TWENTY YEARS OF DOUBLE-STAR INTERFEROMETRY AND ITS LESSONS

W. S. FINSEN
Johannesburg, South Africa

If we may judge by the number of eulogistic review articles to which it gave rise, one of the main talking points among astronomers in the early nineteen-twenties was undoubtedly the application of the interferometer to optical astronomy. It was only natural that interest was focused mainly on the brilliant success of Michelson and Pease (1921) in measuring stellar diameters with the 20-ft beam interferometer on the 100-in Mount Wilson reflector. But almost equally exciting, to the double-star observer at least, was the success of Anderson (1920) and Merrill (1922) in resolving several very close double stars, notably Capella, also with the 100-in but with much simpler equipment. Perhaps the most courageous venture of all was the attempt made by Pease (1925) to measure the spectroscopic binary Mizar Aa as a visual double star with a separation of the order of only 0.″01. This he did by estimating the fringe visibility with the 20-ft beam oriented in four different position angles – a more exacting and tedious operation it would be difficult to imagine. However sceptical one may be regarding the reliability of so delicate a technique, Russell's (1927) orbit based on these observations still has a place in our catalogues if only as a monument to a daring experiment.

After such a promising start one would have expected energetic exploitation of the new technique with the largest telescopes available. It is true that at the Mount Wilson Observatory no time was lost in launching a project for a 50-ft beam interferometer (Hale, 1922) with which Pease (1930, 1931) succeeded in measuring the diameters of a few more stars in spite of many difficulties that were apparently not wholly solved at the time of his death in 1938. But Anderson's double-star interferometer involved no great difficulties of a technical nature and there was no reason why it could not have been put to immediate and highly profitable use. One thinks of the known very close doubles of which ADS 3701 = A 3010 is a baffling example, of the many orbit pairs that have hitherto been unobservable near periastron with consequent ambiguity of quadrant, and of the unknown 'Capellas' that still await discovery. It is therefore not a little surprising that the only serious use of the interferometer during the next decade, apart from the short but excellent series of measures by Jeffers (1945) with the Lick Observatory 36-in, was with an aperture as small as 13 in (Maggini, 1925). It strikes one as incongruous that a technique invented specifically to boost the resolving power of the largest telescopes should have been put to such prosaic use.

Why this should have happened is not altogether clear, but there is no denying the fact that the optimistic enthusiasm of the early days rather quickly gave way to scepticism.

Perhaps this was partly a natural reaction to the somewhat inflated precision claimed

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for the method, notably in the case of Capella. For this system Anderson (1920) computed an orbit based on five position angles and five separations on six nights with the adoption of some of the spectroscopic elements. This gave an average separation residual, disregarding signs, of $0\overset{''}{.}00001$. A later orbit by Merrill (1922), based on the spectroscopic period and 15 position angles and separations on 16 nights, brought the average separation residual up to $0\overset{''}{.}0007$, but even this will be taken with a grain of salt when we remember that it represents only 2 per cent of the fringe semi-interval or resolving power. Nevertheless, an accuracy of one ten-thousandth of a second of arc, quoting Michelson (1920), is frequently claimed in standard texts even to this day.

The disillusionment was all the greater, therefore, when it became known that the results obtained with small telescopes were affected by serious accidental or systematic errors, to such a degree, in fact, that they are commonly disregarded by orbit computers (van den Bos, 1927, 1951; Baize, 1949). I shall not go into the reason for this unfortunate state of affairs except to remark that there is good reason to think that it arose largely from imperfect understanding of the technique.

At the Union Observatory (now the Republic Observatory) Innes was greatly impressed by the success of Anderson and Merrill. At that time he was impatiently awaiting delivery of the 26½-in refractor which had been ordered in 1909, and he lost no time in placing an order for an interferometer for the new telescope, for he realized that he would not long remain satisfied with its direct-vision resolving power. But his interest in the interferometer soon gave place to distrust as he studied the later results obtained with it, and I well remember the day when, completely disillusioned, he finally cancelled the order.

During the next few years we became more and more impatient, as Innes had foreseen, with the inadequacy of our telescope in following up movers that had closed in, not to mention the growing list of pairs discovered or suspected in our survey that were always too close for reliable measurement with the micrometer or for confirmation of their duplicity. And there was, of course, no larger telescope in the southern hemisphere to which we could pass on the responsibility for these pairs. With this incentive my thoughts turned again to the interferometer as a last resort and I began to wonder whether we had not perhaps been too hasty in condemning it out of hand. I therefore rigged up a very simple interferometer of the Anderson type and experimented with it on a few bright stars (Finsen, 1933). We were naturally rather pessimistic at first but to our surprise we ran into very few difficulties. For example, γ Lup, which had appeared single for several years, and γ Cen, which was passing through periastron at a separation of $0\overset{''}{.}1$, were easily resolved and measured without difficulty. However, this rough-and-ready instrument was inconvenient in that it necessitated removal of the micrometer and in several other ways was not without its defects. Its use was therefore restricted to a few of the more important pairs (van den Bos and Finsen, 1935).

In 1948 I felt that the time was ripe for serious exploitation of the method. The next two or three years were devoted largely to experiments with artificial double stars (Finsen, 1951b) and to development of the eyepiece interferometer (Finsen, 1951c, 1954, 1964a). This instrument, which in its final version has been in use since 1954, takes the form of an attachment to the Repsold micrometer and weighs only $1\frac{3}{4}$ pounds. It is easily manipulated with the fingers of one hand, the thumb and forefinger being used for rotation in position angle while the middle and third fingers vary the slit separation, including complete withdrawal of the slits when desired for direct observation with full aperture. One may therefore switch from interferometer to micrometer or *vice versa* at will. A recent important addition was what I have called the Airy-Simms eyepiece (Finsen, 1967) which admirably solves the vexing problem of atmospheric dispersion – a problem which is inseparably tied up with the use of high resolving powers.

The observing programme has included a systematic survey for new pairs as well as routine measurement of practically all close pairs within the range of the interferometer and observable from our latitude.

The survey has involved more than 13000 examinations of 8117 stars between declination +20° and −75° down to magnitude 6.5, extended to 7.5 for stars with proper motions exceeding 0".05 in either coordinate. This has yielded 73 new pairs or nearly one per cent of the stars examined, a surprisingly large number in view of the restricted range of separation and difference of magnitude. Less surprising is the considerable orbital motion shown by many of them; for the eleven pairs listed in Table I it has already been possible to compute orbits.

TABLE I

φ		IDS	Mags.	Sp.	P (years)	a
347	81 Cnc	09068N1524	7.2–7.2	G9 V	2.65	0".1258
312	ε Cet	02347S1218	5.6–5.6	F5 IV–V	2.672	0 .114
363 AB		09159S6816	6.1–6.1	dF5	3.2	0 .1235
316	G Vel	08505S4708	6.1–6.1	Am	7.27	0 .1044
331 Aa	75 Ori	06116N0959	6.1–6.1	Al V	8.35	0 .1299
327	χ^2 Sgr	19192S2442	5.8–5.8	dA5	10.8	0 .1230
335		08305S3215	7.2–7.2	G5	12.5	0 .1253
357		18528S6854	6.6–6.6	dF9	13.58	0 .1306
342	70 Tau	04199N1543	7.2–7.2	dF5	15.5	0 .1316
326	θ Ant	09397S2719	5.4–5.6	F7 V	18.83	0 .1275
329		21086S5341	6.5–6.5	A7 V	21.0	0 .1449

The routine observing programme has yielded nearly 6000 measures of pairs that would have been, in many cases, far too close for measurement with the micrometer. This has naturally resulted in orbits of greater reliability than would otherwise have been possible, including resolution of quadrant ambiguities that had previously defied the micrometer, as, for example, IDS 11005S2645 = φ 47 = χ^1 Hya.

The question springs to mind at once in view of the unreliability of some earlier interferometer observations: how trustworthy are these results? It would, of course, be foolhardy to claim that no spurious doubles have been listed in the survey or that no completely erroneous measures have been made, but I believe that they are few in number and probably no more frequent than would be the case in direct observation with the micrometer. In the course of the survey (and also in the measuring programme) it has always been my practice to accept duplicity as explanation of variable fringe visibility only when all other possible explanations have been tested one by one. Fortunately, van den Bos was able to examine several doubtful cases with the Lick Observatory 36-in telescope or the McDonald Observatory 82-in and I do not recall a single one that he was unable to confirm. It is true that some new pairs, although measured on several nights in the year of discovery, have not been seen double since; one is tempted to think that such cases may be spurious, but on the other hand it is at least equally likely that they were caught at greatest separation and have since closed in.

On the whole, the accuracy of the measures seems to be rather satisfactory, if we may judge from the study made by van den Bos (1953) of the earlier results. However, the separations fail to bear out the early claims made for the method but are liable to an uncertainty of the order of 10 per cent. This seems much more realistic, for the fringes cannot be compared to the graduations of a finely-divided scale as regards accuracy of setting.

I suspect that I have sometimes tried to observe stars that were too faint – a temptation difficult to resist. But van den Bos found rather unexpectedly that the internal accordance of measures of faint pairs was no worse than for bright pairs, perhaps because faint pairs were observed only in better seeing conditions. For the fainter stars the survey has certainly not been as exhaustive as for brighter stars, but there was no relaxation of the precautions mentioned earlier and the incidence of spurious doubles should therefore be no larger.

Perhaps it may be helpful if I try to draw up a balance sheet of the advantages and disadvantages of the method even if it is based entirely on my own limited experience with a refractor of relatively modest aperture.

The main advantage and, indeed, raison d'être of the interferometer is the gain in resolving power compared with direct vision. On bright stars it is no exaggeration to say that the resolving power is doubled. In fact, for separations down to about $\frac{4}{5}$ (or perhaps even $\frac{2}{3}$) of the theoretical interferometric resolving power or fringe semi-interval it is still possible to measure position angles with an accuracy rather better than in direct observation, with the same telescope, of 'elongated' or 'notched' pairs of approximately double the separation (Finsen, 1951d). For a reflector the gain in resolving power may be even greater. For example, van den Bos considers the McDonald Observatory 82-in reflector to be equal in resolving power to a refractor of aperture two-thirds that of the reflector. With an interferometer the effective resolving power of the 82-in would therefore be trebled.

On fainter stars wider slits are necessary, with a consequent decrease in resolving power. However, very much the same is true in direct observation of faint stars, for which one is forced to use eyepieces of lower power, so that the relative gain may still be a factor of two.

It is our experience that there is little to choose between interferometer and direct vision as regards sensitivity to seeing. Seeing that is good enough for the one method is almost invariably good enough for the other. But in identical circumstances the inter-

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ferometer has the advantage of greater resolving power; in other words, if seeing is judged by the closeness of the pairs that can be measured, the advantage is always with the interferometer.

Disappearance of the fringes as the result of duplicity is quite a striking phenomenon, much more so than the appearance of an 'elongated' or even 'notched' diffraction disk, and does not require as high a magnifying power. For a double-slit interferometer (Anderson or eyepiece interferometer), assuming normal visual acuity, the exit pupil should not be greater in diameter than about 2 mm if the potential resolving power is to be fully realized. This corresponds to a magnifying power equal to half the aperture of the object glass in millimetres. In practice a somewhat higher power is necessary; with the 26½-in telescope a power of 620 is adequate and I seldom use any other. This corresponds to an exit pupil of about 1 mm or a power equal to the aperture in millimetres. For direct observation of very close pairs the magnification would have to be doubled or perhaps even trebled. In this respect the interferometer therefore imposes less exacting demands on the seeing.

Finally, mention should be made of atmospheric dispersion which, if not neutralized in some way or other, places an effective limit on resolving power. Fortunately, with the interferometer rough compensation is easily achieved with the Airy-Simms eyepiece while any residual dispersive effect (instrumental as well as atmospheric) is automatically neutralized. This results from the fact that the fringes are themselves not achromatic, but exhibit graduated dispersion in both senses on either side of a central achromatic fringe. Should any small extraneous dispersive element be introduced, there will in general be a fringe whose dispersion is approximately equal but opposite in sense. In consequence, this fringe will now appear achromatic and adjacent fringes nearly so. This compensation is effective until the achromatic and nearly achromatic fringes approach an extremity of the fringe array, and only then is it necessary to introduce deliberate compensation to bring the achromatic fringes back to an approximately central position (Finsen, 1951a, 1955a, 1964b).

So much, then, for the advantages of the interferometer, and very impressive they undoubtedly are. But what of the disadvantages? Unfortunately, it is impossible to gloss over the fact that they are serious enough to make one think twice before putting it to use.

The greatest disadvantage of the interferometer is certainly the fact that one does not see duplicity directly but must deduce it from the delicate and elusive fringe manifestations. One must be on continual guard against misinterpretation, and observing with the interferometer is therefore both slow and tiring. Three or four complete measures in an hour is a good output, but often a measure may take an hour or more to complete. In carrying out a survey a star may be dismissed as single after only a minute or two, but if variation of fringe visibility is suspected it may again be an hour or more before one is able to make up one's mind. The interferometer is therefore much more prodigal of valuable telescope time than is the micrometer.

It follows from this that casual or occasional use of the interferometer, without the benefit of considerable experience, is fraught with danger. In general, one would be well-advised to leave the interferometer alone altogether unless one is prepared to become a specialist in its use. It is certainly necessary that the observer should be completely familiar with his instrument and I myself am strongly of the opinion that it should be regarded as a personal instrument not available for general use.

A great disadvantage of the interferometer is its restriction to bright stars and pairs with small difference of magnitude. The limiting combined magnitude is about six or seven, or perhaps a little fainter for an experienced observer, and seems to be nearly independent of the size of the telescope if the potential resolving power is exploited to the full (Finsen, 1955b). Pairs with a difference of magnitude exceeding 0.5 are difficult to measure with the interferometer and the limit may be placed rather optimistically at 1.0, but it is doubtful whether the situation is really any better in micrometer measurement of very close pairs.

When there is only a small variation in fringe visibility it is often difficult to disentangle separation and difference of magnitude: the components may be equally bright with a separation below the resolving power of the interferometer, or the effect may be caused entirely by difference of magnitude. And if there is some difference of magnitude, the quadrant is generally indeterminate unless the pair is wide enough to permit direct observation by withdrawal of the slits.

There is clearly no need to labour the point that the disadvantages of the interferometer may well outweigh its advantages. This will certainly be true if we try to make use of it when there is no real need to, for it cannot compete in scope, reliability or productivity with larger telescopes of the same resolving power.

To fix our thoughts let us assume that an 82-in reflector is available for regular double-star work and that it is equivalent in resolving power to a 54-in refractor. A 27-in telescope would theoretically have the same resolving power when used with an interferometer, but in practice I have found that such a combination cannot really compete with the McDonald 82-in and micrometer. There would therefore be no justification whatever for an interferometer programme with an aperture of 27 in or less. (The programme carried out with the $26\frac{1}{2}$-in refractor at the Republic Observatory was justified only because no larger telescope was available for double-star work in the southern hemisphere, or likely to be in the near future.)

On the other hand, with an aperture of 36 in or greater an interferometer should rope in many pairs not observable directly with an 82-in reflector and such a programme might well be worth undertaking.

But the most rewarding project of all would be an interferometer programme carried out with the 82-in itself, that is, *with the largest telescope available for double-star work*. Its use in this way as an instrument of last resort is, I believe, its only proper and logical role and is surely what the Mount Wilson pioneers had in mind half a century ago.

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Discussion

Van den Bos' observations at the 82″ McDonald reflector were made before the secondary mirror was refigured, as Finsen explains to Nather. The instrument may well be now more nearly equivalent to a refractor of corresponding aperture. The discussion on measuring very small separations is postponed until later papers.

Pecker asks if Moon-based and spacecraft-borne telescopes may be useful to measure double stars. Finsen doubts the feasibility of such a project; a large number of double stars is to be observed, and a continual watch needs to be kept on them, so this is a long-term programme. This being the case, it is once again stressed (Rösch, Lacroute) that more facilities for double-star observation with large earth-based instruments are needed, and that all possibilities of obtaining and processing data should be exploited.