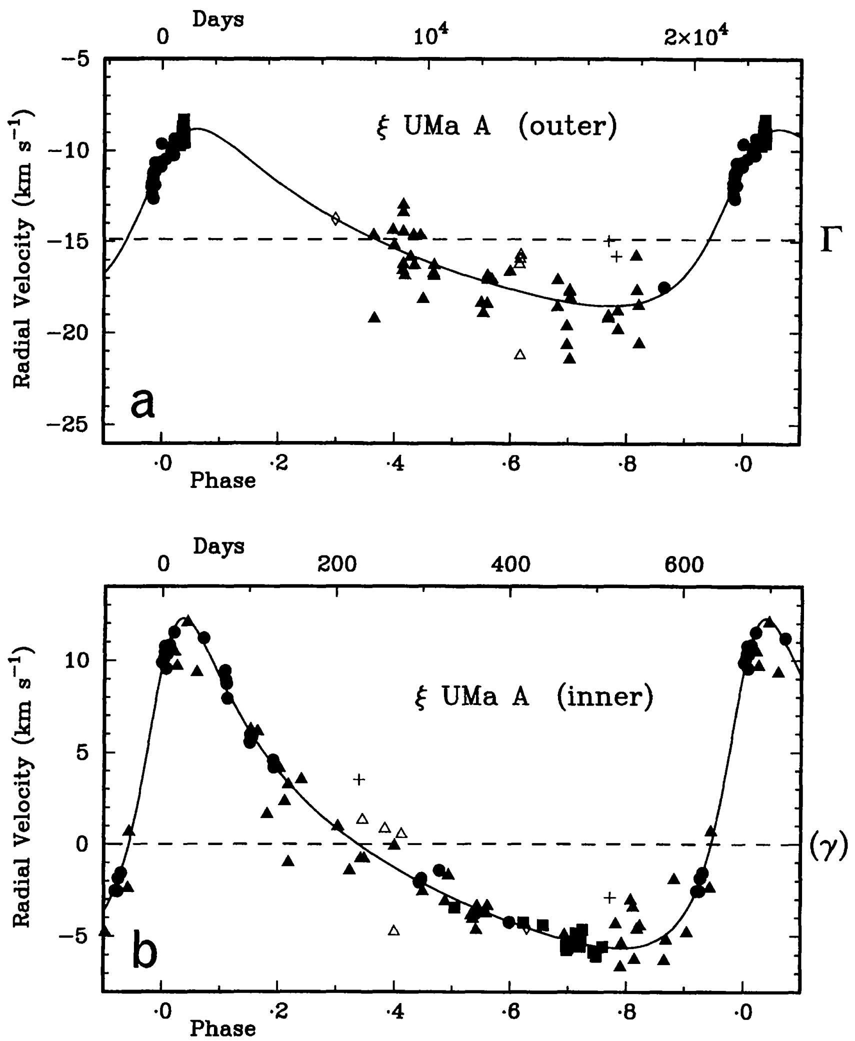

FIG. 1

The computed orbital radial-velocity curves for ξ Ursae Majoris A. For the 'outer' (60-year) orbit, the only parameters fitted are the Γ-velocity and K, the other elements being imposed from the latest visual determination[24]. The actually-observed radial velocities cannot usefully be plotted: in the upper panel they have been corrected by the computed contribution from the inner orbit, in the lower one they have correspondingly been corrected for the motion in the outer orbit. The filled symbols represent the observations upon which the orbits are based, triangles for Lick, circles for Haute-Provence, and squares for Cambridge; the Lick measurements were weighted 0·1 in the solution. Open triangles denote the observations made by Küstner[87] with a 12-inch refractor in 1912, plusses are Harper's[104] DAO measurements, and the open diamond is the single velocity published by Struve & Zebergs[106] on the basis of a 5-Å mm^{-1} Mount Wilson spectrogram; none of those was utilized in the solution.

The deviation of a given observation is the same in both plots, and is the $(O-C)$ residual listed in Table I, but of course the phases of the observation in the two orbits are unrelated to one another. The residuals are shown in their full amount in each plot — they have not been 'shared' between the two orbits.

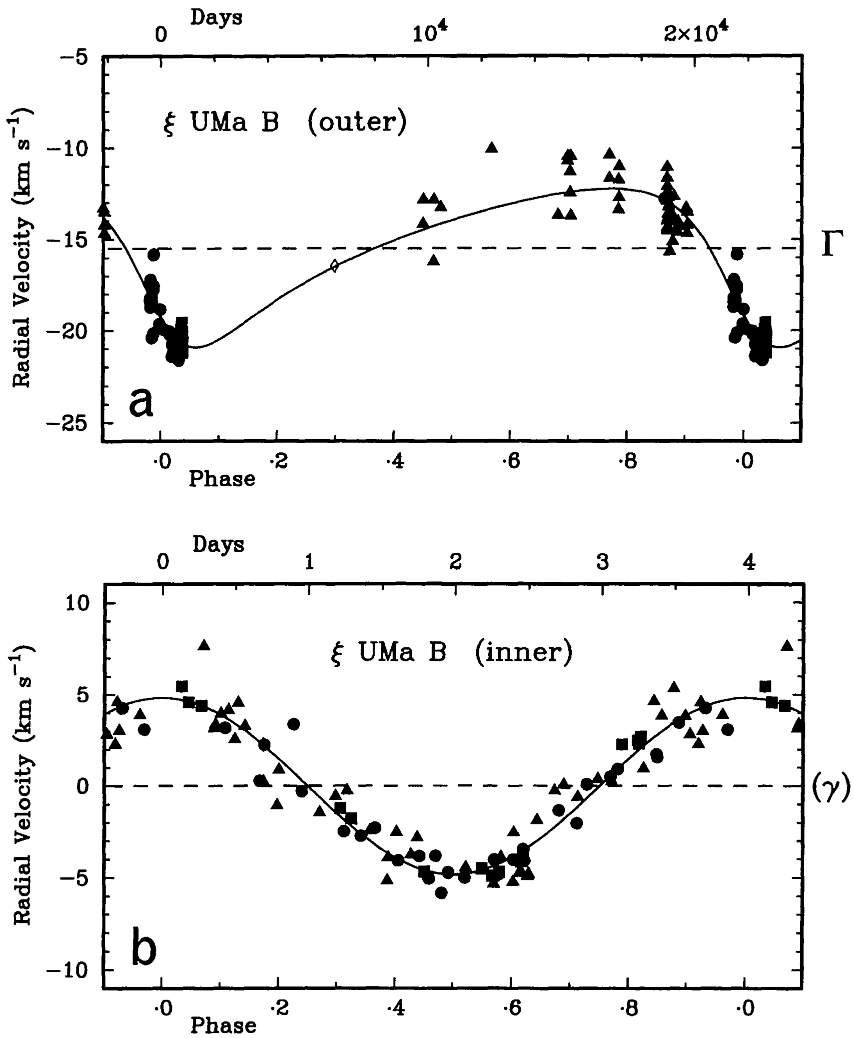

FIG. 2

As Fig. 1, but for ξ Ursae Majoris B. Different weighting was appropriate in this case, Cambridge 1, Haute-Provence 0·5, Lick 0·25.

There is incontrovertible evidence from the integrated colours of the ξ UMa system as well as from the spectral types that Aa, the only detectable component of the sub-system A, is a star of somewhat earlier type than the Sun, so it would be unusual if its mass were really as small as the 0·72 $M_\odot$ found above, or if its absolute magnitude were as faint as the $4^m \cdot 85$ that the 7·9-pc distance implies. In looking for a possible reason for under-estimation of the distance, one is obliged (however reluctantly) to question the radial-velocity amplitudes. Since they are both less than 5 km s^{-1} and their *cubes* enter into the distance calculation, they would only need to be 0·5 km s^{-1} larger than they are given by the orbital solutions presented in Table II above to lead to perfectly 'acceptable' masses and luminosities. Figs. 1a and 2a show how the nodes of the 60-year orbit are defined by observations from different observatories: differences in zero-points of the radial velocities would vitiate the relative amplitudes of A and B. Also, near the nodes of the outer orbit, blending between the spectra of the two visible stars will usually be *towards* the Γ-velocity, so *any* contamination of the spectrum of one component by that of the other will tend to reduce the derived amplitude of the velocity variation. That effect will be worse for B, the fainter star, whose contamination with a given percentage of the light of A will be larger in relative terms — by the square of the luminosity ratio, a factor of about 2·3 — than the same percentage contamination of A by B. Therefore it is the velocity amplitude of B that is particularly susceptible to being under-estimated. That will not only lead to under-estimation of the total mass but also to the assignment of too large a share of that total to B.

It is explained above that an effort has been made to homogenize the zero-points of the three sets of radial velocities upon which the orbits depend; but the fact is that the solutions are not very sensitive to the zero-points because the different series cover different phases of the 60-year orbit. That there is *some* error in the orbit solutions is evident from the fact that the Γ-velocities derived from the two sub-systems differ by about 0·6 km s^{-1}. The sign of the difference is such that the velocity difference between the two sub-systems at the current node is represented as 0·6 km s^{-1} less than it is actually observed to be. If we simply added 0·3 km s^{-1} to the amplitude of each component, by way of a cavalier pretence of rectifying that discrepancy, we should already have achieved more than half of the change we would like to believe necessary.

A quasi-independent estimate of the distance can be made from the elements of the orbit of Aa around the centroid of the A sub-system, for which we know that the semi-major axis[24] is 0″·054, $a \sin i \sim 70$ Gm (see Table II above) and $\sin i \sim 1$. Although the phase distributions of the Cambridge and Haute-Provence radial velocities in that orbit leave something to be desired, they are both fully overlapped by the Lick measurements, and the dangers of zero-point discrepancies thereby become far smaller than in the orbit of AB. The result is a distance of 8·7 pc, corresponding to $\pi \sim$ 0″·115, and a total mass of 2·98 $M_{\odot}$, which if split according to the fractional mass found above from the AB orbit yields 1·39 $M_{\odot}$ for A and 1·59 for B, although for reasons adumbrated above a 50–50 split may be nearer the truth. Even if we take the mass of A to be no more than 1·39 $M_{\odot}$, its division according to the mass function yields 1·00 $M_{\odot}$ for Aa and 0·39 for Ab — much more acceptable for Aa than the values obtained from the AB orbit.

It has to be admitted that not all authors are so prejudiced — so inclined to believe theory rather than observation — as to see a need to try to bend or choose the observational data, as is so transparently done above. For example, de Strobel *et al.*[67] unquestioningly adopted for ξ UMa masses that they attributed to Heintz, citing a paper that does not give them. Heintz *did*, however, put forward the quoted masses, which are embarrassingly small, elsewhere[25]. Somehow de Strobel *et al.* managed to convince themselves (if no-one else) that it is quite all right to have an F-type star (ξ UMa Aa) with the mass of a late-G-type one (0·87 $M_{\odot}$). Heintz, however, rather pulled the rug out from under them when, in the next revision[24] of the orbits, and notwithstanding that the previous set had already twice been graded[112,113] as 'definitive', he raised the mass of Aa to 1·05 $M_{\odot}$ — exactly right for its spectral type! It was merely a question of adopting more agreeable values of the fractional mass and parallax, even though in changing the latter he did not go nearly as far as his own determination[82] of two years previously would have warranted.

The Ab mass is that of an early-M-type star, five or six magnitudes fainter than Aa, so it is not at all surprising that Ab has not been seen. Although it is so faint, it must nevertheless have a radius quite half as great as that of Aa and thus be capable of covering a quarter of the latter's area and thereby creating eclipses up to about $0^m\cdot3$ deep if the orbital inclination is sufficiently close to 90°. Unfortunately the relevant conjunctions are far from periastron, and an orbital inclination within about $^1/_4$ ° of the exact value 90° would be necessary for them to occur; but since the astrometrically determined inclination has vacillated above and below 90° and the latest value is only 1° away from it, the likelihood of eclipses is certainly not negligible. Conjunctions occur 146 days after periastron passages; the next opportunities to look for them are due on 2000 January 13 and 2001 November 13, dates that are expected to be correct within two or three days; the following two will be outside the practical observing season. Eclipses of the secondary would occur near to periastron and are geometrically more than twice as probable, but their depths could be expected to be in the range of mere thousandths of a magnitude, very difficult if not impossible to detect.

Eclipses could last up to about a day and a half if central, with an 'annular' phase of up to about 6 hours. It may seem a bit far-fetched to think that such eclipses could occur without having been noticed already; but an eclipse that occurs only on one night, at intervals of two years, creating a change of less than $0^m\cdot2$ in the total light of the system, and still leaving A brighter than B so as to dulcify even an experienced observer who looks at the system with the optical power to resolve it, could easily have escaped notice. After all, eclipses were never noticed in γ Persei until the writer[114] predicted one, although they have an amplitude of $0^m\cdot3$ and are total for a week. The star is too bright to be usefully measureable on patrol plates.

Since the total mass of B is found formally to be greater than that of A (though considered informally to be nearer to equal), and the contribution to it of Ba is expected to be a little smaller than the mass of Aa since Ba is later in type, the mass of Bb may well be $\gtrsim 0\cdot5\ M_\odot$. That suggests that it is a late-K dwarf, probably no more than about three magnitudes fainter than its primary, and therefore potentially detectable. Indeed, it may already have been unwittingly detected through its *K*-line emission, as discussed below.

K-line and Hα emission in ξ Ursae Majoris B — can we see Bb?

Late-type stars with rotation periods as short as a few days commonly have active chromospheres, which are manifested principally by emission in the Ca II *H* and *K* lines and in Hα in the optical band, and in unusually intense emission at very short wavelengths (hard ultraviolet and X-rays); there is also unusual He I absorption at λ10830 Å. All those characteristics are exhibited by ξ UMa; in cases where it is possible to distinguish between the visual components, the activity is always to be identified with ξ UMa B, although Hodgkin & Pye[115] ascribe it to A, citing Pounds *et al.*[116] as authority. It is ulterior to the scope of this paper and to the powers of the author to discuss the spacecraft data, which in any case seem to amount to little more than basic detections and some quantification; we refer here principally to the optical activity, although it is worth noting that, in a programme in which the λ10830-Å line was looked for in a sample of 71 main-sequence stars, the absorption in ξ UMa was found[117] to be the strongest save for that of α Trianguli.

There is no actual observational evidence that ξ UMa Ba is rapidly rotating. The rotational velocities of both Aa and Ba were too small to be detected by the observers[118–121] who took an early interest in the matter. Cohen[122] gave $v \sin i = 4$ km s^{-1} for A on the basis of Fabry–Pérot scans of the lithium λ6707-Å region, but it was not until 1982 that measured values were published for both stars. In that year Soderblom[123] gave $v \sin i = 1{\cdot}4 \pm 0{\cdot}7$ km s^{-1} for Aa and $2{\cdot}8 \pm 0{\cdot}7$ km s^{-1} for Ba. Taken at face value and compared with the putative true equatorial rotational velocity (~ 13 km s^{-1}) corresponding to rotation of a star very similar to the Sun in four days (it being assumed here that the rotation is synchronous with the orbital period), the $v \sin i$ of Ba yields $\sin i = 0{\cdot}22 \pm 0{\cdot}06$, or $i = 11 \pm 4$ degrees. Formally, the central value of $\sin i$, taken in conjunction with the reasonably well determined mass function of B, implies a mass of about $0{\cdot}2\ M_{\odot}$ for Bb; but by the time a quantity with the fractional error of the above-quoted $\sin i$ is cubed for substitution in the expression for the mass function it does not retain much significance at all. If, as proposed in the discussion above, the mass of Bb is something like $0{\cdot}5\ M_{\odot}$, with Ba nearly $1\ M_{\odot}$, then to obtain the observed mass function requires $\sin i$ to be only about $0{\cdot}1$, $i \sim 6°$. That is of course an improbably small value, having an *a priori* geometrical likelihood of 1/200; on the other hand, even improbable things do happen sometimes, and anyway there are three orbits involved in ξ UMa so the probability of *one* of them having $\sin i \sim 0{\cdot}1$ is three times higher, *viz.* 1/67, perhaps not *so* improbable after all.

The idea that Ba rotates synchronously with the orbital revolution is widespread even in the absence of any supporting evidence. Thus Walter[124] says it is synchronized, as if it were a matter of established fact. Montes *et al.*[125] assert that P_{rot} is 3·9805 days while they give 3·9810 days for P_{orb}, ascribing those data to the 'CABS' catalogue by Strassmeier *et al.*[126], which in actuality gives the right value for P_{orb} and does not give P_{rot} at all. (We note that it does misprint the value of $(U-B)$, though, and so did its predecessor[127], which also has the enigmatic remark, "Double peak in light curve".) As the leader of a different syndicate, however, Strassmeier[128] does assert that B rotates synchronously. Barrado *et al.*[129] list ξ UMa B as having a *photometric* period approximately equal to the orbital period. Saar *et al.*[130] even attribute to Berman[84] the news that the rotational period is 3·98 days; they also assign a $(B-V)$ colour index of $0^{m}{\cdot}66$, of unstated provenance. Really, though, it would be better, if data are unavailable, *not* to make them up!

It is important to notice that, if indeed Bb has a mass near $0{\cdot}5\ M_{\odot}$ and is therefore a late-K dwarf, it will not be very much smaller than its primary and (if synchronized, as still seems likely, even though no reference can be given here to shift the onus of saying so onto others) will itself have a rotational velocity of about 9 km s^{-1}, quite large enough to provoke chromospheric activity.

Joy & Wilson[131] called attention to the *H* and *K* emission in ξ UMa (under the alias Boss[111] 2984) in 1949; they implicitly claimed to have been the first to see it. They appended a note, "*H* and *K* wide" — an interesting assertion, in the light of subsequent knowledge[132,133] that main-sequence stars have narrow Ca emission lines and that ξ UMa has[123] a very low projected rotational velocity. Boss 2984 includes both visual components, however, and in addition to the substantial emission in B there is[134] a weak line in A; but it is too weak to help make the line in B look wide, even in a spectrogram taken at a phase when the velocities differed substantially. That may be verified by reference to the spectrograms reproduced by Wilson[64] of both stars. In that paper Wilson himself said that the lines of ξ UMa B "show rotational widening"; it is not clear whether he was referring just to the *H* and *K* emission lines, or to all lines.

Since reticons and CCDs have come to the fore as detectors of choice, interest has followed instrumentation into the red part of the spectrum, and observers have concentrated particularly on Hα and Li I λ6707 Å. The presence of the lithium line in some strength in A and its absence in B was first demonstrated and remarked upon by Herbig[135] in 1965 on the basis of photographic spectrograms, and such observations have been repeated several times since. An explanation was eventually tendered by Bopp[66] in terms of the difference in spectral type that had then been rediscovered. Herbig[136], using a reticon in 1985, also seems to have been the first to recognize that the Hα absorption line in ξ UMa B is substantially weakened by chromospheric emission in its core. By subtracting the spectrum of a reference star (β CVn, G0 V), Herbig was able to assess the equivalent width of the Hα emission in 45 F8–G3 dwarfs, including both components of ξ UMa; the B component had by far the strongest emission of all, 247 mÅ equivalent width, *versus* 13 mÅ in A. The same sort of thing is shown by Bopp's[66] tracings; the central residual intensity is measured at 22 per cent for A and 46 for B. The difference in equivalent widths of the lines in the two stars is given as 0·76 Å, three times as much even as Herbig found for the emission — but Herbig measured just the core and not the whole extent of the absorption line, which is naturally weaker in the star of later type quite apart from the chromospheric contribution.

Strassmeier *et al.*[128] carried out an Hα survey very much analogous to Herbig's[136], with the subtraction of suitable reference spectra from those of the programme stars. They found the central residual intensity in ξ UMa B to be 47·3 per cent — very similar to Bopp's result — but considered the equivalent width of the emission to be only 97 mÅ. Frasca & Catalano[137] repeated the exercise again, and obtained emission values of 0·15 Å according to their tabulation (0·14 Å in the text). Montes *et al.*[138], doing the same thing yet again, found the equivalent width of the emission to be "very small, in agreement with the values reported by Strassmeier *et al.* (1990) and Frasca & Catalano (1994)". They show a tracing, wherein the central residual intensity appears to be about 34 per cent — much deeper than was found by Bopp[66] and by Strassmeier *et al.*[128]. Later, the same Montes syndicate[125] stated that the central intensity was 33·3 per cent and the equivalent width of the emission was 107 mÅ. Already referred to above in the section on published radial velocities, as well as in Table III, are the observations of Eker, Hall & Anderson[99], which have been shown to be blends although attributed to B. Those authors say that their Hα profiles "appear normal, with central intensities R_c = 0·30".

The reader is at a loss to know what to make of the great variations in the quoted equivalent widths and central intensities — do they represent gross changes in emission intensity or do the differences in equivalent width mainly arise from differences in procedure and/or in the standard spectra being subtracted? But procedural matters would not alter the central intensities of the lines 'as observed'. One has to ask, therefore, are the differences real, or do they reveal that Eker *et al.* are not alone in failing to recognize that ξ UMa A is closely adjacent to the star of interest?

One further set of lines that exhibit emission in their cores, at least in the case of stars with active chromospheres, is the infrared Ca I triplet, whose strongest member is λ8542 Å. A tracing of that line in the spectrum of ξ UMa is shown by de Strobel *et al.*[67]: the absorption core is truncated, and the nearly flat bottom that is left is interrupted by a small emission reversal that is significantly displaced shortward from the axis of symmetry of the absorption line. The caption refers to the feature, saying that it is "possibly related" — it does not explain *how* — "to the enhanced mass-loss" that has been proposed for such an active object. The caption continues, "the low-mass companion star is too faint to contribute at this level".

If the suggestion, made in the *Discussion* section above, that ξ UMa Bb is about three magnitudes fainter than its primary Ba, is correct it seems to me to be far from certain that it cannot contribute significantly to emission lines. There is little experience upon which to base proposals concerning λ8542 Å, but there certainly *is* concerning the *K* line: the strength of the emission typically increases enormously as one goes from solar types to late-K dwarfs. Comparatively faint secondary stars may thereby show up in the emission lines. For example, Wilson[64] discovered the double-lined nature of the 5·6-day Hyades system van Bueren[139] 22 by the doubling of the *H* and *K* emissions many years before other observers tumbled to it. The difference in magnitude of the components of vB 22 has been put[140] at $2^{m}\cdot 3$, the suggested types being G6 and K6 V, so the pair may be quite similar to the ξ UMa B sub-system, though with both the stars a little later in type. The *wide H* and *K* lines that have been reported[131,64] might be seen as giving grounds for cautious optimism for this idea. With a mass ratio of nearly two to one and $K_1 \sim 5$ km s^{-1}, the components would possess a velocity difference near to 15 km s^{-1} at the nodes of the orbit. Suitably timed spectroscopy at high resolution could be expected quickly to adjudicate on this possibility.

Acknowledgements

I am very grateful once again to Dr. M. Mayor and the Observatoire de Genève for the use of the Haute-Provence *Coravel*, and to Dr. S. Udry for performing the reductions. It is a pleasure to acknowledge helpful comments from Drs. C. D. Scarfe, C. Lloyd and B. D. Mason. I thank Miss J. Nicholas, the Observatories' Librarian, for obtaining for me a copy of the paper[86] by Abetti, and her counterpart Ms. M. Ferrarini of the Osservatorio Astronomico di Bologna for providing it. Last, but not least, I am indebted to Dr. H. A. McAlister, whose suggestion prompted my interest in ξ UMa.

References

(1) W. Herschel, *Phil. Trans.*, **72**, 112, 1782.
(2) J. F. W. Herschel, *Mem. RAS*, **35**, 21, 1866.
(3) W. H. Smyth, *A Cycle of Celestial Objects* (J. W. Parker, London), 1844, p. 246.
(4) J. Bayer, *Uranometria* (Gorlini, Ulm), 1655.
(5) W. Herschel, *Phil. Trans. for 1803*, p. 339.
(6) W. Herschel, *Phil. Trans. for 1804*, p. 353.
(7) J. F. W. Herschel, *Mem. RAS*, **5**, 171, 1831.
(8) F. Savary, *Connaissance des Tems pour l'an 1830* (Bachelier, Paris), 1827, p. 56 [given in error as p. 65 in the index].
(9) *ibid.* p. 163.
(10) S. W. Burnham, *General Catalogue of Double Stars Within 121° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1906, part 2, p. 581.
(11) F. G. W. Struve, *Mensurae Micrometricae* (Typographia Academia, Petropolis), 1837, p. 20.
(12) R. G. Aitken, *New General Catalogue of Double Stars Within 120° of the North Pole* (Carnegie Institution of Washington, Washington, D.C.), 1932, **2**, 681.
(13) N. E. Nørlund, *Astr. Nachr.*, **170**, 117, 1905.
(14) T. J. J. See, *Astr. Nachr.*, **139**, 163, 1896.
(15) T. J. J. See, *Researches on the Evolution of Stellar Systems* (Nichols, Lynn, Mass.), 1896, **1**, 105*ff*.
(16) T. J. J. See, *AJ*, **16**, 17, 1896.
(17) W. W. Campbell & W. H. Wright, *ApJ*, **12**, 254, 1900.
(18) W. W. Campbell, *PASP*, **13**, 31, 1901; **15**, 110, 1903.
(19) W. H. Wright, *Lick Obs. Bull.*, **5**, 26, 1908.
(20) E. Hertzsprung, *Astr. Nachr.*, **208**, 111, 1918.
(21) W. H. van den Bos, *D. Kgl., Danske Vidensk. Selsk. Skrifter, Naturvidensk. og Math. Afd.*, Ser.

8, **12**, 293, 1928.
(22) A. H. Batten, J. M. Fletcher & D. G. MacCarthy, *Eighth Catalogue of the Orbital Elements of Spectroscopic Binary Systems*, *PDAO*, **17**, 62, 1989.
(23) B. D. Mason *et al.*, *AJ*, **109**, 332, 1995.
(24) W. D. Heintz, *AJ*, **111**, 408, 1996.
(25) W. D. Heintz, *Astr. Nachr.*, **289**, 269, 1967.
(26) E. W. Brown, *MNRAS*, **97**, 388, 1937.
(27) J. F. Ling, J. A. Docobo & A. J. Abad, *AJ*, **110**, 875, 1995.
(28) D. Hoffleit, *The Bright Star Catalogue* (Yale Univ. Obs., New Haven), 1982, p. 179.
(29) E. C. Pickering, *HA*, **50**, 89, 1908.
(30) E. C. Pickering, A. Searle & O. C. Wendell, *HA*, **14**, 180, 1884.
(31) E. C. Pickering, *HA*, **44**, 51, 1899.
(32) E. C. Pickering, A. Searle & W. Upton, *HA*, **11**, 125 & 149, 1879.
(33) E. Crossley, J. Gledhill & J. M. Wilson, *A Handbook of Double Stars* (Macmillan, London), 1879, p. 270.
(34) H. L. Johnson & C. F. Knuckles, *ApJ*, **126**, 113, 1957.
(35) V. B. Nikonov *et al.*, *Izv. Krim. Astrophys. Obs.*, **17**, 42, 1957.
(36) B. Ljunggren & T. Oja, *Uppsala Ann.*, **4**, no. 10, 1961; *Ark. Astr.*, **3**, 439, 1964.
(37) O. J. Eggen, *AJ*, **69**, 570, 1964.
(38) A. N. Argue, *MNRAS*, **133**, 475, 1966.
(39) H. L. Johnson *et al.*, *Commun. Lunar & Plan. Lab.*, **4**, 99, 1966.
(40) L. Häggkvist & T. Oja, *Ark. Astr.*, **4**, 137, 1966.
(41) V. M. Blanco *et al.*, *Publ. USNO*, Ser. 2, **21**, 371, 1968.
(42) M. P. FitzGerald, *A&AS*, **9**, 297, 1973.
(43) B. V. Kukarkin *et al.*, *New Catalogue of Suspected Variable Stars* (Nauka, Moscow), 1982.
(44) G. Jackisch, *Veröff. Sternw. Sonnenberg*, **5**, no. 5, 1963.
(45) K. G. Strassmeier *et al.*, *ApJS*, **69**, 141, 1989.
(46) E. V. Kazarovets & N. N. Samus, *IBVS*, no. 3530, 1991.
(47) D. Hoffleit, *JAAVSO*, **24**, 105, 1996.
(48) E. Hertzsprung, *Publ. astr. Obs. Potsdam*, **24**, no. 75, p. 32, 1920.
(49) P. Baize, *Ann. d'Ap.*, **14**, 85, 1951.
(50) G. P. Kuiper, *PASP*, **47**, 15, 1935.
(51) P. Muller, *Ann. Strasbourg*, **5**, no. 1, p. 1, 1948; no. 4, p. 1, 1952.
(52) A. Wallenquist, *Uppsala Ann.*, **4**, no. 2, 1954.
(53) G. van Herk, *J. des Obs.*, **49**, 355, 1966.
(54) K. Rakos *et al.*, *A&AS*, **47**, 221, 1982.
(55) G. C. de Strobel *et al.*, *A&AS*, **95**, 273, 1992.
(56) *The Hipparcos and Tycho Catalogues* (ESA SP–1200) (ESA, Noordwijk), 1997.
(57) W. H. Wright, *Lick Obs. Bull.*, **3**, 5, 1904.
(58) W. H. Wright, *PASP*, **33**, 47, 1921.
(59) J. M. Vinter Hansen, *PASP*, **54**, 137, 1942.
(60) C. F. Stearns, *AJ*, **35**, 157, 1924.
(61) W. W. Morgan, P. C. Keenan & E. Kellman, *An Atlas of Stellar Spectra with an Outline of Spectral Classification* (Univ. of Chicago), 1943, p.22.
(62) N. G. Roman, *ApJ*, **112**, 554, 1950.
(63) A. P. Cowley, W. A. Hiltner & A. N. Witt, *AJ*, **72**, 1334, 1967.
(64) O. C. Wilson, *ApJ*, **138**, 832, 1963.
(65) O. C. Wilson, *PASP*, **76**, 238, 1964.
(66) B. W. Bopp, *PASP*, **99**, 38, 1987.
(67) G. C. de Strobel *et al.*, *A&A*, **291**, 505, 1994.
(68) D. R. Soderblom, *ApJS*, **53**, 1, 1983.
(69) P. C. Keenan & S. B. Yorka, *Bull. Inf. CDS*, no. 29, 25, 1985.
(70) P. C. Keenan & S. B. Yorka, *Bull. Inf. CDS*, no. 35, 37, 1988.
(71) P. C. Keenan & R. C. McNeil, *ApJS*, **71**, 245, 1989.
(72) W. S. Adams & A. H. Joy, *PASP*, **29**, 182, 1917.
(73) W. S. Adams & A. H. Joy, *ApJ*, **46**, 313, 1917.
(74) W. S. Adams *et al.*, *ApJ*, **53**, 13, 1921.
(75) H. A. Abt, *ApJS*, **19**, 387, 1970.
(76) W. S. Adams *et al.*, *ApJ*, **81**, 187, 1935.
(77) G. P. Kuiper, *ApJ*, **91**, 269, 1940.
(78) W. Gliese, *Astr. Rechen-Inst. Heidelberg Mitt.*, Ser. A, no. 8, 1957.
(79) W. Gliese, *Astr. Rechen-Inst. Heidelberg Veröff.*, no. 22, 1969, p. 50.
(80) R. K. Young & W. E. Harper, *PDAO*, **3**, 1, 1924.
(81) W. B. Rimmer, *Mem. RAS*, **62**, 113, 1923.

(82) W. D. Heintz, *AJ*, **108**, 2338, 1994.
(83) W. W. Morgan, *ApJ*, **87**, 460, 1938.
(84) L. Berman, *Lick Obs. Bull.*, **15**, 109, 1931. [*Note:* the asterisks to the first footnote to Berman's Table I should be after the first four entries in the 'Observed velocity' column.]
(85) W. W. Campbell, *ApJ*, **8**, 123, 1898.
(86) G. Abetti, *Mem. Soc. Spettr. Ital.*, Ser. 2, **8**, 105, 1919.
(87) F. Küstner, *Astr. Nachr.*, **198**, 409, 1914.
(88) W. W. Campbell, *PASP*, **30**, 351, 1918.
(89) J. H. Moore, *Lick Obs. Bull.*, **11**, 141, 1924.
(90) L. Berman, *PASP*, **42**, 41, 1930.
(91) J. H. Moore, *Lick Obs. Bull.*, **18**, 1, 1936.
(92) J. H. Moore & F. J. Neubauer, *Lick Obs. Bull.*, **20**, 1, 1948.
(93) A. H. Batten, *PDAO*, **13**, 119, 1967.
(94) A. H. Batten, J. M. Fletcher & P. J. Mann, *PDAO*, **15**, 121, 1978.
(95) W. W. Campbell & J. H. Moore, *Publ. Lick Obs.*, **16**, 168, 1928.
(96) T. S. Jacobsen, *Lick Obs. Bull.*, **12**, 138, 1926.
(97) W. I. Beavers & J. J. Eitter, *ApJS*, **62**, 147, 1986.
(98) A. Duquennoy & M. Mayor, *A&A*, **248**, 485, 1991.
(99) Z. Eker, D. S. Hall & C. M. Anderson, *ApJS*, **96**, 581, 1994.
(100) N. Craig *et al.*, *ApJS*, **113**, 131, 1997.
(101) M. Rudkjøbing, *Ann. d'Ap*, **14**, 272, 1951.
(102) J. Audouze & G. Israël, *The Cambridge Atlas of Astronomy* (Camb. Univ. Press), 1985, p. 282.
(103) J. B. Cannon, *Publ. Dom. Obs., Ottawa*, **4**, 253, 1918.
(104) W. E. Harper, *PDAO*, **6**, 149, 1934.
(105) R. E. Wilson & A. H. Joy, *ApJ*, **111**, 221, 1950.
(106) O. Struve & V. Zebergs, *AJ*, **64**, 219, 1959.
(107) M. Mayor, A. Duquennoy & J.-L. Halbwachs, *A&AS*, **88**, 281, 1991.
(108) R. F. Griffin, *MNRAS*, **145**, 163, 1969.
(109) R. F. Griffin & A. P. Cornell, *The Observatory*, **117**, 82, 1997.
(110) E. E. Bassett, *The Observatory*, **98**, 122, 1978.
(111) L. Boss, *Preliminary General Catalogue of 6188 Stars for the Epoch 1900* (Carnegie Institution of Washington, Washington, D.C.), 1910, p. 269.
(112) W. S. Finsen & C. E. Worley, *Republic Obs. Johannesburg Circ.*, 7, 201, 1970.
(113) C. E. Worley & W. D. Heintz, *Publ. USNO*, Ser. 2, **24**, part 7, 1983.
(114) R. F. Griffin, *S&T*, **81**, 598, 1991.
(115) S. T. Hodgkin & J. P. Pye, *MNRAS*, **267**, 840, 1994.
(116) K. A. Pounds *et al.*, *MNRAS*, **260**, 77, 1993.
(117) D. M. Zarro & H. Zirin, *ApJ*, **304**, 365, 1985.
(118) S.-S. Huang, *ApJ*, **118**, 290, 1953.
(119) G. H. Herbig & J. Spalding, *ApJ*, **121**, 118, 1955.
(120) O. Struve & K. Franklin, *ApJ*, **121**, 337, 1955.
(121) A. Slettebak, *ApJ*, **121**, 666, 1955.
(122) J. G. Cohen, *ApJ*, **171**, 71, 1972.
(123) D. R. Soderblom, *ApJ*, **263**, 239, 1982.
(124) F. M. Walter, *ApJ*, **253**, 745, 1982.
(125) D. Montes *et al.*, *A&A*, **294**, 165, 1995.
(126) K. G. Strassmeier *et al.*, *A&AS*, **100**, 173, 1993.
(127) K. G. Strassmeier *et al.*, *A&AS*, **72**, 291, 1988.
(128) K. G. Strassmeier *et al.*, *ApJS*, **72**, 191, 1990.
(129) D. Barrado *et al.*, *A&A*, **326**, 780, 1997.
(130) S. H. Saar *et al.*, *A&A*, **326**, 741, 1997.
(131) A. H. Joy & R. E. Wilson, *ApJ*, **109**, 231, 1949.
(132) O. C. Wilson & M. K. V. Bappu, *ApJ*, **125**, 661, 1957.
(133) O. C. Wilson, *ApJ*, **205**, 823, 1976.
(134) J. L. Greenstein, *PASP*, **64**, 71, 1952.
(135) G. H. Herbig, *ApJ*, **141**, 588, 1965.
(136) G. H. Herbig, *ApJ*, **289**, 269, 1985.
(137) A. Frasca & S. Catalano, *A&A*, **284**, 883, 1994.
(138) D. Montes *et al.*, *A&AS*, **109**, 135, 1995.
(139) H. G. van Bueren, *BAN*, **11**, 385, 1952.
(140) R. F. Griffin *et al.*, *AJ*, **90**, 609, 1985.