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THE HISTORICAL SEARCH FOR STELLAR PARALLAX

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Coarse observations, made by honest well-meaning men, have more perplexed the astronomer than all their labors and dreams upon them can make him satisfaction for. Their pretty thoughts and conceits in the theories are always excusable and sometimes to be commended: but when rude and ill-managed observations and experiments are brought to confirm them, though they may serve the author's present turn, yet they become a load on the science, and at last turn to his shame and reproach ...

JOHN FLAMSTEED, 1685.

Introduction. The heliocentric theory of the solar system has an important consequence: if the earth orbits the sun and thus changes position, the nearer stars should appear to change position as seen against the background of distant stars. That is to say the nearer stars should show a parallax relative to the distant ones. This consequence of the heliocentric theory has been realized almost throughout its long history, and traditionally formed the crucial test of the theory. But while a positive result would, of course, favour the theory, a negative result would be inconclusive. Either the theory could be wrong, or even the nearer stars could be so far away that their parallactic displacements fell below the limits of detectability. For a very long time the former possibility was almost always preferred over the latter. Thus Aristotle had little hesitation in dismissing an earlier Greek theory of a sun-centred solar system, partly on the grounds that naked-eye observation reveals no stellar parallax. And after Copernicus had re-introduced the heliocentric theory, no less an astronomer than Tycho Brahe again dismissed it because of his inability to detect any star's parallax, although, as we shall see, he had seemingly good reasons for doing so.

But as the seventeenth century gave way to first the eighteenth and then the nineteenth, there was no longer any real doubt that the heliocentric system was correct, and the search for stellar parallax, now known to exist, slowly intensified until for some it became almost obsessional. There was no dearth of claims for positive results, and it was specifically the heated arguments over these that caused John Flamsteed to complain in the words quoted above. Ironically, Flamsteed himself was to fall victim, although in his case more because of an insufficient understanding of physics than because of "coarse observations".

But if there were many red herrings, there was also serendipity. Several very important and unexpected results emerged from the search for stellar parallax, among them, interestingly enough, a quite independent proof of the heliocentric theory's validity.

History, however, has largely forgotten most of the attempts at determining stellar parallax. The modern student tends to learn to his amazement that after several centuries of no more than sporadic effort, stellar parallax was suddenly established independently by three men, all within a few months of one another. In truth, by the opening decades of the nineteenth century almost every observer was working hard at the problem, so that with that pressure and a steadily improving technology, it is not entirely surprising that success was suddenly achieved on more than one front. Consider the words of Sir John Herschel (1842), taken from his address as President of the Royal Astronomical Society congratulating those who had finally succeeded in measuring stellar parallax:

> To accomplish this has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct apprehension, and so leading him on in hopeless, endless, and exhausting pursuit.

Florid though the words seem for today's reader, they clearly indicate the great efforts that had been made in the preceding years in trying to determine stellar parallax. The second factor, that of advancing technology, is also largely overlooked today. This is particularly unfortunate because the instrumental advances important for the problem were largely the work of one man, Joseph Fraunhofer, whose role in the achievement is usually ignored altogether. Both Bessel and Struve, the two men who received most acclaim for measuring stellar parallax, used Fraunhofer optics for their work, and it is almost certain that neither would have achieved what they did had they not had another of Fraunhofer's great advances: the recent introduction of equatorial mountings and clock drives.

So history, at least popular history, becomes distorted. It is the purpose of this paper to set out in a fairly systematic way the history of attempts to determine stellar parallax, and to show that they culminated naturally in the results of Bessel, Struve, and Henderson.

Unsuccessful Attempts: The Angular Diameters of Stars. By the eighteenth century there was no longer any doubt, among scientists at least, of the validity of the heliocentric hypothesis, so that the original need to detect stellar parallax as a test of that hypothesis became secondary. That purpose was superseded by another, equally important: the determination of stellar distances. Knowing the distance between the earth and the sun, a measurement of a star's parallax enables one to calculate its distance. So although the original aim of a direct geometrical determination of a star's parallax remained foremost, other methods of finding stellar distances also came to be considered. These generally were of two types: attempted measurements of the angular diameters of stellar disks, and a photometric method based on the idea that stars are suns.

Although he worked without the benefit of a telescope, it was the problem of stellar disks coupled with his inability to detect parallax that led Tycho Brahe to reject the Copernican system. In his *Progymnasmata* Tycho reports his estimation that stars of the first magnitude have angular diameters of $2'$, those of second magnitude $1\frac{1}{2}'$, third magnitude $1\frac{1}{12}'$, fourth magnitude $\frac{3}{4}'$, fifth magnitude $\frac{1}{2}'$, and sixth magnitude $\frac{1}{3}'$. He was, of course, grossly mistaken, and it would be interesting to know just how he arrived at these numbers since the resolving power of the unaided eye is about $3\frac{1}{2}'$. At the same time he was quite unable to detect the parallax of any star, which meant that even a first magnitude star must have a parallax significantly less than $1'$. Now it is very easy to show that there exists a simple relation between the linear diameter D of a star, its angular diameter θ, its parallax π, and the radius of the earth's orbit about the sun R. It is

$$D = R \times \frac{\theta}{\pi}.$$

Thus if the angular diameters of first magnitude stars were $2'$, while their parallaxes were much less than $1'$, it follows that the linear diameters of such stars must be much greater than twice the radius of the earth's orbit about the sun. Tycho, from the (erroneous) figures available to him, would have taken the sun to be about ten times the size of the earth itself, so to speak of a sun-like star having a diameter many times the size of the earth's *orbit* seemed completely absurd. He therefore firmly rejected the Copernican theory and invented a new theory that was essentially a compromise between Ptolemy and Copernicus. Still, he wasn't the last observer to err by a factor of twenty-thousand or so in his results.

Galileo was unimpressed by Tycho's argument. In fact, he doubted that Tycho had ever actually made the necessary measurements:

> To speak quite frankly, I thoroughly believe that none of them, not even Tycho himself ..., ever set himself to determine and measure the apparent diameter of any star except the sun and moon. I think that arbitrarily and, so to speak, by rule of thumb some one among the most ancient astronomers stated that such-and-such was the case, and the later ones without any further experiment adhered to what this first one had declared. For if any of them had applied himself to making any test of the matter, he would doubtless have detected the error (*Dialogue Concerning the Two Chief World Systems*, p. 361*).

Galileo had found one of the two difficulties involved in trying to measure the angular diameters of stars: that when the eye looks at a small brilliant object, irradiation (the scattering of light within the eye) makes it appear larger than it really is. The other difficulty, that even with irradiation removed, a combination of seeing effects in the earth's atmosphere and diffraction results in a completely spurious disk for a star, was not to be realized for centuries to come. Galileo announces the problem thus: "On account of [irradiation], bright distant objects are not represented to us as simple and plain, but are festooned with adventitious and alien rays which are so long and dense that the bare bodies are shown as expanded ten, twenty, a hundred, or a thousand times as much as would appear to us if the little radiant crown which is not theirs were removed" (*Dialogue* pp. 335–6). Galileo had a very simple and ingenious method for removing the effects of irradiation:

> I hung up a light rope in the direction of a star ... and then by approaching and retreating from this cord placed between me and the star, I found the point where its width just hid the star from me. This done, I found the distance of my eye from the cord [and] from the ratio of the thickness of the cord to its distance from my eye ... I immediately found the size of the angle ... In making the observation you will be astonished to see how thin a rope will cover that great torch which seemed incapable of being hidden except by a much larger obstacle ... By this ... operation I find that the apparent diameter of a star of first magnitude (commonly believed to be two minutes ...) is no more than five seconds ... (*Dialogue* pp. 361–2).

Thus, unknowingly, sometime before 1630, did Galileo make history's first estimate of astronomical seeing.

Yet there, having made so great an advance over Tycho's estimate of the angular diameters of stars, Galileo let the matter drop. He had, by implication, removed Tycho's objections to the Copernican system, and since he

*Pagination in this and subsequent references to the *Dialogue* is that of the translation by Stillman Drake, University of California Press, Berkeley, 1967.

himself had other suggestions for getting at stellar parallaxes, he did not attempt to put his determination to quantitative use. The other suggestions we shall come back to in later sections.

And now the telescope enters the picture. Galileo was well aware that using a telescope to magnify the stellar disk would reduce the problem of irradiation by reducing the disk's surface brightness, but it was a number of his successors who put the telescope to greatest use in attempting to measure the angular sizes of stars.

Johannes Hevel, usually known as Hevelius, was one of the great German-Polish astronomers of the mid-seventeenth century. He is often cited as the last important astronomer to work without a telescope, yet not only was he a telescope builder, but on occasion at least used them to good advantage. Hevelius discovered that using a mask to reduce the aperture of a telescope would diminish the brightness of stars, which seemed an excellent way of overcoming irradiation. Ironically, then, Hevelius placed successively smaller diaphragms over his telescope's objective until the stellar image presented a smooth round disk unaffected by irradiation and the size of that disk he then measured as the star's diameter. Ironic, of course, because as the telescope aperture steadily diminished, the size of the Airy diffraction disk steadily grew, and what Hevelius was really doing was simply finding the point at which diffraction would overcome seeing effects. But an understanding of that was almost two centuries away, and so, meanwhile, in 1661 Hevelius triumphantly announced an angular diameter of about 6″ for Sirius and 5″ for Procyon.

Such determinations of the angular sizes of stars became one of the most brisk controversies in astronomy at the turn of the seventeenth century. Hevelius' results were supported by similar measures made by Jacque Cassini in Paris in 1717, and by results rather vaguely reported by Riccioli in Italy. They were opposed by such as Christiaan Huygens in Holland, who had obtained negative results in his attempts at such measures. Significantly, Huygens had not used small apertures to reduce the brightness of a star, but had simply used a smoked glass for the same purpose.

Happily engaged in the battle was that stormy petrel of British astronomy, John Flamsteed. He was a firm believer in Hevelius' method, and while he had been a close friend of Huygens right up to the time of the latter's death in 1695, he thought Huygens had been wrong in claiming negative results:

> the particles of smoke being spread all over the eye-glass, the stars could only be seen through the interstices or spaces betwixt the particles, that were only points, and not wide enough to receive the image of a large star passing over them; so that the largest stars appeared no bigger than the point-like spaces through which they shone, *or as points* (Baily 1835, p. 207).

Flamsteed described his own early observations thus:

> 1672, Oct. 22, when Mercury was about 10 degrees high, I observed him in the garden with my longer tube (of 14 feet), but could not see the fixa near him, the day-light being too strong. Only I noted his diameter 16″, or a little less: for turning the tube to Sirius I found his diameter 15″, which I judged equal to Mercury's; the aperture on my object glass was ¾ of an inch, so that Sirius was well deprived of spurious rays, and shone not turbulently, but as sedate as Mercury, the limbs of both well defined, but Sirius best (Baily 1835, p. 205).

Isaac Newton was doubtful of this result, for Flamsteed tells us (Baily 1835, p. 205):

> I was with Mr. Newton on Friday last, and told him of this observation. He would have said something [about] the nature of the difform rays of light; but when I urged the smallness of the aperture on the object-glass, he let his discourse fall. There is nothing to be said against this observation ...

It wasn't like Newton to give up so easily, but of course doing battle with Flamsteed was a tiresome business, as Newton well knew.

Flamsteed's main opponent, however, was David Gregory, who had taken a photometric approach to stellar distances, and felt that the angular diameters of Flamsteed implied parallaxes that were completely at variance with his own estimates. Gregory wrote a pamphlet and spread it about that he did not think much of Flamsteed's work, which produced in Flamsteed a vitriolic outburst of nova-like proportions:

> Certainly the Dr. [i.e. Gregory] has a mighty opinion of his own authority ... But he is a closet astronomer; and having never thought of throwing away money on instruments, he had rather do his business by a party, on supposals, than observations. For the future he shall be *Doctor the fair Supposer* ... Now I am resolved not to be murdered ... and lest this not do, and other persons, as perverse as myself, should reject his suppositions with scorn, he will kill me with another demonstration upon another supposition still, that the diameters of the fixed stars are insensible ...

(Gregory's first supposition was that stars could be identified with the sun; his second that Huygens' observations gave insignificant angular diameters. Flamsteed rejected them both.) He continued piously

> Pray take notice that I do not attack any part of the Doctor's works: I only show you his unreasonable suppositions and wilful mistakes ... I pray God forgive him ... I am not his enemy, but can be his friend, even before he makes me reparation, as a good Christian ought (Baily 1835, p. 204–8).

Small hope. The battle diminished, but simmered on, unresolved, through the years.

In fact, a century later there was still hope that the parallaxes of stars might be arrived at through measures of angular diameters. In 1780 William Herschel considered the various ways by which parallax might be determined, and addressed himself to the angular diameter method. On October 22, 1781, he tried measuring Vega, and with a magnifying power of 6450 arrived at a diameter of 0″3553 (W. Herschel 1782). He himself, however, put little faith in the result, and passed on to other techniques. It was at least a great advance over Flamsteed's 10″ or 15″, but still more than two orders of magnitude too large.

Curiously, an observation which showed that all hopes of measuring the angular diameters of stars were futile had been made even before the death of Galileo. On the 19th of March, 1637, that remarkable pair of young English astronomers, Jeremiah Horrocks and William Crabtree, their great potential soon to be lost through death in the Cromwellian Wars, observed an occultation of the Pleiades by the Moon. Horrocks noted that since the stars disappeared virtually instantaneously their true angular diameters must be "mere points which are not capable of measurement" (Grant 1852). Edmund Halley (1718) later made the same observation, concluding that Spica and Aldebaran must have diameters much less than 1″, and as a consequence expressing his suspicion that Cassini's measure of the diameter of Sirius was an optical fallacy (Halley 1720).

Yet so slow are some results to take hold, that one must go almost to the time of Struve, writing in 1837, to find a firm and final statement that the angular diameters of stars are beyond the reach of ordinary micrometer measures.

Photometric Estimates of Stellar Parallax. An idea that took hold early was to estimate the distances of stars by attributing to them the sun's luminosity and then finding how far away they must be in order to have their observed luminosities. Potentially, it was a method of great power, and today, in modified form, is the basis of many of the most accurate and widely used methods of stellar distance determinations, but historically, of course, it suffered at least one major defect: in Flamsteed's scornful phrase, the supposition that stars are identifiable with the sun. Whatever results one arrived at, they always rested on that assumption, and so, although, as we now know, these results were often close to the truth, they carried no real weight in the definitive determination of stellar parallax. A second difficulty that bedeviled most of the early photometric results was how to determine the apparent magnitude of the sun.

One of the earliest photometric estimates of stellar parallax was made by Isaac Newton (1686), who ingeniously solved the problem of the sun's apparent magnitude by using Saturn to relate the solar luminosity to the scale of stellar magnitudes:

> ... the disk of Saturn, which is only 17″ or 18″ in diameter, receives only 1/2100000000 of the sun's light; for so much less is that disk than the whole spherical surface of the orb of Saturn. Now if we suppose Saturn to reflect about ¼ of this light, the whole light reflected from its illuminated hemisphere will be about 1/4200000000 of the whole light emitted from the sun's hemisphere; and, therefore, since light is rarefied inversely as the square of the distance from the luminous body, if the sun was $10000\sqrt{42}$ times more distant than Saturn, it would yet appear as lucid as Saturn now does without its ring, that is somewhat more lucid than a fixed star of the first magnitude. Let us, therefore, suppose that the distance from which the sun would shine as a fixed star exceeds that of Saturn by about 100000 times, and its apparent diameter will be 7^{v} 16^{vi} [= ″.000034]* and its parallax arising from the annual motion of the earth 13^{iv} [= ″.0036]* ...

It is not clear (at least to me) how Newton arrived at these figures for the diameter and parallax from the result of a distance 100000 times that of Saturn. The latter is quite reasonable, implying a distance of almost a million astronomical units, which corresponds to a parallax of 0″.22. In fact, the sun shining as a first magnitude star would have a parallax of 0″.55. But Newton's own figure for the parallax is two orders of magnitude smaller, although the parallax and diameter he quotes are mutually consistent. Whatever the reason, it is clear why Newton doubted Flamsteed's measures of angular diameters.

Newton also foresaw another difficulty that would take well over two centuries to resolve:

> Some may, perhaps, imagine that a great part of the light of the fixed stars is intercepted and lost in its passage through so vast spaces, and upon that account pretend to place the fixed stars at nearer distances; but at this rate the remoter stars could scarcely be seen. Suppose, for example, that ¾ of the light is lost in its passage from the nearest fixed stars to us; then ... the fixed stars that are at a double distance will be 16 times more obscure ... and those at a quadruple distance will be 1024 times more obscure; but so great a diminution of light is no ways consistent with the phenomena ...

A supposition of 30 magnitudes per kiloparsec for the rate of interstellar extinction was really going a bit far, but then Newton would by no means be the last to reject interstellar extinction out of hand.

There were a number of others who, over the course of more than 150 years, made very similar calculations. These began with James Gregory (not to be confused with David Gregory), who was probably the first to

*Newton is here using a now archaic extension of the sexagesimal system: $1° = 60'$, $1' = 60''$, $1'' = 60'''$, $1''' = 60^{iv}$, etc. The extension beyond arcseconds had fallen into disuse by the early nineteenth century.

attempt such a photometric estimate of stellar parallax. His result appears very briefly in an appendix to a work on geometry published in 1668, suggesting that Sirius is 83,190 times further away than the sun. This brief statement was never enlarged upon, for the genius of the young Scotsman was cut short when he was blinded by a paralyzing stroke while working at the telescope a few years later, dying within days at the age of 38. Later such estimates were made by Huygens, Chesaux, Lambert, Michell, and Olbers, all of whom agreed that the parallax of a first magnitude star must be under $0''.5$ (Grant 1852).

Particular interest attaches to the work of William Wollaston, an English chemist best known to astronomers for his discovery of the absorption lines in the solar spectrum. In a posthumously published paper, Wollaston (1829) reports an observation of sunlight reflected in a glass bead 0.1 inch in diameter, finding that the bead appeared equal in brightness to Sirius when the observer was 210 feet from the bead. From this, assuming the bead's reflectivity to be 0.5, he calculates a distance for Sirius of 141,421 astronomical units, or a parallax of $1''.8$ *if* Sirius has the same intrinsic luminosity as the sun. But by Wollaston's time direct geometrical observation had already shown that the parallax of Sirius must be under $0''.5$, whence the conclusion that Sirius must be intrinsically more luminous than the sun. It was the first indication that not all stars have the same absolute magnitude. If Flamsteed had still been living he doubtless would have given vent to a triumphant "I told you so".

This result cast yet greater doubt on the photometric method, which had always rested shakily on the assumption of equal intrinsic luminosities for stars. There was a suggestion by Struve (1837) that a statistical approach be used, in the hope that among a group of stars the average intrinsic luminosity would be that of the sun, but this led nowhere, and was in any case soon forgotten in the almost immediate discovery of a true geometrical parallax.

Essentially no more was heard of the photometric method of stellar distance determination for the remainder of the nineteenth century. The problem had to be turned around to await sufficient trigonometric parallaxes for the calibration of absolute magnitudes, and it wasn't until after the birth of astrophysics that the method was to come into its own.

(*To be continued*)