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ERRORS OF OBSERVATION IN DOUBLE-STAR WORK

(Abridged Version of Introductory Exposé)

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Astronomical observation frequently is focused on minute quantities, and on digging information even from below the 'noise level'. In all cases of long-term variations, such as visual binary motions, measurements over a long time interval have to be combined. All of this requires a knowledge of the observational errors in the past and present. We usually are not at liberty to discard old observations since we cannot repeat them at any later time desired. Visual observations leave no re-measurable records, so we have to take the word of the observer, and make the best of it.

Systematic corrections to micrometer observations seem to be outmoded today, or even in disrepute, after the doubtful or inconclusive results by investigators such as O. Struve (the elder) and Ekenberg. Perhaps the problem is now less urgent since there are more extended series of measurements which are virtually free from systematic errors, and in general the coverage of important objects (or part of them, at least) by several, simultaneous observers has improved. However, if a series is sufficiently long and homogeneous the systematic errors can be very precisely determined and removed so the presence of these errors need not have anything to do with the reliability (the random error) of the observations (within a certain limit, of course; I usually reject series whose systematic errors amount to 0.″2 or more since the correction then is too uncertain). By ignoring this possibility, we will lose a great deal of data which can be converted into good measurements, the most important cases in this century being Komendantov's, Rabe's, and the early Greenwich results. Some old orbits derived with a careful study of personal equations, for instance by Silbernagel, have turned out to be of excellent, long-lasting quality.

I should like to invite a discussion if corrected results (from well-established corrections by the observer) should be included in the *Central Card Catalogue*. I discussed the problem with Worley, and his view should be appreciated that we have to keep the original records rather than revised and re-revised figures. On the other hand, the absence of the authentic, corrected values virtually ruins the information as far as its use is concerned. The commonly held opinion, to leave the decision up to the individual orbit computer, entirely misses the problem. It is practically impossible for him to do this; he would have to study the measurements of all objects even if he was interested in merely one.

Can the system of angular separations as produced by the 'average observer' be assumed to be error-free? I think this holds true, even for the range which cannot be checked directly by either photography or interferometry. Errors would have shown in the areal velocity of pairs with large distance variations, and they would probably

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also have been revealed by comparison with double-image measurements since they should largely cancel out in the latter technique. The Muller type micrometer which has been increasingly employed of late, seems to be a suitable device to reduce the risk of systematic errors, and it should be recommended to beginning observers. I do not know much about the performance of the micrometer introduced by Camichel; it apparently has not been used recently.

Most observers show no significant systematic errors in position angles. The reversing prism appears to be efficient in reducing such errors but there has been little need to employ it.

The random errors $\varrho\,\Delta\theta$ and $\Delta\varrho$ depend on the separation ϱ. Ekenberg found that they are about proportional to $\varrho^{1/5}$; I think that $\varrho^{1/3}$ perhaps fits better. When combining results from different observers, I even assume the errors to vary with $\varrho^{1/2}$ because the contribution by less experienced observers is relatively greater when the separation is larger. The weights I use for the normal places are approximately given by the expression $\varrho\sqrt{p_1 p_2}$, where p_1 is the weight (number) of observations, and p_2 the sum of weights assigned to the individual observers.

The errors of photographic observations are considerably smaller yet some systematic errors do occur, again chiefly in the separations. They can be quite disturbing in the study of astrometric pairs and sub-systems. The evidence for an invisible companion is always suspicious if shown by the separations only. All the companions reported by Holmberg, and a number of other cases as well, have failed to be confirmed. If the visible pair has a fast orbital motion or a third visible component, additional criteria are available to check the reality of the presumed sub-system.

With regard to magnitudes of binary components, observers' estimates have to supply the differences Δm in close pairs, and the total magnitude for faint pairs as well. Estimates by several experienced observers will be found quite reliable though, again, some systematic adjustment is necessary. Formulae to correct the estimated Δm have been proposed by Öpik and by Baize. In order to avoid arbitrary reductions, and for lack of any better system, the IDS catalogue usually gives the Δm estimated at the discovery of the pair even if it is known to be in error. One has to be aware that the catalogue data are not expected to be uncritically accepted. A drastic case occurred in connection with the most complex, and most-discussed of all binaries, β Lyrae. For years the best evidence on the distance seemed to be offered by the luminosity of a distant, physical companion – but the magnitude taken from ADS was an uncorrected visual estimate with a systematic error of more than a magnitude.

In the calculation of dynamical parallaxes from visual orbits, by the way, the total magnitude enters much more critically than Δm.

High-quality orbit and mass determinations require that no observational information be wasted. We have to explore the chasm of observing errors in order to step as closely to its edge as possible. One cannot afford to neglect many observations, or to forget the evidence from radial velocities, for instance. I do not go along with the often-heard statement that the agreement or disagreement of orbital elements of one system by different computers is a criterion on the determinacy of the orbit. Frequently the discordance merely indicates that some of the orbits (or all of them) have been poorly calculated. Multiple systems are a particularly tricky field with regard to the weighing of evidence.

In the computation of an orbit, after all the observations are assembled, scrutinized, and a set of normal places is arrived at, then I think the most critical part of the job is done.

Discussion

Strand: It has been my experience that the (uncorrected) $O\Sigma$ observations showed better agreement with other observers than do his corrected results. Perhaps his artificial stars were too artificial to give reliable corrections, as had been noticed before. Concerning the systematic errors in separation of micrometer observations, there are a substantial number of cases where the photographic observations have shown that averages of all the visual observations are affected by such errors. In η Cas the visually observed separations are too small by $0''.04 \pm 0''.01$ (m.e.) for the entire observed interval of 130 yr, when compared with photographic observations. In 70 Oph, the separations are measured too large when they exceed $1''.5$, and too small below that limit, again as compared to the scale derived from photographic observations.

Rösch: Le micromètre utilisé par Camichel est dû à Lyot. Il ne diffère de celui de Muller que par l'effect de biréfringence utilisé (lame de spath taillée parallèlement à l'axe et tournant autour de cet axe) et présente l'avantage d'être plus lumineux. Il a été peu employé (Pic-du-Midi, Athènes) mais il n'y a pas de raison pour que ses résultats diffèrent de ceux du micromètre de Muller.

Morel: Dans votre système de pondération comment tenez-vous compte de l'ouverture de l'instrument? Ce qui est important pour certains observateurs tels que van Biesbroeck, van den Bos, Couteau, Worley qui ont utilisé des ouvertures différents parfois sur les mêmes couples a des époques voisines.

Heintz: I do not consider the aperture directly. Of course it is known which telescope was used by each of the observers, so one may say that the problem is merely shifted into the name of the observer, i.e. into the weight p_2. Otherwise the telescope size does not matter much. Compare Burnham's results with various instruments, or Worley's with the two Lick refractors. Of course, the larger instrument reaches more difficult pairs. But as far as the material overlaps the accuracy is practically the same.

Jonckheere: As you say, the systematic differences in the measured separations do not seem to depend on the size of the instrument. The personal equation is often very important, especially for faint pairs. Fortunately it is remarkably constant with each observer, at least for well-separated pairs. In my own double star discoveries, separations have always been measured larger by van Biesbroeck, my measures standing between his and Giacobini's.

Sahade remarks that the evidence from the study of β Lyr itself now pointed towards an absolute magnitude of about -4.5, and the present model takes this fact into account. The existence of the visual companion and its implications was not considered at the time the 1957 paper was written. Heintz replies that he had referred to later papers.

Muller: Que pensez-vous des estimations de distances pour les couples très serrés par un bon observateur? En tenez-vous compte dans les calculs d'orbites?

Heintz: Of course I utilise them; I cannot see an objection.

Reference added in proof: A more detailed study is being published in the author's monograph *Doppelsterne* (Goldmann Verlag, Munich 1971).