THE HISTORICAL SEARCH FOR STELLAR PARALLAX

(*Continued*)

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Unsuccessful Attempts at Direct Measurement. While discussions of the angular diameters of stars and photometric estimates of their distances may have been both useful and interesting, they lacked the excitement that a direct proof-positive measurement of trigonometric parallax would bring. Thus far more effort was lavished on attempts at direct measurement than on any secondary arguments.

Probably the first person to use a telescope to search for annual parallax was Robert Hooke, working in Gresham College (forerunner of the Royal Society) in London. In July, August, and October of 1669 he made observations of the zenith distance of the star γ Draconis, using an enormous zenith sector 36 feet long. The reason for choosing γ Draconis was that it passes very close to the zenith in London, so that problems arising from refraction in the earth's atmosphere would be minimized. It is important to notice that the early attempts were at measuring what today would be called absolute parallax, rather than relative parallax, which is the parallax of a nearer star with respect to that of a distant star. Ultimately, only one of the three first successful attempts achieved this much more difficult task of measuring absolute parallax. However, Hooke announced that γ Draconis showed a parallax of between 27″ and 30″. For reasons that are no longer clear, this result does not seem to have attracted the attention that one might have expected, although possibly a negative result by Picard from similar measures on Vega a few years later (Lemmonier 1741) may have cast doubt on Hooke's result. That people realized the difficulties of refraction in these results can be seen from a suggestion by Wallis (1693) that such zenith distance measures be replaced by measures of the maximum azimuths of circumpolar stars, where, to first order at least, refraction corrections would cancel. But this, apparently, was never put into practice.

And now John Flamsteed reenters the picture. Fresh from his triumphs of measuring the angular diameters of stars, he began in 1689 a program to determine the fundamental places of stars by meridian observations with a mural arc. Some ten years later he concluded from an analysis of the zenith distances of Polaris that the latter showed a parallax of 40″. This he announced in a letter to Wallis in 1698, only to have the news leak out to his old enemy Gregory, who, it seems, tried to get Wallis to suppress the result. Flamsteed blew up again, and in a darkly worded letter warned Wallis of just how nefarious a character "the Doctor" really was:

> I fear you mistake him much: his friends are neither friends to you nor me; they resort commonly to Mr. Hindmarsh's, a bookseller's shop in Cornhill; and who they are you may learn at Oxford (Baily 1835, p. 167).

One wonders.

Somewhat earlier, in 1692–3, Olaus Roemer in Denmark had likewise noticed a series of annual irregularities in the declinations of stars and, a little later, in their right ascensions. He preferred, however, to attribute these to variations in the direction of the earth's rotational axis. But the young English astronomer Horrebow was so convinced that Roemer had found parallax that he used the results as the basis for a book entitled *Copernicus Triumphans.*

In truth, neither Hooke nor Flamsteed nor Roemer were mistaken in their observations, only in their interpretations. What they had unwittingly done was to discover stellar aberration, but the realization of this had to await another assault on the problem of stellar parallax thirty years later, this time by James Bradley.

Bradley's search for stellar parallax and the results that stemmed from it is so well-known a story (see, for example, Blackwell 1963, Shapley and Howarth 1929, Woolley 1963) that only an outline need be given here. In 1725 Bradley and his friend Samuel Molyneux set to work to repeat the observations made by Hooke more than fifty-six years before. Molyneux had a special 3¾ inch, 24 foot telescope made, and this was fixed vertically to the chimney stack of his house in Kew (with appropriate holes cut in the floorboards and roof), so that only stars close to the zenith could be observed. With this instrument the two men began measuring the meridian zenith distance of γ Draconis on December 3 of 1725. Within two weeks Bradley realized that there had been a significant change in this quantity, which hardly seemed possible in so short a timespan if the effect was a parallactic one. Thinking they were mistaken, the pair made a careful check the next clear night, which was December 21. Not only was the displacement real, it had increased. Bradley, unlike Hooke or Flamsteed before him, now began to think carefully about the possible causes of this effect. He soon realized that it definitely could not be due to parallax, because the displacement was in a direction at right angles to that expected from parallax. Like Roemer, Bradley concluded that it was most likely due to nutation – a 'nodding' of the earth's axis caused by the pull of the moon on the earth's equatorial bulge and the fact that the moon's orbital plane does not coincide with either the earth's equatorial plane or the ecliptic. In other words, Bradley thought the likely explanation was that the position of the zenith had shifted rather than the position of the star. But if so, he quickly realized, then observations of a star on the other side of the zenith to γ Draconis should show the same effect in the opposite direction. Within two weeks he had checked this: the expected direction was there, but the displacement was of the wrong size. Nutation might be playing a part, but it couldn't be the whole explanation. Bradley was nonplussed.

He decided now that he needed observations of more stars over a greater range in zenith distance, and because Molyneux' telescope could cover a range of only about a tenth of a degree, Bradley now began using a smaller telescope which was more moveable. He also wanted to be quite sure that another instrument would give the same results. There were twelve stars bright enough to observe both by day and by night that came within the limits of his telescope and a good many fainter ones visible only by night. Bradley settled down to a long series of careful observations to find how the displacements depended on a star's coordinates, which took him into 1728. Still he couldn't find the explanation, until, so the story goes (Thomson 1812), he was on a sailing boat on the Thames and noticed how, each time the boat put about, the pennant on its mast changed direction. The direction of the pennant depended not only on the direction of the wind, but also on the speed and direction of the boat. This is an aberrational effect. Put another way, it is like the driver of a car driving through rain which, to a bystander, appears to be coming straight down. The driver, however, finds his windshield gets wet, the rear window does not; the rain appears to be slanted towards him at an angle that depends on the downward speed of the rain and the forward speed of the car. A driver travelling in the opposite direction finds the rain slanted from the opposite angle. This, Bradley realized, was what was apparently making his stars change position. As the earth moves around in its annual orbit it changes direction, travelling in opposite directions six months apart, so that the 'downfalling' starlight appears to shift direction.

Bradley could check this explanation. He estimated the earth's orbital speed from the best available distance to the sun and the fact that it takes a year for the earth to travel around the circumference of the orbit having that radius. He also recalled Roemer having first established a figure for the velocity of light (from observations of Jupiter's satellites – when the earth is further from Jupiter the light signalling an eclipse of a satellite has further to travel and the eclipse appears delayed). Given the earth's velocity and the velocity of light, Bradley could calculate the expected aberrational shift in direction for any star. He compared such calculations with his actually observed shifts, and as he himself remarks, was amazed at how well they agreed.

There are a number of implications of Bradley's discovery (McCrea 1963), but by far the most important was that by actually showing the earth to move, he had given the first direct proof of the Copernican hypothesis – something that he and all the others had been trying to do for so long by searching for stellar parallax.

Bradley, incidentally, returned to the problem of nutation, and by steadily keeping up his observations for eighteen years, succeeded in showing that it too existed and affected the positions of stars in the sky.

But as for parallax, Bradley had to admit failure. Taking out the effects of aberration and nutation, he searched his most careful observations for any residual effect that might be due to parallax. The best he could say was that if γ Draconis had had a parallax of as much as $1''$ he would have observed it. The search would have to continue.

The great accuracy of Bradley's observations and their failure to detect any star's parallax seem to have put something of a damper on further attempts at direct absolute measurement for the remainder of the eighteenth century. Instead, observers such as William Herschel turned to attempts at measuring relative parallax, which we shall consider presently.

The opening of the nineteenth century, however, brought what people hoped was an improvement in instrumentation since Bradley's time, and once again the cry was on for measures of absolute parallax. The first efforts of the century came from observers in Italy. Giuseppe Piazzi had in 1800–01 been engaged in producing a catalogue of fundamental star positions when he made his famous accidental discovery of the first known asteroid. It was from these same stellar observations, supplemented by some made with an alt-azimuth instrument, that he finally concluded he had found parallaxes in three out of seven bright stars (von Zach 1808). Aldeberan was assigned a parallax of $1\overset{''}{.}6$, Sirius $4''$, and Procyon $5\overset{''}{.}7$. Almost simultaneously, Piazzi's countryman Calendrelli (von Zach 1808) claimed to have measured a parallax of $4\overset{''}{.}4$ for Vega.

Once again, as in the case of Robert Hooke many years earlier, these claims seem only to have stirred minor interest. There is evidence that some leading astronomers of the day were, perhaps unconsciously, coming to the conclusion that stellar parallax would forever be immeasurable. With hindsight we can now see that they were closing in on the few tenths of a second accuracy needed to finally measure a true parallax, but at the time there was no indication that stellar parallaxes might not lie in a range several orders of magnitude smaller. Thus there was a tendency on the part of some to almost automatically relegate all positive claims to the vale of, in John Flamsteed's plaintive phrase, "coarse observations."

This attitude is evident on the part of one of the participants in what is perhaps the most celebrated controversy in the long history of the search for stellar parallax. John Brinkley was Andrews Professor of Astronomy at the University of Dublin, and in 1814 published a paper* claiming that as a result of observations made with an eight-foot circle, he had found absolute parallaxes of 1″.0 for Vega, 2″.7 for Altair, 1″.1 for Arcturus, and 1″.0 for Deneb. He was opposed in this viewpoint by the Astronomer Royal, John Pond, who considered the Greenwich Observatory to have the world's finest instruments suited to absolute parallax searches, and with which he was unable to find any parallax for any of the above stars – or, for that matter, any others. Brinkley, however, was not one to give in easily, and the Brinkley-Pond dispute raged back and forth through the pages of the Royal Society Transactions for over a decade (Brinkley 1814, 1818, 1821, 1822, 1824a, 1824b; Pond 1817a, 1817b, 1818a, 1818b, 1822a, 1822b, 1822c). In general they were very courteous towards one another in their published papers, although one can sense the rising acerbity as the debate continued. Basically, the argument was a highly technical one devolving upon such matters as the best refraction tables to be used, and a long list of possible instrumental instabilities and errors. Pond concluded his side of the argument with the following words (Pond 1822c):

> The history of annual parallax appears to me to be this: in proportion as instruments have been imperfect in their construction, they have misled observers into the belief of the existence of sensible parallax. This has happened in Italy to astronomers of the very first reputation. The Dublin instrument is superior to any of similar construction on the Continent; and accordingly it shows a much less parallax than the Italian astronomers imagined they had detected. Conceiving that I have established, beyond a doubt, that the Greenwich instrument approaches still nearer to perfection, I can come to no other conclusion than that this is the reason why it discovers no parallax at all.

In the same paper Pond firmly stated that from exhaustive searches at Greenwich for both the absolute and relative parallaxes of Vega neither of these could be as much as 0″.1. Now at the time it seems to have been taken (e.g. Main 1840) that Pond had firmly squashed Brinkley and that that was that. Brinkley's reputation ever since has been that of a man who thought he had discovered something when in fact he had not. History, however, must award Brinkley the last laugh, albeit an ironic one. For Pond had finally gone too far when he claimed that Vega could not have a parallax amount-

*An amusing aside is that this volume of the Proceedings of the Royal Irish Academy contains two indexes: one headed 'Science,' the other 'Polite Literature'.

ing to 0″.1; it in fact has a parallax of 0″.12 or a double parallax of 0″.25. (Early workers, Brinkley in particular, often spoke of parallax when, in modern terminology, it was the double parallax they were referring to. It is sometimes not clear which is being referred to in a given case.) Brinkley, in his concluding paper (1824b), claimed that the reason why Pond could find no parallax was that he had rejected too many of his own observations; any that appeared to lead to a positive result Pond rejected as being probably faulty. Brinkley claimed that from a more reasonable approach to the Greenwich observations as listed by Pond, he himself could find a parallax of between 0″.1 and 0″.5. But by then the onlookers had grown tired of the debate, and Pond had nothing more to say. Brinkley's final claims passed into the might-have-been of history.

The general mood was that attempts at measuring absolute parallax by observations of meridian zenith distance or declination were unlikely ever to be satisfactory. This was sustained by reports from Taylor in the early 1830s (Main 1840) that attempts to measure the absolute parallax of Altair at Madras, India, by this means had failed, and from Airy (1837) that the latest round of measures on Vega at Greenwich had yielded no significant results. At the end of 1839 Thomas Henderson, Astronomer Royal for Scotland, read a paper to the Royal Society indicating negative results for Sirius (Henderson 1840). But on January 3 of 1839 he had read a paper to the Society which suggested that observations made by him at the Cape of Good Hope seemed to yield a positive parallax for α Centauri (Henderson 1839), but so gloomy was the prevailing view of such absolute parallax measures that the paper met with little but scepticism. We shall return to this paper, for in it Henderson had ended the centuries-long search for absolute stellar parallax, although it would take some years for his claim to be substantiated.

Long before this, though, astronomers had turned to the idea that their best hope lay in searching for relative rather than absolute parallax. That is, in making differential measures of one star with respect to another.

Probably the most celebrated effort to determine relative parallax involves the use of double stars. Although the name of William Herschel is best known in this connection, the idea goes back at least as far as Galileo:

> For I do not believe that the stars are spread over a spherical surface at equal distances from one center; I suppose their distances from us to vary so much that some are two or three times as remote as others. Thus if some tiny star were found by the telescope quite close to some of the larger ones, and if that one were therefore very very remote, it might happen that some sensible alterations would take place among them. ... (*Dialogue*, p. 382).

But Galileo was in no position to put the idea into practice, for he had no sufficiently sensitive micrometer with which to make measurements.

Although William Gascoigne in England invented the filar micrometer in 1640, it wasn't until some three decades later that it came into more general use. Galileo's suggestion then took on a practical cast and in 1673 James Gregory wrote a letter to the Secretary of the Royal Society suggesting that double stars be used in the search for parallax. Gregory, however, died within two years, and does not seem to have undertaken the work himself. Huygens, sometime in the later seventeenth century, and Long, in the early eighteenth century, were the first to actually try the observation, but both confessed to total failure in the detection of any relative parallax (Grant 1852, p. 549). It would be interesting now to know the details of their work, for if they were working with sufficient accuracy they may have unknowingly shown the likelihood that true physical binary stars do exist, a conclusion not accepted for almost another century.

The existence of physical binaries was first shown, as any reader of elementary texts knows, by William Herschel. And although rightly hailed as a great discovery, it is, from the standpoint of stellar parallax, a rather curious story. Right from the beginning of his astronomical career Herschel was taken up with the question of determining stellar distances, and in his earliest sweeps of the skies he began cataloguing double stars with a view to using them to look for parallax in the manner suggested by Galileo. In December of 1781, having recently become famous through his accidental discovery of Uranus, he read a paper before the Royal Society detailing his approach to the problem, and showing how the use of close double stars would obviate all the old difficulties of refraction, aberration, etc., that so plagued the searches for absolute parallax (W. Herschel 1782). The first curious aspect enters through the publication of this paper; the Royal Society's Publications Committee, acting as referee, drew Herschel's attention to a paper by John Michell (1767), a remarkable English astronomer now all too often forgotten by his modern counterparts. Michell's paper disagreed rather strongly with Herschel's over the question of how apparent magnitude is related to distance, but even more importantly, it contained a very simple statistical calculation on the question of the probability of finding two stars very close together on the sky, presuming them to be randomly distributed and given the total numbers of stars of any particular apparent magnitude. Michell's calculation showed that this probability is exceedingly low, so the fact that many such pairs are observed must imply that they are not chance coincidences in direction, but real physical pairs. It was not a difficult calculation and Herschel could easily verify it for himself, yet he seems to have purposely chosen to ignore it and proceeded with his observations. (A physical system, having both stars at the same distance from us, would of course show no relative parallax.)

But this raises a second curiosity. For while Herschel seems to have been unimpressed by Michell's argument, he does not seem to have been very ardent in his pursuit of binaries as candidates for parallax. One might have expected intense work on some of the binaries for a few years, and then an announcement as to whether or not any showed parallactic effects, but instead Herschel published two catalogues of binaries with relatively crude measures of position angle and separation in the early 1780s, and then nothing more on the subject for twenty years. In 1802, after years of work on galactic structure and nebulae, he returned to his double stars and remeasured many of them. It was this series of measures which revealed the existence of orbital motion (W. Herschel 1802, 1803, 1804), and so vindicated Michell's probability arguments and opened a vital door to future astrophysics. As such it was the second major piece of serendipity to emerge from the search for parallax, but in one way at least it was an unwelcome result for Herschel. By providing definite proof that not all stars have the same absolute magnitude, it cast serious doubt on the validity of his years of effort at fathoming the depths of the Galaxy. Herschel, it is clear, never really reconciled himself to this, and as the Dictionary of Scientific Biography puts it: "It is notable that Herschel ignored the implication of his own discovery. ... [He] was not prepared to abandon the hypothesis [of equal magnitudes] despite this conclusive evidence to the contrary, and indeed his career in sidereal astronomy can be seen as a prolonged rearguard action in defense of the hypothesis in the face of ever increasing counter evidence." Not every great discovery is welcome.

Herschel's son John, in a typically ingenious and elegant paper, suggested another way in which relative parallax might be looked for among double stars (J. Herschel 1826). Like his father before him, he assumed that the two stars were not a physical pair, so that the parallax of one would be much greater than that of the other. The ingenuity of the method lay in pointing out that if a pair were chosen such that their line of separation lay at about right angles to the expected direction of parallactic displacement, then even a small relative parallax would cause a quite large change in the position angle of the binary. Herschel showed that for separations of a few arcseconds, a parallax of one arcsecond would cause changes in the position angle of several tens of degrees, so that even a much smaller parallax should be readily detectable. The effect, of course, could be distinguished from true orbital motion by its annual periodicity. Novel though the idea was, however, there is no record of its being tried in practice, and in any case, as we can now see, it would almost certainly have been doomed to failure because of its initial premise that the stars were at very different distances. Almost any binary that might have been observed then, with very small separation and only a few magnitudes difference between the components, would have been a physical system.

The difficulties of making sufficiently accurate angular measurements to detect relative parallax led some observers to quite a different approach. This was to look for the effect of parallax in measured right ascensions of stars, and so replace angular measurements with time measurements. The principle can be seen quite simply. Consider first that the effect of parallax is always to displace a star in the direction of the sun. Thus a star on the meridian at sunset will be displaced westwards and have its right ascension diminished, while a star on the meridian at sunrise will have its right ascension increased. Hence the difference in their right ascensions will be increased by parallax, but six months later the situation will be reversed and the difference in right ascensions made too small by parallax. In an introduction to his own reductions of Bradley's observations of fundamental stellar positions, Bessel in 1818 worked out the mathematical details of the method and proceeded to search Bradley's observations for the effect. It was necessary to have stars separated in right ascension by about 12 hours, and bright enough to have been observed so many times that random errors in individual observations would cancel. Bessel chose two pairs: Sirius and Vega for one, and Procyon and Altair for the other. After the most rigorous examination, however, he had to admit failure; the first pair showed no parallactic effect at all, and while the second pair seemed to show some slight effect, Bessel was doubtful of its significance. This conservatism of Bessel's would later add credence to his eventual claim to having measured the parallax of 61 Cygni.

And finally, to complete this survey of ever-widening attempts at finding ways of detecting stellar parallax, we may note the introduction of the method of dynamical parallax. This too involves the use of binary stars, but now requires that they in fact be physical systems. Kepler's third law, applied to the orbit of one star in the system relative to the other is

$$(M_1 + M_2)P^2 = a^3$$

where M_1 and M_2 are the masses of the stars in solar masses, P the orbital period in years, and a the semi-major axis of the orbit in astronomical units. The observations of separation, however, are made in arcseconds, and the analysis then yields a in arcseconds. To obtain the correct units, Kepler's third law must then be written

$$(M_1 + M_2)P^2 = a^3/p^3$$

where a is now in arcseconds and p is the parallax in arcseconds. This equation can be rewritten as an expression for parallax:

$$p = a/[(M_1 + M_2)P^2]^{\frac{1}{3}}.$$

The only quantities not known on the right-hand side are the masses. We now know that there is only a fairly restricted range in stellar masses, so that setting $M_1 = M_2 = 1$ solar mass will usually cause no great error since the masses enter only as the cube-root. (It is possible to improve this by an iterative procedure involving the use of the mass-luminosity relation.) This idea was first put forward by F.G.W. Struve in 1837 in the introduction to a catalogue of double stars, the paper, in fact, which contained the first publication of a correctly measured stellar parallax.

Struve calculated the dynamical parallaxes of several binary systems, obtaining, for instance, 0″.24 for 70 Oph and 0″.25 for 61 Cyg (modern values 0″.20 and 0″.29 respectively). Despite these accurate values, however, the method suffered the same drawback as the photometric method, *viz.* the assignment of solar quantities to other stars, and this was simply a rank assumption. But at least Struve gained the confidence that, because of the cube-root factor in the calculation surely his dynamical parallaxes must be correct to within an order of magnitude. At last it was known with reasonable certainty that the greatest parallaxes would turn out to have values of the order of tenths of an arcsecond.

Struve at the same time drew attention to a very important correlation. This was the fact that the binaries with the largest dynamical parallaxes in general showed the largest proper motions. Finally people came to realize that merely the brightest stars might not be the best candidates for parallax searches – high proper motion is a much better criterion of nearness.

Thus the groundwork was laid for ultimate success. The dynamical parallaxes gave strong indications of what sort of values to look for, the searchers became attracted to stars of large proper motion, and the instrumental improvements, thanks to the work of Joseph Fraunhofer, were to hand.

Ultimate Success. Friedrich Georg Wilhelm Struve, or Vasilii Yakovlevich Struve, as he was later to be known, was born in 1793 in Altona, now a suburb of Hamburg, but then a part of the Duchy of Holstein. As a boy his interests lay in the classics, and had it not been for one of those curious accidents of history he might well have become a classicist. But on a summer's day in 1808, the fifteen-year-old Struve was visiting Hamburg when he was captured by a recruiting party from Napoleon's army, then occupying the city. An unguarded moment on the part of his captors, however, gave the boy a chance to leap out a window and escape home. His father, fearing reprisals, decided he should go much farther afield, and sent him to join his brother, a lecturer in classics at the University of Dorpat in the Russian Baltic Provinces. And thus the father of Russian astronomy, in a story resembling that of another German expatriate, William Herschel, came to his adopted country. At first he continued with his classics, and in 1811 was awarded a degree in the subject for a dissertation on the philological works of Alexandrian scholars. He then became a school teacher, and it was during this period that he became friendly with a man named Parrot, who taught astronomy at the University, and who aroused Struve's strong but latent interest in that subject. Parrot was instrumental in not only training Struve in astronomy, but in arranging for him an assistantship to the Professor of Astronomy. Struve's brilliance in his new-found subject was such that not only did he obtain his doctorate within two years, but at the same time there was created for him a special professorship. He soon also became the University's Rector, but, finding his taste for administration slight, later resigned that (though retaining his headship of the University's fire-brigade!).

In 1824 the University of Dorpat obtained the world's largest refractor, a 9-inch telescope by Fraunhofer, as well as auxiliary equipment which included one of Fraunhofer's filar micrometers. Superbly equipped, Struve immediately began work on double stars. There was no publish-or-perish syndrome in Struve's day, though, and it was not until 1837 that he published any results. These came in a monumental monograph entitled *Stellarum duplicium et multiplicium mensurae micrometricae, institutae in specula Dorpatensi*, in the introduction of which Struve reviewed the whole problem of stellar parallax and brought out the discussion of dynamical parallaxes and proper motions already referred to. There then followed the 11,000 measures of double stars. In a final chapter, Struve analysed his measures of one of them, Vega, to show that they actually revealed the presence of a true parallax.

While others, such as the Herschels, had advocated the use of very close doubles, not realizing that this almost guaranteed a physical rather than an optical system, Struve had had the good fortune to hit on a wide pair containing one very bright star, the optimum situation for an optical system. The parallax he derived for Vega was 0.″125, which may be compared to the modern value of 0.″123. However, in one of those unkind twists of history, Struve in this instance lacked the courage of his convictions, and put this result forward in only a very tentative manner, suggesting that additional observations were called for. Had he been more positive in his stand, prompting others to re-analyse his observations (as they were later to do) and confirm his result, it is likely that he would have obtained the recognition as being the first to find a parallax. (Confirmation would come from a check that the direction and proportion of parallactic displacement varied correctly as a function of time of year.) Instead, Struve continued observations for another year or two, and then published (Struve 1839) a revised value of $0''.2619 \pm 0''.0254$ for the parallax of Vega. This unfortunate result, with its large change, served only to put Struve further into the background on the question of priority.

Struve at this stage was much taken up with the design and his future directorship of what was to become one of the world's greatest observatories, that at Pulkova. He thus steps out of the parallax story at this point, although his interesting life (including two marriages and eighteen children!) had still a long way to go.

Friedrich Wilhelm Bessel was some nine years older than Struve, and, like the latter, started out in a career quite different from astronomy. As a young man he spent years as an apprentice in a shipping firm in Bremen, studying geography and foreign languages and the economics of foreign trade. It was while preparing to become a cargo officer on a merchant ship that he first encountered mathematical astronomy in the form of a course on celestial navigation. His imagination was immediately fired by the whole subject and soon he was spending all his spare time studying mathematics and astronomy. In 1804 he presented Heinrich Olbers with an extensive piece of work on the orbit of Halley's Comet, and Olbers, being an expert in the field, immediately recognized the singular abilities revealed in the paper. It was soon published and attracted the attention of professional astronomers around the world. Two years later Bessel had given up his mercantile career to become an assistant in the private observatory of another wealthy amateur, Johann Schröter, in Lilienthal near Bremen. By 1810 he had become Professor of Astronomy and director of the observatory in Königsberg, which had just been built by order of the King of Prussia. The professorship required a doctoral degree, but on the recommendation of Gauss, the University of Göttingen awarded Bessel this degree without further formality.

Bessel was launched on his brilliant career in fundamental positional astronomy. As we have seen, this included a long-standing interest in the search for stellar parallax, but after his early efforts *circa* 1818 had failed, he put the matter aside for some considerable time. It was specifically Struve's announcement in 1837 of the parallax of Vega, as well as Struve's suggestion that high-proper-motion stars become prime candidates for parallax measurements, that brought Bessel back to the search. He was quite open, even after his subsequent success had been widely praised, that it was to Struve that he was indebted for the inspiration to continue the work. In a letter dated October 10, 1838, read before the Königsberg Academy of Sciences, Bessel says:

> Allein das Jahr 1837 war frei von Hindernissen, und auch die Hoffnung des Erfolges, welche Struve auf seine Beobachtungen α Lyrae gründete, war geeignet dem Vorsatze, eine neue Beobachtungsreihe über 61 Cygni anzufangen, neue Kraft zu geben.

Bessel chose 61 Cygni simply because it was the star of highest proper motion known to him. It is in fact a double star, but Bessel knew that both components have the same proper motion, and that it is, therefore, a physical system. He therefore did not attempt to measure the parallax of one component with respect to the other, but chose two faint stars up to 12′ away from the binary, and forming a right-angled triangle with it. This was an ideal arrangement, for it not only provided series of perpendicular measures to take maximum advantage of the parallactic displacement, it provided two quite independent series on the two binary components, and another series between the two comparison stars to eliminate any spurious instrumental effects. Bessel had the further advantage of having available for his measurements probably the most accurate instrument of his day: a heliometer made by Fraunhofer. The heliometer was a refractor with a rotatable and split objective, the two halves of which could be offset along the line of division with a micrometer. Each half imaged the field, and separations between two stars were measured by noting what offset was required to superimpose the image of star A from one objective on the image of star B from the other. Not only was it more accurate than a filar micrometer, but it could measure larger angles. Its achievements were not to be exceeded in accuracy until the advent of long-focus photographic refractors at the end of the nineteenth century.

Little wonder then that Bessel's observations met with resounding success. What is interesting though, from the standpoint of our age where every hint of a major success is immediately proclaimed in the pages of the *New York Times*, is the casual manner with which Bessel announced his success. After well over a year of work, he wrote a letter to John Herschel. Dated October 23, 1838, it begins:

> Esteemed Sir, – Having succeeded in obtaining a long-looked-for result, and presuming that it will interest so great and zealous an explorer of the heavens as yourself, I take the liberty of making a communication to you thereupon. Should you consider this communication of sufficient importance to lay before other friends of Astronomy, I not only have no objection, but request you to do so.

The letter (printed in *Mon. Not. Roy. Ast. Soc.* **4**, 152, 1838) then continued at some length to give the details of the measurements, and announced the parallax of 61 Cygni as 0$''$.3136 ± 0$''$.0202. The modern value is 0$''$.292 ± 0$''$.004. The observed displacements were all in the right direction and proportion for the times at which they were observed, so that right from the first announcement there was no doubt that Bessel had definitely triumphed. At long last, beyond all argument, a stellar parallax had been well and truly measured.

Bessel was already in the afternoon of his life when he accomplished this. Most of his other great achievements in astronomy, geodesy, and mathematics were already behind him, and six years later he was found to have cancer. He died within two years, in great suffering, at the age of 61.

Thomas Henderson, the third of the three men who almost simultaneously were the first to measure stellar parallax, is a far more shadowy figure than either Struve or Bessel. Obscure, in fact, to the point that it was recently found impossible to locate a portrait of the man. Born in Scotland in 1798, he, like the other two, began a career far removed from astronomy; in Henderson's case it was law. But in the early 1820s he developed an interest in astronomy as a hobby, and whenever his legal work took him to London he made a point of attending meetings of astronomers there. Soon he was making original contributions to the subject, and before long had become quite well known in professional circles. When the directorship of the Royal Cape Observatory in South Africa became vacant in 1831, Henderson, apparently with some reluctance, allowed himself to be persuaded to give up his law career and accept this position. He arrived in South Africa in 1832 and found it a far cry indeed from his beloved native Scotland. He took an instant dislike to the observatory, always thereafter referring to it as 'Dismal Swamp', and on a later occasion wrote to one of his successors: "I will tell you about my residence in Dismal Swamp among slaves and savages – plenty of insidious venemous snakes. What would you think if, on putting out your candle to step into bed, you were to find one lurking beside the bed?" His intense detestation of the Cape was aggravated by indifference on the part of his superiors in London over matters of assistants and new instruments, and after only a year he resigned on the distinctly flimsy pretext of not getting improvements to the Observatory's sanitary arrangements.

Nevertheless, in the year Henderson was at the Cape, he carried out an incredible number of observations, both in depth and variety. In a way, from the standpoint of priority in determining a stellar parallax, this was his undoing. He left the Cape with an enormous quantity of raw data, which, along with his new duties as Astronomer Royal for Scotland, took a number of years to reduce. Unknown to Henderson, his observations of α Centauri in 1833 showed that star to have a sensible parallax, but it was not until he later discovered (from Johnston on St. Helena) that it has a large proper motion and that, despite being a wide binary, it shows rapid orbital motion, that he realized the star would be a good candidate for a parallax measurement. Thus five years elapsed between the actual observations and their analysis. In fact, there is a small mystery over this, because Henderson reports that he heard about the star's large proper motion at the time the ship returning him to Britain called at the island of St. Helena. Why, then, did he wait another five years to examine the observations for parallax? It seems quite likely that once again it was Struve's seminal paper of 1837, in which attention was drawn to the importance of proper motion, that finally prompted Henderson to analyze his results. Be that as it may, it was not until January of 1839 that Henderson read a paper before the Royal Society announcing the parallax of α Centauri (Henderson 1839).

Unlike Struve and Bessel, whose observations for parallax were differential ones, Henderson's were much cruder absolute ones made with a mural circle, i.e. they were part of a program to establish the fundamental right ascensions and declinations of southern stars. Except for the good fortune that α Centauri happens to be the star of largest known parallax, the observations would have been too crude to show the parallax. In fact, to Henderson's chagrin, there was some question as to their reliability, the results from the right ascension measures differing somewhat from those of declination. Had he only known at the time, says Henderson, "a much greater number of observations, and of such as would have been adapted for ascertaining the parallax, would have been made. ..." The right ascension observations gave a parallax of $0\overset{''}{.}70$, the declination ones $1\overset{''}{.}41$, which Henderson combined for a mean of $1\overset{''}{.}16 \pm 0\overset{''}{.}11$. Thus there were considerable reservations on the part of other astronomers about this result, particularly in view of the long history of unfortunate claims from absolute measures, and it was not until Thomas Maclear, Henderson's successor at the Cape, had carried out further careful observations of α Centauri which yielded a parallax of $0\overset{''}{.}98 \pm 0\overset{''}{.}02$ (Henderson 1842), that Henderson's claims were generally vindicated. Even so, the result, compared to the modern value of $0\overset{''}{.}760$, was not particularly good.

These events might have been the making of a more famous career for Henderson, but, like Bessel, his life was almost over, and in 1844 he died at the relatively early age of 46.

Since there is often considerable murkiness over the chronology of these three parallax determinations, it might be as well to summarize the course of events. Henderson was the first, in 1833, to make actual observations which showed a parallax, but he failed to reduce these observations for another five years. Struve's observations, principally those of 1835–6, were shown by him in 1837 to indicate a parallax for Vega. This result was in fact accurate, but deemed less so by Struve himself, and then revised to a considerably different value in 1839. This resulted in other astronomers having at the time considerable reservations about Struve's work. His paper of 1837, however, was instrumental in stimulating Bessel, and probably Henderson, to look again at the problem of stellar parallax. In Bessel's case this resulted in a carefully planned observational attack on 61 Cygni that was carried out with excellent instruments and consummate skill, and resulted in a parallax that was definite beyond question. This was announced in the autumn of 1838 and immediately hailed as the long-sought-after breakthrough. At the same time Henderson was examining his observations of α Centauri, made with a much cruder instrument for a different purpose, and in January of 1839 he announced they showed a parallax. But because of a difference between the right ascension and declination results, and because they were absolute measures, credit was generally withheld until confirmation by Maclear in 1842.

Thus it was that the Royal Astronomical Society decided to award its gold medal of 1841 to Bessel alone, although it is clear from the records that there was some hesitation over this, and care was taken to remark that very likely Struve and Henderson had achieved parallax measures too.

Perhaps it is difficult now to feel the excitement that attended these results. It was, after all, one of the very greatest achievements of astronomy, comparable in our own times to man's first landing on the Moon. John Herschel, in presenting the gold medal, summed it up thus:

> Gentlemen of the Astronomical Society, I congratulate you and myself that we have lived to see the great and hitherto impassable barrier to our excursions into the sidereal universe; that barrier against which we have chafed so long and so vainly ... almost simultaneously overleaped at three different points. It is the greatest and most glorious triumph which practical astronomy has ever witnessed. Perhaps I ought not to speak so strongly – perhaps I should hold some reserve in favour of the bare possibility that it may all be an illusion – and that further researches, as they have repeatedly before, so may now fail to substantiate this noble result. But I confess myself unequal to such prudence under such excitement. (J. Herschel 1842).

In conclusion we might reflect briefly on the implications of this 'noble result'. Our present abilities to measure trigonometric parallaxes are less than an order of magnitude better than those of the 1830s (say $\pm 0\overset{''}{.}005$ now as against $\pm 0\overset{''}{.}02$ then), so that even now we can measure the parallaxes of only the nearest stars. We are lucky, therefore, that interstellar distances, already some 12 orders of magnitude greater than terrestrial distances, are not another order or two greater. For if so, we would likely even now not have measured a trigonometric parallax. Nor, probably, would we have succeeded with other geometric techniques such as statistical and secular parallaxes, moving clusters, etc., for proper motions would also be reduced by the same factor. Yet all of our photometric methods for measuring great distances, for studying the structure of the Galaxy and of the Universe itself, ultimately require calibration by these geometric methods. Astrophysics too could not have proceeded without knowledge of such things as absolute magnitude and effective temperature (the H-R diagram), which require distance determinations. Thus it can undoubtedly be said that were it not for the fortunate fact that interstellar distances are only 12 and not, say, 14 orders of magnitude greater than terrestrial distances, the history of modern astronomy and astrophysics would have been vastly different. Just what form it might have taken, though, I leave as a speculation to the reader.

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