IS THIS ORBIT REALLY NECESSARY?*

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Some years ago I made the unfortunate statement: "Orbit computation leaves nothing to be desired. Whenever a double star has been adequately measured—and not infrequently long before this stage has been reached—one or more orbits are certain to appear."[1] I wish to amend this statement. In recent years I have become more and more convinced that orbit computation leaves a great deal to be desired. Too many orbits are being published that do not give us useful, reliable information. I have no wish to spoil the sport of those who prefer computing the orbit of a double star to solving crossword puzzles. But why *publish* such orbits, taking up space in astronomical periodicals that could be put to better use? The situation is aggravated by the fact that publication is often unnecessarily verbose. Instead of simply the results of the computation and the means of judging their reliability, we are given in great detail the various steps leading to the final result. Why?

There are, in my opinion, two main kinds of orbit computation that fail to give information that is both useful and reliable: (1) recomputation of a definitive orbit and (2) computation of a premature orbit. In the first case the information, though reliable, is not useful; in the second it is neither reliable nor useful.

By "definitive" I mean an orbit based on *reliable* measures covering *at least* a full revolution. Even the best orbit is likely, in the long run, to show an increasing deviation between observed and computed positions. Clearly, the period is the only element responsible. It may be desirable to bring the orbit up to date by correcting the period, but it is quite unnecessary and a waste of time to recompute the orbit; the changes in the other elements are bound to be insignificant.

As an example I take Finsen's orbit[2] of A 111 AB = ADS 450. A recent observation gave: 1961.67, 174°.0, 0".13, while Finsen's orbit gives for this epoch: 44°, 0".08. The difference is, of course, much too large to be an error of observation, but on the other hand,

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the orbit was based on measures from 1915, the discovery year, to 1937, inclusive, adequately covering the observable half of the orbit twice over. It was found that if Finsen's period is changed from 10.5 to 10.788 years ($n = 33^{\circ}.37$) and his periastron passage from 1940.1 to 1940.52, with all other elements unaltered, the orbit represents all the measures to 1961, inclusive, as well as can be expected. The ephemeris given by Finsen obviously remains valid, provided that the epochs are changed to conform to the corrected values of P and T.

As an example of unnecessary recomputation I take a recent orbit by Couteau[3] for β Delphini = ADS 14073. Even without any correction, Finsen's earlier orbit,[4] which was based on more than two revolutions, abundantly observed, represents the later measures to 1961 inclusive, as well as could be desired. So, of course, does Couteau's orbit. The three elements that give useful information are

	Finsen 1937	Couteau 1959
P	26.60 yr	26.65 yr
a	0''.480	0''.475
e	0.35	0.35

I fail to see that the new orbit has added anything of value to our knowledge of this system.

A particularly bad example of this practice was given years ago by N. Voronov of Tashkent, who published a flood of orbits, many of which were simple copies of some other orbit with small changes in the final, often meaningless, decimals. Fortunately, this extreme case has not so far been repeated, but some of our present-day computers seem in grave danger of becoming successors to Voronov. The tedious part of orbit computing is searching the literature for the observations; the rest is pleasant. When an orbit is revised after a few years, the computer is spared that laborious task and one is almost tempted to think that the revised orbit is merely a device for deleting the earlier computer's name and substituting one's own.

By "premature" I mean an orbit based on observational data so insufficient that its resemblance to the true orbit is purely accidental. If such double stars have comparatively short periods, they become a delightful source of entertainment to orbit computers: after a few years the observations deviate from the ephemeris, as could have been predicted; then comes a new orbit, almost though not quite as premature as its predecessor, by another computer or even the same one, until at last the elements begin to settle down and to show some resemblance to the true orbit. Incidentally, this practice can drive the poor compiler of an orbit catalog to distraction: he can hardly keep up with the spate of orbits produced in this manner. If, on the other hand, such premature orbits have very long periods, the computer is, from his standpoint, on much safer ground: the motion is so slow that it will be long after his death before observations begin to show how misleading the orbit is. At the Berkeley Symposium, Finsen showed the following comparison between the number of orbits available in 1938 and those available in 1961:

	Definitive	Reliable	Preliminary	Indeterminate or Premature	Total
1938	42	43	66	45	196
1961	50	75	173	195	513

These figures prove in a striking manner that the impressive growth in the total number of "known" orbits during the past quarter-century consists largely of preliminary and premature orbits. While the total number of binaries for which orbits have been computed increased by 317, the number of binaries with definitive and reliable orbits increased by only 8 and 32.

Some computers seem to believe that premature orbits are useful for warning observers that the pair in question is in need of measurement. I say, without fear of contradiction, that no observer who takes his work seriously needs any orbit, premature or not, to tell him which pairs he should observe. It is most unfortunate that so many double stars in need of observation remain unobserved for too long a time, but the reason is not that there are no orbits for them; it is simply that such pairs are very numerous, while observers using powerful telescopes in good climates are very few.

The number of orbit methods is already so large that no computer can be said to be familiar with most of them. By familiar I mean that he should not merely have studied them, but have applied them repeatedly in practice. Nevertheless, new methods continue to be added, some ingenious, a few useful, but in the majority of cases never applied again after the first application by their inventor. The needs of the orbit computer are fully met by a good working knowledge of perhaps three or four standard methods, a supply of common sense, accuracy in working with figures, and a clear understanding of the degree of reliability of his data. The last, perhaps the most important, is often sadly lacking.

If I had to give some advice to computers who, while they may have much experience in computing orbits, have little or none of measuring a difficult pair, it would be this: If you are thinking of correcting by a least-squares solution the orbit of a double star, the measures of which are neither abundant nor well distributed, forget it. The method of least squares is a splendid device when applied to problems for which it is suitable. Correction of a double-star orbit when the measures are of this kind is not one of them. As an instructive example of the sort of thing to which a blind application of the method of least squares may lead, I take an orbit given by Wierzbinski for A 2329 = ADS 1865.[5] The available measures consisted of a group in the fourth quadrant, another in a restricted sector of the second, an isolated mean of two nights by Aitken in the third, and a single night's very uncertain result by myself in the first, when the pair was too close for reliable measurement with 26-inch aperture—about as unsuitable a case for a least-squares solution as one can imagine. The solution gave a residual of $+45^\circ\!.5$ for Aitken's isolated measures. This looked like a misprint, but the ephemeris confirmed it. No orbit computer should accept for one moment the possibility that Aitken, at the 36-inch refractor, measuring a pair far from difficult for him, could be in error by half a quadrant, even on a single night, let alone two. The orbit should never have been published. It is an insult to the memory of that grand observer.

I am well aware that the foregoing remarks, while they may be approved by most double-star observers and some computers, may give umbrage to others. I regret this. All I can say is that they have not been made in a spirit of grumpy criticism, but in the hope that computers may think them over and ask themselves if the time and energy they devote to computing unnecessary orbits might not be spent more fruitfully in other ways. Computing orbits may be more pleasant and is certainly much more comfortable than observing in a dome at night. But I think that Aitken was wholly right when he wrote: ". . . an hour in the dome on a good night is more valuable than half a dozen hours at the desk in daylight."[6]

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[1] W. H. van den Bos, *Trans. I.A.U.*, 7, 274, 1950.

[2] W. S. Finsen, *Union Obs. Circ.*, 4, 445, 1938.

[3] P. Couteau, *I.A.U. Commission des Etoiles Doubles Circ.* No. 23, 1961.

[4] W. S. Finsen, *Union Obs. Circ.*, 4, 461, 1938.

[5] S. Wierzbinski, *Acta Astr.* 6, 201, 1955.

[6] R. G. Aitken, *The Binary Stars*, 2d ed. (New York: McGraw-Hill, 1935), p. 55.