

ATTEMPTS TO MEASURE ANNUAL STELLAR PARALLAX:

HOOKE TO BESSEL

by

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ABSTRACT

One of the most obvious consequences of the Copernican theory is that the stars should appear to have shifted slightly when observed from the Earth at opposite ends of its orbit around the Sun. The angle through which the star appears to move is a measure of its annual parallax, a knowledge of which enables astronomers to calculate stellar distances. Parallactic angles, even of the nearest stars, are very small and despite many claims to have measured such an angle, three hundred years elapsed between the publication of Copernicus's theory in 1543 and the production of acceptable values of annual stellar parallax in the 1830s.

In this thesis I trace the development of interest in the problem of the detection of parallax through the period between Hooke's pioneering attempt in the 1660s to design and build an instrument specifically to measure parallax, and the eventual publication by Bessel in 1838 of the widely accepted value for the parallax of the double star 61 Cygni.

The production of suitable instruments was essential for the successful measurement of parallax, and I outline the main developments in telescope design. However, good instrumentation alone was not enough; in addition astronomers had to be prepared to devote their time to making very precise observations of carefully selected stars over a period of at least a year. Also, it was vital to develop effective methods of analysing the data once they had been accumulated. As the eighteenth century passed phenomena other than parallax that affect stellar positions were discovered - aberration of starlight, nutation of the Earth's axis and proper motions - so by 1800 the problems of data reduction were considerably more complex than they had been in 1700.

The thesis therefore traces the parallel developments in astronomical theory and instrumentation necessary for the eventual identification of parallax. Finally, I assess the significance of such a measurement within both the slowly emerging new discipline of sidereal astronomy, and the study of astronomy as a whole.

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Chapter One

INTRODUCTION

"If we are to understand the nature of heavenly bodies, it is essential to know how far away they are; only then can we calculate the sizes and energy outputs of planets, stars and galaxies, and ultimately determine the scale of the Universe itself". (1)

One of the basic measurements upon which all calculations of astronomical distances beyond the solar system rely is that of annual stellar parallax. Today, it is for that reason that the measurement of parallax forms a sometimes tedious, but nevertheless essential, part of routine activity at astronomical observatories (2). By now the distances of the closest stars - those lying within 10 light years of the Sun - are believed to be known from measurements of parallax to an accuracy of 2%. Interest in the measurement of annual stellar parallax has a long history, stretching back at least to 1543, and the larger part of that time tells of astronomers' failure to detect the phenomenon. In the main, it is with that history of failed attempts that this thesis deals.

"First it will be necessary to define the word parallax, which will be frequently used in the ensuing discourse and will be a stumbling block to those that do not well understand it". (3)

Thus said Edmond Halley in a lecture reputedly delivered at Gresham College sometime later than 1679 (4); he then explained that "the word is originally greek and signifies distance". Halley followed this with an astronomical definition of parallax as the angle subtended by the radius of the Earth at the centre of

any celestial object; from a knowledge of this angle and the Earth's radius, simple triangulation gives us the distance of the object from the Earth. As the subject of Halley's lecture was lunar parallax this was a sufficient definition. For our purposes, however, we must extend the basic concept; we are still concerned with an angle subtended at a celestial object, but by annual stellar parallax we mean the angle subtended at a star by the mean radius of the Earth's annual orbit. Because of the enormous distance of even the nearest stars, although the radius of the Earth clearly must subtend an angle at them, it is immeasurably small, and a larger baseline for the triangulation is required. The only other possibility is to use the radius (or diameter) of the Earth's orbit round the Sun; either of these is in fact just large enough to subtend very small, but noticeable angles at the Sun's nearest stellar neighbours. Originally astronomers sought the angle subtended by the diameter of the orbit, but since the early nineteenth century it has been standard practice to quote parallactic angles as those subtended by the mean radius.

A number of accounts of the history of attempts to detect parallax have been written, the earliest of which in fact antedates by 4 years the first successful measurements: in 1834 G. R. Fockens, a student at the University of Leyden, wrote a dissertation reviewing fully the state of the problem of parallax detection, and including a large section devoted to its history (5). Subsequently, during the 1840s, the German astronomer C. A. F. Peters, then working at the new observatory at Pulkowa, devoted much of his time to the study of stellar parallax. The results of his observations and reductions were published in 1846 by F. G. W. Struve (6), but Peters wished to add a detailed survey of all previous attempts to detect the phenomenon; he finally published this work in 1853 (7).

By the mid-nineteenth century, therefore, two detailed chronologies of attempts to detect parallax

existed. Since then the history has been sketched in all the major histories of astronomy (8); but of course these accounts cannot be detailed. If anything, the importance of the problem of parallax is overstated in historians' attempts to create a larger role for the history of 'stellar' astronomy than is justified. This is particularly true of nineteenth century histories of astronomy. During the second half of the nineteenth century the study of the stars had become as important an undertaking as that of the solar system, and those people engaged in writing the history of the subject deemed it necessary to trace histories of the two branches of astronomy of equal significance. This trend has diminished slightly during the twentieth century, but it is still present, as 'new' histories of astronomy are produced based in form on the old. Thus the only secondary sources dealing with the history of parallax available until recently tell us (to a varying degree) the main events specifically within that history. However, very little interpretation has been attempted, and the only means we have of estimating the significance at any time of the quest for parallax is to assess the importance attributed to it by the historian rather than by the astronomers involved. As pointed out above, this can lead to an overestimated or, at least, a distorted view of the role of the problem.

In the last decade two new contributions to scholarship dealing with the history of attempts to measure parallax have been made, one of which was included within the study of the emergence of a discipline labelled 'sidereal astronomy' (9), and the other of which, according to its title, is concerned specifically with the historical search for parallax (10). Unfortunately, in the former we find little more than a reiteration of the chronology, and in the latter J. D. Fernie confuses the issues of parallax detection with those of the

estimation of distance. The two issues are of course closely related, historically as well as at the present time, but they are not interchangeable concepts. By treating the history as though they were Fernie fails to recognise several of the problems, both practical and theoretical, which were important to the astronomers about whom he is writing.

In addition to the surveys of the whole history of attempts to measure parallax, there are a small number of papers dealing with particular aspects of the subject. Michael Hoskin has written a brief account of the use made by certain astronomers of double stars in their parallax work (11), and there are two papers concerned particularly with the final phase of the story: the successful measurements of parallax made in the 1830s (12). In none of these papers, however, does the author try to relate what he is writing about to any other aspect of the history of astronomy in general, or even of the history of parallax detection. The available secondary literature can therefore best be described as incomplete.

The overall view of the history of attempts to measure parallax which emerges from this literature is also misleading in places, principally because insufficient attention is paid to the underlying trends and interests prevalent in astronomy at any part of the period under consideration. As we are given little idea either of the role in the respective astronomical communities of the particular astronomers involved, or of the individual's assessment of the importance of the problem, we are left merely with a number of names and publications. The resulting picture is one in which, prior to 1728, several people appear to have rushed about trying desperately to measure the elusive phenomenon, and repeatedly to have failed; then in 1728 James Bradley wrote his famous paper in which he explained for the first time the effect of the aberration of starlight, and further pointed out that

parallax could not amount to more than 1" because if it did he would have detected it (13). The effect of this pronouncement, we are led to believe, was that for the following 70 years (with the exception of William Herschel) astronomers stopped looking for parallax, being convinced that it was too small to detect. However, by 1800, astronomical instrumentation had improved to such an extent as to make it worth astronomers' time and trouble to begin the search again. During the early years of the nineteenth century several attempts were made without success, but then, in the 1830s, three astronomers independently announced the parallaxes of the stars δ Cygni, Vega and α Centauri; these values were accepted as genuine and the long-term problem of parallax was solved.

This version of the history is necessarily condensed and highly abstracted; nevertheless it encompasses all the main points made in the secondary literature. Clearly it is time to reassess the history of attempts to measure annual stellar parallax. In the first place it is possible to construct a more coherent account of the history than has hitherto appeared, in which the changing views and opinions of the astronomers involved are considered. The most important point to remember is that we are always dealing with particular individuals who made specific decisions for discoverable reasons. Very often these individuals interacted with one another - usually commenting on their contemporaries' claims to have detected the phenomenon - and such interactions shed extra light on the very shadowy picture we have of the history we are investigating. For this reason it is essential that where possible we assess more than the published papers discussing parallax. Furthermore, even when the only remaining evidence is the published material, we can at least attempt to assess that within the context of the author's other work and interests.

The parts of the story which are covered best by the available secondary accounts are the early years

of investigations - during the second half of the seventeenth century - and the final measurements made in the 1830s. Yet even in these sections much remains to be told. Moreover, the long middle period, between approximately 1730 and 1830 has been very poorly dealt with. In fact, apart from the work of William Herschel, it is fair to say that the period 1730 to 1800 is not discussed at all. After this the early years of the nineteenth century are glossed over: the work of the Italian astronomer Guiseppe Piazzi is usually cited as marking the beginning of a general revival of interest in the detection of parallax at the major European observatories. Different accounts then refer to a variety of publications produced during the first twenty years of the nineteenth century (with no identifiable criteria for inclusion) with the general comment that none provided acceptable evidence of parallax. The period is summed up by Michael Hoskin as "the darkest hour before the dawn" (14). However, this is far from sufficient. If we consider closely all the evidence available to us, it is possible to say more about each stage in the history of attempts to detect parallax.

The first aim of this thesis will therefore be to give a finely detailed account of work specifically related to the problem of parallax, and to attempt an explanation of the motives for and the results of such work. Chapter Two will deal with the period 1660 to 1720 by considering the contributions of each of the individuals in terms of their aims and reasons for studying parallactic movements. It was during this period that many of the problems of parallax detection were faced for the first time and, although many of them remained unsolved, they were at least recognised. In the next chapter the specific question of the reason for the supposed absence of interest in parallax during the years 1728 to 1802 will be answered, by challenging the view that Bradley's paper on aberration had a completely negative effect on astronomers as far as the

detection of parallax was concerned. Admittedly there was never any enormous interest related to parallax during the last two thirds of the century, but there was some activity which must be evaluated. With the removal of 1728 as the great divide in the history of attempts to measure parallax we shall be left with a fairly uniform picture of interest in the problem up to about the turn of the eighteenth century.

Furthermore, a close analysis shows that the early years of the nineteenth century, those years of darkness, were crucial for the eventual successful identification of stellar parallax, mainly because they were so vitally important for the emergence of a particular practice of astronomy which has prevailed until the present day: the precise and rigorous study of astrometry. The process through which the study was established was complex; astronomers working during the early years of the nineteenth century were very well aware that the concepts with which they were working and which they were striving to understand were often ill-defined and insufficiently accurate. Their problems of precision - both theoretical and practical - and how they affected astronomers' endeavours to measure parallax will be analysed in Chapter Four.

As will be shown, many of the problems of precision, including the accuracy of observations and the difficulties of rigorous reduction, had been solved by about 1830. The person mainly responsible for their solution was F. W. Bessel and his work, particularly his early career, will be discussed at length in Chapter Five. Bessel's role both in the general emergence of astrometry and in the particular study of stellar parallax detection has never been adequately assessed; in fact, much of the relevant material has largely been ignored by historians. Specifically, the volumes of correspondence between Bessel and Olbers and Bessel and Gauss are never mentioned. Yet they provide a wealth of information about Bessel's astronomical

background, and they allow us several insights into the ways in which astronomy was changing during these crucial years of the early nineteenth century. The origins of many of the ideas which appeared in print can be traced in these letters, a number of which referred in particular to the problem of parallax.

At a rather more general level, the correspondences between several German astronomers (including Gauss/Olbers and Gauss/Schumacher as well as those already mentioned) reveal much about the determination of these men to perfect the study of astronomy. This was very significant for the final solution of the problems related to the detection of parallax, as will be explained in the early sections of Chapter Six. The achievement of a particular standard of accuracy in positional astronomy was essential before astronomers could realistically have any hope of detecting the phenomenon. However, achieving the necessary standard was not on its own quite enough to provide reliable measurements, and the other vital parallel developments are also discussed in Chapter Six. Finally in this chapter we shall reconsider the three 'successful' measurements, with the emphasis on the question of the reception of the values rather than the issue of priority, which is discussed at length in the available secondary sources.

From the account which will be given here of the history of attempts to measure parallax, it will become clear that the successful identification of parallax did not rely solely on the acquisition of sufficiently accurate instrumentation, as is implied in the secondary literature. Of course astronomers had to have highly accurate instruments before they could detect the phenomenon, and the main developments - principally the work of Jesse Ramsden in this country and later of Joseph Fraunhofer in Germany - will be analysed. However, mere possession of first class instruments was not in itself the automatic solution to the long-term problem of

parallax measurement. One of the most important aspects of this thesis is that it will demonstrate the existence of other vital factors: the inclination of individual astronomers to devote time to the problem, their conviction that certain stars only were likely to exhibit parallax and their reasons for deciding which, and, most importantly, the development of theoretical tools for the reduction of the data into forms useful for the purposes of comparison. In the final analysis it will be shown that parallax was eventually measured as a result of parallel developments in the theory and practice of positional astronomy; in fact the history of attempts to detect the phenomenon is one of the clearest illustrations we have of how closely related these two trends were.

Finally, the question of the significance of the quest for parallax to the astronomers involved will be analysed. This can only be answered by relating specific work on the problem to wider issues in astronomy. We must attempt to uncover how much of their time individuals were prepared to devote to the problem, whether they believed in their thoughts about parallax sufficiently to face up to all the practical difficulties known to be associated, and also how they rated the significance of the problem compared with their various astronomical (or other) enquiries. The picture of the development of the problem of parallax which emerges from such investigations differs sharply from that which has hitherto been available. In the whole history of parallax prior to the mid-1830s the detection of the phenomenon was almost never the single most important study for any of the participants. Indeed the only possible exception is the work of John Brinkley in Dublin; and even he, though by the 1820s quite determined to establish that he had succeeded in measuring parallax, was spurred on mainly because of personal differences with John Pond at Greenwich. It was only

in the 1830s with the special study of 61 Cygni made by Bessel that the situation changed.

Nevertheless, despite this negative view of the significance of attempts to measure parallax, it is very important that the history be written. Apart from providing a new and much needed interpretation of the development of the particular problem of parallax, it has definite implications within the history of 'sidereal' astronomy, and also the general history of astronomy. By reconsidering this particular history and setting it into a wider context than has ever previously been done, we can appreciate that there were definite influences between different fields of enquiry within astronomy. These influences should be investigated. Historians of astronomy should now be aiming for a synthetic view of the development of the subject as a whole. The history of astronomy consists of more than a succession of histories of individual branches of the discipline, each written as though the others do not exist.

Chapter Two

EARLY IDEAS 1660 - 17202.1 Prologue: the sixteenth and early seventeenth centuries

The question of the existence of annual stellar parallax first emerged in modern times in 1543 with the publication of Nicholas Copernicus's De Revolutionibus (1). As an obvious corollary of heliocentricity the 'fixed' stars ought to exhibit an apparent annual motion reflecting the actual movement of the Earth in its orbit around the Sun. Copernicus possessed no evidence of such a motion in any of the stars but he did not regard this as a problem. In Book I of De Revolutionibus he wrote:

"Although [the Earth] is not in the centre of the Universe, nevertheless its distance therefrom is still insignificant, especially in relation to the sphere of the fixed stars". (2)

Hence the stars were so far from the Earth that any parallactic effect must be indeterminately small. Copernicus did not dwell on the problem of the absence of annual parallax, and the apparent non-existence of the phenomenon seems not to have affected in any way the immediate reception of De Revolutionibus.

However, for Tycho Brahe later in the sixteenth century, absence of measurable parallax was an important factor in his rejection of heliocentricity. As he was unable to detect the phenomenon, he argued that the distances involved must be so great that there would have to be an enormous gap serving no useful purpose between the orbit of Saturn and the sphere of the fixed stars. As Tycho explained in 1588,

"... otherwise the annual revolution of the Earth in the great orb, according to [Copernicus's] speculation, will not turn out to be insensible with respect to the eighth sphere, as it ought". (3)

Such a vast gap had no use and would thus have no place in the economical Nature, in which Tycho believed. Further, an enormous gap between Saturn and the fixed stars implied that the size of the fixed stars must be ridiculously big if, as Tycho believed, their apparent diameters as seen from the Earth were as much as 1' (4). These arguments, together with Tycho's physical and theological beliefs, convinced him of the need for a new astronomical theory to replace both the obsolete Ptolemaic scheme and the new but contentious Copernican system. As Tycho was primarily an observational rather than a theoretical astronomer it is likely that his inability to detect parallax was a very important factor in his decision to cast aside heliocentricity. Unlike Copernicus, Tycho clearly tried to observe parallax; in a letter to Kepler written in 1598 Tycho said that he had made a series of observations of the Pole Star with the specific aim of measuring its annual parallax (5). It is really with Tycho's work that the detection of annual stellar parallax became a practical problem, or indeed any sort of problem.

It was also during Tycho's lifetime that astronomers began to derive mathematical (that is geometrical) descriptions of parallax. Early in 1573 in England both John Dee and his pupil Thomas Digges wrote books containing trigonometrical theorems on parallax. Digges's Alae seu scalae mathematicae (6) was the first of the two to appear, but it was followed within weeks by Dee's Parallaticae commentationis praxeosque... (7) Establishing the truth of Copernicus's system was of enormous concern to both astronomers, but response to De Revolutionibus was not the immediate reason for either publication. Both men wrote their books as a result of their observations and studies of the new star of 1572. All astronomers tried to measure the diurnal parallax of that particular object with the intention of establishing whether it was

atmospheric or terrestrial, and all obtained different results (generally depending on the particular axe each individual had to grind). Digges and Dee however investigated in addition the theoretical possibility of measuring an annual parallax; the unprecedented phenomenon of 1572 served to focus the attention of astronomers on the problems of parallax - both diurnal and annual - in a way that Book I of De Revolutionibus had never done.

By the end of the sixteenth century however, only Tycho had faced up to the overwhelming practical problems of detecting parallax, and his inability to find any trace of the phenomenon had led him to question the Copernican theory rather than his own methods of observation. It was otherwise when Galileo came to consider whether the absence of annual stellar parallax meant that the Copernican theory was untenable. On the third day of the Dialogues concerning the two chief world systems, when Salviati reached the point where he wanted to discuss the fixed stars in relation to the heliocentric theory, he began by destroying the basis of Tycho's argument from the observed apparent diameters of the stars (8). In characteristic manner Salviati dismissed Tycho's claim that the apparent diameters of the stars were at least 1', and replaced the figure with his own estimate of 5", which he justified with spurious experimental evidence. However, although Salviati's experiment to measure apparent diameters was dubious, he had gone straight to and destroyed the heart of Tycho's first observational objection to heliocentricity; Tycho's argument relied on his estimate of the diameter of stellar images and as Galileo, through the words of Salviati, pointed out, Tycho's value was, in any case, only an estimate.

After making this point clear, Salviati tackled the other part of Tycho's argument from observation: the problem of the absence of annual stellar parallax. The position adopted by Galileo was the same as that of

Copernicus: parallax was yet to be detected. But Galileo went further than Copernicus by suggesting ways in which the small angle might be measured. His idea was that certain stars should be observed as they passed behind a prominent marker placed on a distant horizon; if the observation were repeated at some later date, because of the effective length of the instrument (that is the distance between the observer and the marker) very small angular differences in the position of the star, even those resulting from parallax, should be detectable (9). The obvious objection to such a method is that the position of any star observed so close to the horizon will be affected greatly by atmospheric refraction, so that even if Galileo had tried his experiment he could not have obtained any results. Nevertheless, the significant point is that Galileo tried to come to terms with the problem of measuring a very small angle. And although there is no evidence that he himself put his idea into practice, the method was tried at at least one astronomical centre later in the seventeenth century. (See below, section 2.2).

Of even greater significance was the suggestion implicit in Galileo's discussion of the relative distances of the stars. The development of his ideas in fact formed the core of the method used by two of the three astronomers who succeeded in the 1830s in measuring annual parallax by the method of measuring relative parallax (10). In reply to Sagredo's remarks about the need for something beyond the sphere of the fixed stars against which the positions of the stars might be traced, Salviati said that he did

"... not believe that the stars are spread over a spherical surface at equal distances from one centre; [he supposed] 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 corresponding to those of the outer planets." (11)

In other words if a small and presumably therefore distant star were used as a reference point against which a larger and closer star could be observed, any difference in the angle separating the two should be easy to detect, even though it would be very small. Again, there is no evidence that Galileo sought parallax in this way, or even that he worked out the many important and advantageous implications of the method: this was left for his successors. However, Galileo's discussion of annual stellar parallax was significant for the long-term development of interest in the problem. Following Tycho, he made the detection of parallax an issue in the debate over Copernicanism, and the fact that his thoughts on parallax appeared in the widely read Dialogues meant that they were seen and noted by subsequent generations of astronomers.

Galileo was not the only person during the first half of the seventeenth century to refer to parallax or to stellar distances (12). He was however the most important and influential to do so, and he stated clearly the defence for the Copernican astronomer faced with the problem of the absence of sensible parallax: the phenomenon must be exceedingly small, and inability to detect it was no reason for rejecting the Copernican theory. Its absence meant merely that the stars must lie very much further away than had hitherto been suspected. Absence of parallax was of course used by anti-Copernicans in their arguments against heliocentricity (13), but lack of such a measurement was never the only, or even the most important, reason for any astronomer's rejection of the new astronomy.

Thus by 1650 the problem of the detection of annual stellar parallax was established on both theoretical and practical bases. Such a measurement was already perceived as a useful, but non-essential,

confirmation of the Copernican theory; and it was also appreciated, after the work of Digges and Dee at least, that the diameter of the Earth's annual orbit could be used in conjunction with parallactic measurements to establish stellar distances. But at no time between 1543 and the mid-seventeenth century was absence of parallax a crucial problem for any astronomer apart perhaps from Tycho. Prior to 1650 astronomers were not trying desperately to detect the phenomenon as a means either of refuting or of confirming Copernicus's theory, and they were not addressing themselves to the practical difficulties associated with the measurement of very small angles. Those who were convinced anti-Copernicans were content with astronomers' inability to detect the phenomenon. The onus to measure parallax was on the Copernicans if they wished for an argument in favour of heliocentricity stronger than that which placed the fixed stars at a very great distance. The first astronomers who really tackled some of the many problems associated with the detection of annual stellar parallax were those of the generation immediately after the mid-century.

2.2 Hooke, Wren and J. D. Cassini

In 1674 the first of Robert Hooke's Cutlerian lectures was published (14). Entitled An attempt to prove the movement of the Earth from observations, it concerned what Hooke called an experimentum crucis, the outcome of which was intended to establish the truth of the Copernican system. The 'proof' was to consist of a measurement of annual stellar parallax. There is no doubt about Hooke's motive in this case: he wished to provide evidence which, he believed, would demolish the arguments of the "anti-Copernicans". In order to do this he gave serious thought to the type of instrument he should use and to the choice of star to observe; then, in the summer and autumn of 1669, he actually made a

number of observations.

Hooke's 1669 observations are generally cited in the existing surveys of attempts to measure parallax as the starting point in 'modern' approaches to the problems of parallax (15). This claim is justified in so far as Hooke was the first to produce observations within a specifically defined project, in which he anticipated and did his best to allow for certain practical and theoretical problems. However, what the available accounts of early interest in the detection of parallax fail to explain is that Hooke was not the only astronomer of his generation who showed interest in the problems associated with parallax. His work in fact reflects the beliefs and discussions held by and between various of his contemporaries. As J. A. Bennett has pointed out in his account of Sir Christopher Wren's career in astronomy (16), it is highly likely that Hooke's project was a continuation of a similar one initiated by Wren some years earlier. We know from two separate sources that Wren had, during the 1650s, considered possible means of measuring parallax. In the Parentalia, a selection of Wren's writings and letters compiled by his descendent Stephen Wren, reference is made to Christopher's wish to establish whether or not the Earth moves (17), and in a letter written by Sir Robert Moray to Christian Huygens in 1663 a more definite picture was painted. Moray described how

"... Mr Neile ... told [him] that it was 10 years ago that he and Mr Wren had spoken of that business [of mounting a long telescope without a tube]... they proposed to place an object glass at some great height and to secure it there to observe and compare meridional altitudes of some star in order to try to discover whether it had a parallax ... " (18)

It is well known that Hooke worked closely with Wren on a number of projects during the 1660s (19), so it is not surprising that they should have exchanged

ideas on parallax as well as on a variety of other subjects.

Wren's interest in parallax recurred a number of times throughout his life. During the 1670s, when designing the London Monument, according to John Ward in his Lives of the Professors of Gresham College,

"The learned architect built it hollow, that it might serve as a tube to discover the parallax of the Earth, by the different distances of the star in the head of the Dragon from the zenith, at different seasons of the year". (20)

The building was started in 1671 and completed six years later, but Wren was never able to house a zenith sector in it because the structure was insufficiently rigid, being "liable to be shaken by the motion of coaches and carts almost constantly passing by". (21)

After his plan to use the 200 foot Monument, there is no evidence that Wren considered the problems of parallax again until the early years of the eighteenth century, but he was not the only one of Hooke's contemporaries to express an interest in the phenomenon. In 1675 the Scottish astronomer James Gregory wrote a letter outlining his thoughts on stellar parallax and its detection to Henry Oldenburg, Secretary of the Royal Society (22). He was writing in praise of Hooke's work, but it is clear that he had given the problem of the detection of parallax some thought in the previous few years. Gregory's main reason for seeking parallax was different from that of Hooke and Wren in that he was concerned primarily with the establishment of stellar distances rather than in the proof of the Copernican system (23). He had previously tried Galileo's method of observing the position of a star relative to a distant marker (24), but without success, and was now considering the other suggestion put forward by Galileo: observations of close pairs of stars.

Thus Hooke was not alone in his determination to measure stellar parallax; realising this makes it easier to understand Hooke's motivation than does a

reading of the available secondary literature. However, his work was more important than that of his contemporaries because he went beyond mere speculation as to the use that could be made of different instruments and buildings; he actually built a zenith telescope and used it to observe the movements over a length of time of a carefully chosen star. At the outset of the project Hooke appears to have been extremely anxious to carry out his "experimentum crucis". He was at least sufficiently keen that he was prepared to cut one hole in the roof of Gresham College, in which he secured the object glass of the telescope, and another hole in the first floor of the building, to allow light to pass through to the eye-piece at ground level, 36 feet below the objective. As Christopher Wren had realised, keeping the telescope rigid was both difficult and important; it was well known that the angle sought was very small so any deviation in the position of the instrument could have ruined the experiment. Also, it was not merely a matter of keeping the telescope steady while one reading was taken: it had to remain rigidly fixed over a period of several months. The problem was not made any easier by Hooke's decision to observe the star γ Draconis because it culminates very close to the zenith in London, making the problem one of mounting the telescope vertically. However, that decision was deliberate and important: both Wren and Hooke had chosen to observe γ Draconis to eliminate the effect of atmospheric refraction, which was so difficult to allow for at low altitudes. So Hooke faced the problems of securing as rigidly as possible an object glass of long focal length and of keeping it fixed relative to the eye-piece over a long period of time. As we have seen, his solution was to embed both parts of the telescope in the most secure structure to which he had access: Gresham College. As well as considering the problems of refraction and of the rigidity of the

telescope, Hooke realised that he also had to ensure that he could allow for expansion and contraction of the metal parts of the instrument and for any warping of the wooden parts. In fact he devoted a large section of his Cutlerian Lecture to an account of all the practical precautions he took with respect to the telescope (25).

After all the hard work that Hooke undertook at the beginning of the project it is surprising and a little disappointing that he should only have reported four observations of γ Draconis made during the summer and autumn of 1669. According to Hooke himself, after the fourth observation made on the evening of 21 October, bad health and bad weather prevented him from making any further observations; but why did he not make any more than four between June and October? Of course he was always very busy (26), and Hooke has always had the reputation for failing to finish the projects he began, or for rapidly losing interest in them. However the answer to this particular mystery may lie in the nature of the project; Hooke was looking for an effect which manifests itself completely only over a period of a year, and the important points in the annual cycle occur quarterly. Thus having made two observations of γ Draconis in July Hooke might not have thought it necessary to observe it again until the effect of parallax had had sufficient time to change the apparent position of the star by an appreciable amount.

Whatever the reason for the absence of any further observations, Hooke was not deterred from concluding from the few measurements he had made that γ Draconis possessed a noticeable parallax. He claimed that its annual parallax was between 27 and 30" (27). This result was passed on by Oldenburg to a number of astronomers in this country and on the Continent, and from the responses of each of these astronomers some account of contemporary interest in the problems of parallax can be constructed. Hooke's book was published

in March 1674 and within a very few weeks Oldenburg had sent copies to Huygens, J. D. Cassini, and Hevelius, asking each of them for their opinions of it (28). No one was prepared to accept unreservedly Hooke's value of 27 to 30" for the very good reason that he had made too few observations over too short a period of time. However, Huygens considered Hooke's work to be "of great consequence" (29) and Cassini felt that the endeavour "was not a vain one" (30).

The implications of Cassini's reply to Oldenburg were that efforts were being made in Paris to build suitable apparatus for observing parallax; observers there had detected annual alterations in the position of some stars which could not be accounted for by the precession of the equinoxes, but astronomers had not yet "been able to perform...premeditated observations concerning this question" (31). Cassini went on to say:

"We have a well-shaft at the Observatory for this, having an aperture in the vault (built over it) open to the sky which is fully 200 feet above the bottom, to which a convenient ladder leads; but (it has been) hitherto obstructed by the works of the masons constructing the upper vault. And so it has not been possible for us to attempt anything with it so far". (32)

Clearly therefore French astronomers had given the subject of the detection of parallax some thought, and they had reached conclusions similar to those of Wren and Hooke. They decided to observe stars as near the zenith as possible to avoid the problems of refraction. Again inevitably this involved having to support an objective lens of long focal length vertically above the eye-piece, but Cassini's solution to this particular problem differed from both Hooke's and Wren's. Cassini chose to embed the objective at the top of a well-shaft and to have the eye-piece below ground. There are traces that an attempt to use a well-shaft to house a zenith sector was also made by John Flamsteed in the late 1670s at the newly founded Observatory at Greenwich. Certainly

some optical parts which could have been used still remain, and according to Derek Howse, Flamsteed made at least one observation with such an instrument (33). In addition to this, the English astronomer William Ball wrote in March 1674 to Huygens saying that he too had plans to detect parallax using a well at his home in Devonshire (34). Thus the concept of using a telescope restricted in field of view to the zenith for the detection of parallax was obviously accepted and approved by a number of astronomers during the 1660s and 1670s. The difficulties lay in putting the idea into practice, and it is greatly to Hooke's credit that he succeeded at least in building a workable instrument, even if he did not obtain enough observations. It is also to his credit that he published his account of the major practical problems involved and of how it might be possible to overcome them.

However, in the aftermath of Hooke's publication there seems to have been a general reluctance on the part of astronomers to make any further attempts to detect the elusive phenomenon of parallax. A phase of decided interest in the problems and of real activity aimed at their solution was over. For about 30 or 40 years after An attempt to prove the movement of the Earth, although intermittent interest was still apparent, no astronomers other than perhaps the Dane Ole Roemer set out, in a way similar to Hooke, to obtain measurements of annual stellar parallax. Of course this does not mean that nothing of any relevance to the problem was produced during this time; it means rather that contributions to its development were all either of a purely theoretical nature, or that they were incidental to other astronomical work being undertaken.

2.3 Wallis, Flamsteed and Jacques Cassini

Hooke's An attempt to prove the movement of the Earth was the only account of a genuine attempt to measure parallax to be published during the seventeenth century.

Although a number of papers were written about the phenomenon in particular and references were made to it within larger works, none of these described the outcome of a project devised specifically with the problems of parallax in mind. In 1680, Jean Picard published in his Voyage d'Uranibourg a list of observations of the Pole Star taken over a period of 10 years, during which time the star appeared to have been subject to a small annual variation (35). Picard considered parallax as an explanation of these observations, but he also considered refraction and finally concluded that he could not say with any certainty what had caused the observed variation. Also, the Voyage was a scarce book before it was included in a selection of monographs compiled in the 1730s. Picard's observations would not, therefore, have been widely known when first published.

Over 10 years later, in 1693, another series of observations, this time taken over a period of more than 20 years, was sent to the Académie royale des Sciences by J. D. Cassini (36). He too had noticed a small periodic variation in the position of the Pole Star, but he wrote that it was unlikely that

"... one could attribute that to a real change in the position of the Heavens with respect to the Earth, or to a movement of the stars; and it is far more reasonable to attribute this difference to accidental causes". (37)

The important point about both Cassini's and Picard's passages is that in neither case did the astronomer gather the data with the intention of using it to detect parallax. Both were far more concerned with the establishment of a value for the precession of the equinoxes and each wanted accurate observations of the Pole Star to study how the position of the north celestial pole changed with time. Picard had been sent to Hveen so that he could make observations which could be compared with those obtained by Tycho a century earlier. This was all part of a larger scheme devised by Cassini for making accurate geographic and geodetic surveys of the Earth. Thus neither astronomer

was deliberately seeking parallax; both would nevertheless have been interested in the possibility that annual parallax could be or had been detected. However, their consideration of parallax as an explanation of their observations was cursory, and their attitude seems understandably to have been one of extreme caution. After recognition in both England and France during the 1670s that the practical difficulties were virtually impossible to solve, such an attitude was completely justified. In many ways, Hooke's 1674 publication may be seen as marking the end of an initial phase of enthusiasm concerning the detection of parallax among astronomers in the late seventeenth century. More than anything else, the book demonstrated clearly the overwhelming nature of the instrumental problems and the vast amount of work required merely to produce a small number of dubious observations. Astronomers had other, more rewarding projects upon which to spend their time.

However, the theoretical problems associated with the detection of parallax received some attention towards the end of the century. Two papers discussing the question of parallax were published in the Philosophical Transactions during the 1690s, but neither was concerned at all with the practicalities of detecting the phenomenon. The first, and more important, entitled "Concerning the parallax of the fixed stars, in reference to the Earth's annual orbit", was written in the form of a letter addressed to the Royal Society by John Wallis, Savilian Professor of Geometry (38). In the letter Wallis explored some of the possibilities of Galileo's idea of observing pairs of stars. He pointed out (and was the first person to do this) two particular stars lying very close together which might be suitable: the star at "the shoulder of the lesser bear" (39) [β Ursae Minoris] and the very faint star Alcor [80 Ursae Majoris]. In addition, Wallis provided a new selection criterion: he explained that to avoid refraction it would be profitable to observe circumpolar stars at their utmost

east and west azimuths as this measurement "will not be obnoxious to the refraction, as the meridional altitude is" (40). However, Wallis's idea was never tried either by himself or anyone who may have had the necessary equipment (41). Although he did make certain suggestions about how the observations could be gathered, they involved the use of:

"... some tower, steeple, or other high edifice, in a convenient situation, a good telescopic object-glass, in such a position as may be proper for viewing that star; and at a due distance from it, near the ground build some little stone wall, or such like place, on which to fix the eye-glass so as to answer that object glass... " (42)

with absolutely no notions as to how any of this might be accomplished! It is perhaps not surprising therefore that his theory was never put into practice.

The following year, a second paper concerned with the evaluation of stellar distances appeared in the Philosophical Transactions (43). It was written by Francis Roberts, a Fellow of the Royal Society (44), and really concerned the more general question of the estimation of stellar distances, rather than the specific problem of the detection of parallax. However, Roberts remarked that the measurement of parallax gave a more precise value for stellar distances than any other method could, but that the diameter of the Earth's orbit might not be large enough to subtend a discernible angle even at the nearest stars. But he offered no insights into the problems of stellar parallax detection, and his paper appears not to have had any impact on his contemporaries.

On the other hand, John Wallis figured again in the history of attempts to measure parallax at the very end of the seventeenth century, for it was to him that the Astronomer Royal, John Flamsteed, wrote in December 1698, claiming to have measured an annual parallax of 40" for the Pole Star (45). The observations which Flamsteed believed demonstrated the existence of parallax had not been gathered expressly for that

purpose; they were among the vast number of observations made by Flamsteed between the years 1689 and 1695 with his mural arc. He scrutinised those of Polaris after receiving a request from Newton for a new evaluation of the altitude of the north celestial pole which Newton required for his lunar theory (46). Once he had studied the data and concluded that they exhibited parallax, Flamsteed then decided to observe the Pole Star specifically with parallax in mind, but even these subsequent observations cannot be considered as an attempt to measure parallax in the way that Hooke's observations of γ Draconis were; Flamsteed's later observations of the Pole Star were made to test the theory based on his earlier ones.

Flamsteed's claims provoked several of his contemporaries into a reaction of one sort or another. Broadly speaking, the claims were accepted as genuine by certain astronomers in this country, but were criticised on justifiable theoretical grounds by continental astronomers in general and by Jacques Cassini in particular (47). Flamsteed was eventually forced to retract, which he did in November 1702 in a private letter to Sir Christopher Wren (48). The mathematical details of the dispute between Flamsteed and Cassini are discussed in Appendix I, together with various responses to Flamsteed's original letter made by other astronomers. In addition to that aspect of the episode it is important to note the complete absence of attempts by any of Flamsteed's contemporaries either to confirm or to refute observationally the claims made by the Astronomer Royal. Several of Flamsteed's continental contemporaries must have had access to good quality observations and of course Flamsteed himself had made systematic observations of the whole heavens, yet there is no evidence that even he analysed the variation with time of the positions of any other stars. When he wrote the letter to Wren acknowledging that Cassini was right he outlined certain intentions, saying:

"I shall return to my stock of night observations, to seek out such as are most proper for discovering the Errors of the Instrument; afterward those that are most convenient for showing the parallaxes of the Orb". (49)

However, there are no subsequent references to show that he ever carried out his plan. From James Bradley's time onwards it has been recognised that the effect Flamsteed had detected was that of the aberration of starlight. Hence it is impossible that he could, in fact, have found any 'error of the instrument'; but there is no evidence that Flamsteed returned at all to the problem of parallax determination. During the final years of his life he observed less often than in earlier years because of illness and age, and any observing he did was to be used exclusively for his new catalogue (50). Moreover, during these last years Flamsteed's life was dominated by the long dispute with, among others, Newton, Wren and Halley over the publication of his observations, and the whole idea of the detection of parallax seems to have been forgotten (51).

The fact that none of Flamsteed's contemporaries appear to have tried to find evidence for parallax can perhaps be explained by considering the extent of their interest in the phenomenon, and the reasons for such interest. In Paris, Cassini reacted quickly to Flamsteed's letter and showed complete understanding of the geometry involved, but his reply went no further than to say that Flamsteed could not have detected parallax (52); it offered no practical insights. But is there any reason that it should have? Astronomers in Europe had turned away from the problem of parallax detection because of the practical difficulties involved. There was no need, merely because the British Astronomer Royal believed he had unearthed observations which incidentally indicated the existence of parallax, to devote their time to the problem, especially once the theory provided by Flamsteed was shown to be wrong (53).

In England, it is likely that those who accepted Flamsteed's claims would see little point in trying to confirm them. The best instruments of the day were known to be at Greenwich, and presumably only the best instruments could demonstrate the small effect; anyone else would therefore be wasting his time using inferior telescopes to detect the phenomenon. However, Flamsteed's paper and Cassini's reaction to it brought Sir Christopher Wren's attention back to the problem of annual parallax. We have seen that in November 1702 Flamsteed wrote to Wren to defend his ideas concerning the parallax of the Pole Star against Cassini's attack. That Flamsteed's letter was the direct cause of Wren's revived interest in the phenomenon is unlikely (in fact it is more likely that Flamsteed was writing in reply to a query from Wren), but at about the same time as he received Flamsteed's letter Wren was thinking again about the problems of the support for a telescope of very long focal length. In 1692 Huygens had presented the Royal Society with the lenses of a 123 foot telescope (54), and in 1703/4 Wren devised a plan to use "the hollow in the great staircase on the fourth side" of Saint Paul's Cathedral to house Huygens's telescope (55). Unfortunately, the plan came to nothing as the staircase is only 96 feet 10 inches high! After this interlude, there is no evidence to suggest that Wren ever returned to the problem of parallax.

The reactions of other British astronomers to Flamsteed's paper are discussed in Appendix I. To summarise the arguments given there it can be said that Flamsteed's colleagues in Oxford, John Caswell and John Wallis, and the natural theologian William Whiston all accepted the Astronomer Royal's claims; on the other hand James Gregory's nephew David Gregory definitely did not, and Sir Christopher Wren clearly had doubts. However, although the publication of Flamsteed's paper and Cassini's response brought the issue of annual stellar parallax back to the attention of several astronomers, these publications were not indicative of any general enthusiasm

for projects to do with parallax, in the way that Hooke's Cutlerian Lecture had been. It was to be more than 10 years into the eighteenth century before an astronomer set out again with the specific intention of measuring parallax.

2.4 Ole Roemer

Before leaving the history of attempts to measure parallax during the seventeenth century, some account must be given of the relevant work of the Danish astronomer Ole Roemer. Assessing any of Roemer's contributions to the history of astronomy is difficult, as so few of his papers survived the fire which, in 1728, swept through the observatory in Copenhagen, where most of his written material and his instruments had been gathered together by Peter Horrebow, Roemer's successor as Professor of Astronomy at the university in Copenhagen (56). However, owing to a quirk of fate, one of the few fragments which was not destroyed was a very short piece of work, unpublished during Roemer's lifetime, entitled "The movement of the Earth, or the parallax of the annual orbit from observations of Sirius and Lyra, 1692-93" (57).

In this extract, Roemer had analysed the way in which the difference in right ascension between Sirius and Vega (α Lyrae) varied with time - a method which was reintroduced to help to solve the problems of parallax by F. W. Bessel over a century later (58). Because all that remains of Roemer's work on parallax is one small fragment, it is impossible to say whether he made the observations of the right ascensions of Sirius and Vega in the hope of detecting stellar parallax (59). Roemer did not comment on this, although the analysis of the data must have been undertaken with the specific aim of identifying a parallactic effect; Roemer would not have been comparing differences of right ascension between these two stars as a matter of course. However, in his conclusion he wrote that despite having detected a

variation in the difference between the right ascensions of these two stars he was convinced that it could not have been caused either by refraction or by the motion of the Earth, but by some nutation of the Earth's axis, the theory of which he hoped to work out at some later date (60). And that was all Roemer ever said about annual parallax. Perhaps anything else he wrote was destroyed in the fire; however, according to Horrebow, when the fire first broke out in the observatory he was engaged in writing a commentary on Roemer's fragment. Presumably, therefore, Horrebow would have had any other extracts concerning parallax and known to him close to hand; they too should thus have been saved, or at least referred to in Horrebow's subsequent account. The only remaining possibility is that Roemer did make other references to parallax but they were destroyed before Horrebow had the chance to study them.

Despite the lack of evidence, the Danish historian K. M. Pedersen has suggested that it was Roemer's determination to measure parallax which caused his obsession with continually increasing the accuracy of his instruments (61). In a paper published in 1976, Pedersen justified his claim in the following way:

"Why was Roemer so interested in improving his instruments? Was it just because he was a very skilled instrument maker? I think that a better explanation is that to find a stellar parallax he had to eliminate the annual variation that had already been discovered, because it would conceal the parallactic variations. The only explanation Roemer could think of was that these annual variations were caused by periodic errors of his instruments". (62)

Then in a later paper (63) Pedersen extended his view to suggest that it was Roemer's wish to measure parallax which led to the development of the Fahrenheit thermometer. Roemer certainly was interested in thermometry as he was well aware of the effect that temperature would have on the different materials of which his instruments were made, and he and Fahrenheit met and discussed such things in 1708 (64). However, Pedersen's basic premise that

Roemer's inspiration derived ultimately from his wish to measure parallax is unsubstantiated. He is justified in saying that to suggest that Roemer's interest in improving his instruments resulted merely from his skill as a craftsman is an insufficient explanation, but why select stellar parallax from all Roemer's astronomical interests as his sole motivation? It is not even clear that stellar parallax was one of his principal concerns. He had obviously considered the question in 1692-3, and did so again briefly in the second half of 1701 when he wrote to Flamsteed in response to the latter's claims about the parallax of the Pole Star. But in this letter, after thanking Flamsteed profusely for bringing the subject to his attention, he had to say that he neither had time to write a proper reply,

"nor could the subtlety of the argument put forward by [Flamsteed]... be properly appreciated and understood on a casual reading by a mind considerably occupied with other things". (65)

So although Roemer would have undoubtedly been greatly interested to hear of a genuine detection of stellar parallax, in 1701 at least it was not of particular concern. Also the subject does not occur at all in one of the only other sources of material on Roemer's life: the Adversaria, which consists of several manuscripts written on a variety of different astronomical topics (among other things), believed to date mainly from the year 1708 [see note 61].

A more likely explanation of Roemer's continued desire to improve the accuracy of his astronomical instruments is that he was well aware of the need for increased accuracy in practical astronomy as a whole. He had clearly shown interest in obtaining accurate observations of the solar system as early in his career as 1675 when he first suggested that light travels with a finite speed and calculated a value for that speed (66). In order to confirm that theory and to obtain a more accurate value for the speed of light he would certainly need to use the most accurate observations he could find

or make. There are also hints in the Adversaria that Roemer believed that stars might have motions of their own; if these were ever to be detected the more accurate the observations he made, the easier it would be for later generations to use them for the purposes of comparison (67). The detection of stellar parallax could clearly have had a place in Roemer's thoughts on improving the accuracy of his instruments, but not necessarily the most important place. He appears rather to have been inspired by a realisation of the need for improved accuracy in all fields of astronomy and his thoughts were manifested in new designs and constructions of scales, telescope tubes and telescope mounts.

Concern about the measurement of parallax during the last decade of the seventeenth century and the first decade of the eighteenth was therefore not very different from that during the years following the publication of Hooke's An attempt to prove.... Astronomers were prepared to listen to and to consider their colleagues' occasional references to it but in general they did not wish to set up any specific experiment - crucial or otherwise - to detect the phenomenon.

2.5 Jacques Cassini and Edmond Halley

The situation with respect to the detection of parallax did not change until 1714, when Jacques Cassini set out to determine the parallax of Sirius, the brightest star in the heavens. Why Cassini decided to try to measure parallax at this particular time is unclear. From his own account, in a letter addressed to the Académie royale des Sciences in 1717 (68), it is apparent that he had developed an interest in the fixed stars as individual objects and was determined to find out whatever he could about them, including therefore their true size and their distance from the Earth. He was particularly interested in calculating their actual size and wished to know their distance as a means of doing this. By expressing a wish to know

the sizes of stars Cassini was implicitly rejecting the assumption of uniformity among the stars, an assumption which had been made by a number of astronomers during the late seventeenth century. The assumption was made principally because of the interesting conclusions which could be drawn concerning the distributions and distances of the stars if they were all held to be intrinsically similar to the Sun, so that their brightness therefore differed only because of their different distances from the Earth (69). Further on in the paper Cassini spoke out explicitly against the assumption. He pointed out that if it were used to calculate the distance of Sirius the result obtained was 3.84×10^6 Earth semi-diameters; but at such a distance Sirius would exhibit an easily detectable parallax (approximately $20''$), so Cassini had to reject the assumption that Sirius and the Sun were basically similar objects.

Cassini was left therefore with the problem of finding stellar distances by a non-photometric method, and the only means available to him was to measure parallax. He agreed that all previous attempts to detect the phenomenon had failed, but it was as though the passage of time had renewed his hope that parallax might in fact be measurable. Also, it must be remembered that Jacques Cassini himself had never actually tried to make any such measurements. He would not have had the direct experience of the problems that his father J. D. Cassini had had to face with the construction of the well-shaft, and could perhaps have thought them exaggerated. In the hope that parallax might at last be measured, therefore, he set up his apparatus expressly to study the annual movements of Sirius.

The selection of Sirius marked a definite change in astronomers' approach to the problem of the choice of which star to observe in the hope of obtaining its parallax. In Hooke's time the predominant difficulty was perceived to be atmospheric refraction; hence the

use made of the zenith sector and the selection of γ Draconis. However, this approach had manifestly not been successful and Cassini obviously gave careful consideration to the choice of which other stars to study. He provided four reasons for his final selection of Sirius: being clear and bright it would be easy to observe every day; its brightness might indicate nearness and hence a measurable parallax; it lies close to the solstitial colure, so its celestial latitude varies with time in a similar way to its declination (70); and finally the annual change in the declination of Sirius as a result of precession is only about $2\frac{1}{2}''$ (71), against which any variation caused by parallax might be easily recognised.

Cassini followed his reasons for the selection of Sirius with a description of the arrangement of his telescope, which was in fact similar to that used by Flamsteed except that Cassini attached his to a quadrant rather than to a 140° mural arc. He set his telescope in position in such a way that as Sirius crossed the meridian on a particular date it was seen to pass through the common point of intersection of four cross-wires placed in the focal plane. The telescope was then fixed securely in that position, after which Sirius was to be observed through the telescope as often as possible and a note made of its position relative to the cross-wires as it transited. Cassini continued to observe Sirius at transit until late June 1715, and claimed that in the period considered its distance from the horizontal wire followed a fixed pattern, oscillating about it, its greatest distance from the wire never exceeding the apparent diameter of the star, which Cassini estimated to be about 5 or 6".

Cassini also included in his 1717 paper a discussion of the theory of parallax and, unlike Flamsteed's 1698 theory, Cassini's explanation of how parallax is manifested was sound if cumbersome. In his letter to Christopher Wren in 1702 Flamsteed had in fact given

a very clear and correct account of the theory of parallax (72), but that letter was not published until 1750. So in the early eighteenth century the only published accounts of the geometry of parallax were in Cassini's letters of 1702 and 1717. The second letter was in fact the earliest published account of parallax to deal with the practical problems, to give a correct theoretical explanation of the phenomenon, and to offer a set of data comprising several observations gathered over a period of well over a year. For this reason it was a significant contribution to the history of attempted stellar parallax determinations, but it is one which has been neglected almost completely by historians (73).

The paper was not ignored totally at the time of its publication. It certainly provoked Edmond Halley into considering the problems of measuring parallax, and he replied to Cassini's claims in a paper published in the Philosophical Transactions for 1720-1 (74). In his reply Halley pointed out the main problem with Cassini's work: although the Frenchman had discussed how annual variations in refraction would alter the apparent position of Sirius at transit, he did not actually allow for refraction in his calculations. This could affect the position of Sirius by as much as 8 or 10", and thus the whole endeavour had been ruined. Cassini took no barometric readings, which would be essential for evaluating refraction, particularly at the transit altitude of Sirius (less than 25° from Paris).

Halley objected also to Cassini's estimated value of the apparent diameter of Sirius, calling it an "optick fallacy" (75). He argued that in an occultation by the Moon of the stars Aldebaran and Spica Virginis, both disappeared instantaneously as the Moon passed in front of them. They both appear to be smaller than Sirius, but they are not, as Halley said, less than half its size; their apparent diameters cannot therefore be less than $\sqrt{2}$ times that of Sirius. Giving an apparent diameter of

5 or 6" for Sirius meant that those of Aldebaran and Spica must be at least 4", in which case they would not disappear instantly when occulted. According to Halley, the 'optick fallacy' was caused by Cassini's stopping down the object glass too much, which Cassini did in an attempt to eliminate the scintillating rays surrounding Sirius when viewed normally through his telescope.

Despite his criticism of Cassini's work, Halley clearly did not consider the project to have been completely worthless; at the end of his own paper he suggested that Cassini should try again, but that he should observe Vega instead of Sirius. Vega is bright, it lies near the solstitial colure, it is 28° from the north ecliptic pole and it thus would exhibit a relatively high proportion of any parallax effect in declination, and finally - of the greatest importance - it crosses the meridian at Paris at an altitude of about 80° , thus almost eliminating refraction considerations. Although Halley suggested that Cassini should try again, there was no mention in his paper of anything similar being undertaken, or even contemplated, by astronomers in this country. In 1720, on the death of Flamsteed, Halley had succeeded him as Astronomer Royal at Greenwich, but Halley had been left in the awkward position of having no instruments to use. Flamsteed's widow had taken them with her because they had mostly been paid for with Flamsteed's own money. Unfortunately for Halley this meant that he had to spend a good deal of his first few years at Greenwich re-equipping the observatory. It is hardly surprising therefore that he was unable to discuss any of his own work being done at the time when he was writing his reply to Cassini.

It was not until 1725 that the problem of parallax was again considered in depth in Britain. But in that year the Irish astronomer Samuel Molyneux decided to repeat Hooke's 1669 experiment on the measurement of the parallax of γ Draconis. The story of how Molyneux invited Bradley to help him with his investigation and

Bradley's subsequent discovery of the aberration of starlight may be read in any of the standard histories of astronomy and need not be repeated here (76). However, there are two points significant for the history of attempts to measure parallax and not discussed in the standard accounts. The first is that Bradley was the first astronomer to consider testing whether the phenomenon he himself had observed in one star was apparent in any others. After observing γ Draconis with Molyneux's telescope at Kew, Bradley wished to have a telescope built in his own home at Wansted, and of all the reasons he gave for this he claimed that:

"... the chief [reason] of all was the opportunity [he] should thereby have of trying in what manner other stars were affected by the same cause, whatever it was". (77)

Of course, with the arrangement he had Hooke would not, without much trouble, have been able to check for parallax in other stars, but there is no evidence that he even contemplated such a step. In the cases of Flamsteed and Jacques Cassini, there is no obvious reason why the former should not have analysed any other of his observations, or why the latter should not have repeated his experiment with certain other stars. In fact, it would have been easier for either of them to check their work than it was for Bradley to repeat his observations as the instrument he used at first (Molyneux's) was modelled on, and very similar in its extent to, Hooke's.

The other important point about Bradley's account of his discovery of the aberration of light is the insight it gives concerning Bradley's attitude to his instruments. It is clear that Bradley considered himself to be very much beholden to his instrument maker George Graham. Bradley spoke of his "exact and well-contrived instruments" claiming it was "chiefly owing to our curious member Mr George Graham" that he was, on their account, able to increase the exactness of Hooke's observations (78).

Improvements are clearly noticeable in the period between Hooke and Bradley, even in that between Flamsteed and Bradley. Flamsteed's most optimistic estimate for the accuracy of his mural arc was 5", whereas by the end of his work on aberration Bradley was claiming that he would have detected parallax even if it were as small as 1" (79). Bradley was also very much more confident about the performance of his telescope: when Flamsteed realised that the variation he had noted in the position of the Pole Star was not due to parallax, he immediately assumed an instrumental cause; Bradley on the other hand had the confidence to say that he had tested his telescope thoroughly and that he was certain that something else was responsible for the observations he had made.

By the late 1720s, therefore, a sophisticated level of understanding of the phenomenon of parallax had been attained. Astronomers appreciated how the effect should be manifested, and realised that it was actually very much smaller - and hence that the dimensions of the universe were very much larger - than had been suspected even by astronomers in the late seventeenth century. Of course they were not to know that it would be another century or more before 'successful' measurements were to be obtained.

2.6 Conclusions

The history of attempts to measure stellar parallax is generally divided into two major phases: before and after the discovery by Bradley of the aberration of light. The choice of Bradley's 1728 paper as the divide is not arbitrary but, as will eventually be shown, it is artificial. At this point, however, it is more important to concentrate on the earlier period. If any of the standard chronological accounts of attempted parallax measurements is consulted, the story which is told of events during the final third of the seventeenth century is one of great activity as each astronomer struggled to be hailed as the one who had managed to solve all the problems. This

activity apparently continued until Bradley's work in the 1720s, after which astronomers despaired of being able to detect the phenomenon for over 70 years.

However, from a closer analysis of the work of the astronomers involved, a different, less clear-cut picture emerges. To begin with, the pre-Bradley period can be divided into three shorter phases beginning with the time during the 1660s, and possibly even earlier, when ideas about the building of suitable instruments and the choice of star were being exchanged between Hooke, Wren, John Wallis and possibly James Gregory. In addition to the evidence given above (section 2.2), we know from a letter to Oldenburg in 1674, in which Wallis praised Hooke's work, that Wallis had given some thought to the question of parallax about 26 years previously, when it is quite possible that he was in close contact with both Hooke and Wren (80). In Paris during the 1660s and 1670s similar possibilities were being explored - although there is no evidence of any exchange of ideas between England and France until after Hooke's publication. The culmination of this initial phase of interest came with the delivery and publication of Hooke's lecture: it provoked a certain amount of discussion among his contemporaries, but it did not convince them of anything except the difficulty of the undertaking.

During the second phase, the years following the publication of Hooke's work, active interest in parallax was rare indeed. Wallis published his thoughts in 1693, but that paper contained ideas which had been maturing for many years and it had no impact on astronomers, except perhaps in influencing Flamsteed's decision in the late 1690s to send his "measurement" of parallax to Wallis. And even Flamsteed's 1698 letter, which created a minor stir in the astronomical world, did not result in any great activity as far as actual attempts to measure parallax were concerned. No flurry of activity transpired until Jacques Cassini's work of the mid-1710s, marking the beginning of the third phase in the 'pre-Bradley' era, which included Cassini's work, Halley's response, and the

interest of Molyneux and Bradley.

However, the remarkable feature of this period, in the context of the history of astronomy rather than that of the history of attempts to measure parallax, is the very small proportion of time devoted to parallax detection by any of the figures involved. Seeking parallax was not a major part of anyone's astronomical endeavours. None of them is especially famous for his contributions to the development of this enquiry. It was after all not so very important an enquiry. Annual stellar parallax was a problem which many astronomers reviewed from time to time, but only occasionally did anyone spend much time on it. It did not affect any astronomer's commitment to the Copernican theory, despite Hooke's claim to the contrary; absence of parallax could always be explained by Copernicus's own reasoning that the stars are too far away for the phenomenon to be detected (81). Measurement of parallax did not necessarily prove anything. Astronomers all had many other studies to occupy them, most of which were far more profitable in terms of results than parallax detection. If the contents pages of either the Philosophical Transactions or the Mémoires de l'Académie royale des Sciences are consulted it will be seen that there are any number of papers on meteors, the satellites of Jupiter and Saturn, Sun spots, lunar observations and comets; anything in fact to do with the solar system. This is not surprising, particularly after the publication of Newton's Principia (82), when astronomers were all very anxious to test the theory of gravitation within the solar system. Newton himself was eager for new observations of the Moon to help him with his lunar theory (83). Observations of the stars were made only for the compilation of catalogues, the main reason for which was to enable astronomers to plot the positions of objects in the solar system against the background of the stars. The 'fixed' stars were still well and truly fixed at the beginning of the eighteenth century, and hence uninteresting in themselves. There was no such thing as 'stellar astronomy'

as we know it now: interest in the stars as individual entities developed slowly and intermittently during the eighteenth century, and in the period under consideration in this chapter it was hardly even in its infancy.

Chapter Three

JAMES BRADLEY AND THE EIGHTEENTH CENTURY 'GAP'

3.1 Introduction

When, in December 1728, James Bradley wrote to Edmond Halley of his "new discovered motion in the fixed stars" (1) Bradley pointed out an important implication of his work for the problem of the detection of the annual parallax of the stars. In conclusion he wrote thus:

"I believe I may venture to say, that in either of the two stars last mentioned [the annual parallax] does not amount to 2". I am of opinion, that if it were 1" I should have perceived it in the great number of observations that I made, especially upon γ Draconis; which agreeing with the hypothesis ... nearly as well when the sun was in conjunction with, as in opposition to, this star, it seems very probable, that the parallax of it is not so great as one single second". (2)

This statement has been cited by historians as a decisive turning point in the history of attempts to measure parallax: a turning point in the wrong direction, however, as after this, it is claimed, astronomers no longer sought parallax measurements, believing such small angles to be beyond the limits of even the most sensitive instruments. According to Michael Hoskin:

"Not surprisingly, Bradley's revelation of the most incredible delicacy of the required measurements ... and the apparent near impossibility of maintaining such accuracy over an annual cycle, resulted in a failure of nerve among those few astronomers who possessed instruments capable of precision measurements". (3)

And the Canadian astronomer J. D. Fernie, in his paper "The historical search for stellar parallax", suggests that

"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". (4)

After making their point about Bradley's paper on aberration, both authors start their accounts of new interest in the problem of parallax shown with the work of the Italian astronomer Guiseppe Piazzi during the early 1800s. Thus the overall picture portrayed is essentially one of a suspension of activity between 1728 and 1802. These authors offer no discussion of whether astronomers' "failure of nerve" resulted in the question's being abandoned altogether or whether the problems were still considered, but no practical solutions were obtained; nor is it altogether clear whether the solution of the problem was merely a matter of waiting for the necessary improvements in instrumentation, although this is what is implied. Also the question of why Piazzi should have returned to the problem when he did is not discussed. It is these questions, amongst others, which must be addressed and answered in this chapter, in an attempt to fill the 'gap' in current knowledge of eighteenth century stellar parallax measurements.

From the chronological surveys of attempts to measure parallax, compiled in the early nineteenth century by the German astronomer G. R. Fockens (5), and later by his fellow-countryman C. A. F. Peters (6), it is clear that attempts were made during the period 1730-1800 to measure parallax, and that papers were written on the theoretical aspects of the subject. Peters referred to work by Eustachio Manfredi in the 1730s (7), and by Roger Long in the 1740s (8); he spoke of observations made by the Abbé de Lacaille during the 1750s, discussed in the following decade by Jérôme le Français de Lalande (9) and by Nevil Maskelyne in the latter's account of the possibility of detecting the parallax of Sirius (10).

Finally, Peters gave an account of the most important paper of the period: that by William Herschel published in the Philosophical Transactions for 1782 (11). In addition to the papers discussed by Peters, Fockens's dissertation referred to material by Clairaut written in 1739 (12), and to the paper by John Michell published in 1767, in which Michell applied simple probabalistic arguments to an analysis of the distribution of the stars (13). It should also be remembered that Peter Horrebow's book Basis astronomiae (14), containing Roemer's short paper on parallax was published seven years after Bradley's paper on aberration, and nowhere in it does Horrebow retract his 1727 thesis that parallax may be detected from observations made by Roemer earlier in the century (15).

The above list shows that there is no truth in the belief that the problem of measuring parallax was merely forgotten for seventy years. Perhaps half a dozen or so publications in such a timespan is not many, but in the sixty years previous to Bradley's paper the number was comparable. From those papers written before Bradley's it is apparent that overall interest in parallax in the period from Hooke's work in the 1660s to Bradley's in the 1720s was less than that suggested in the secondary literature; it will now be shown that there was more post-Bradley interest than might be supposed from a knowledge of the secondary sources, giving a more uniform picture of eighteenth century concern in the problem of the detection of annual stellar parallax.

3.2 Immediate reaction to Bradley's paper

Bradley's paper on aberration was read to the Royal Society in early January 1729, and was published in the next issue of the Philosophical Transactions. The paper seems not to have caused much reaction - either negative or positive - at the time of its publication, but it has since been hailed as the paper which established his

"character" as an astronomer (16). The significant question for the history of attempts to measure parallax does not, however, concern Bradley's subsequent work; rather, it concerns the reaction of his contemporaries to the claim that parallax must be less than 1" since Bradley had been unable to detect it. To evaluate that reaction, the important people to consider - in fact the only people to consider - are those who, at about the time of Bradley's publication or soon after, addressed themselves to the issues of parallax, or those who commented specifically on any limiting effect of the paper. The number of astronomers who broached the question of parallax around the same time as Bradley is possibly as small as three: Eustachio Manfredi working in Bologna, Peter Horrebow in Copenhagen and Joseph Delisle in Paris. This number should not surprise us, since in the whole history of interest in the measurement of annual stellar parallax there were never more than a very few astronomers who were concerned at any one time: in the 1720s and early 1730s there was Bradley in Britain and Manfredi, Horrebow and Delisle on the Continent. It is the impact of Bradley's paper on the other three which must therefore be investigated.

Eustachio Manfredi (1674-1739) was appointed professor of Astronomy at Bologna in 1711 and soon after he began making observations of stellar positions from the observatory there (where J. D. Cassini had started his career). In 1715 he published two volumes of ephemerides based on his own four years of observations, together with observations made some years earlier (about 1704-5) by Jacques-Philippe Maraldi, who had been working in Paris since 1687 when he was summoned to the Observatoire by his uncle, Cassini I (17). While compiling the volumes (presumably, though not certainly) Manfredi became aware of an annual variation in the position of certain stars. He did not publish anything concerning this until 1729, but in the account he

eventually produced he claimed that he had already established by 1719 that what he had observed was not a parallactic effect. He provided no theory to explain his observations however, and although the publication date is later than that of the appearance of Bradley's aberration paper, it is clear that at the time of writing Manfredi had not heard of Bradley's work. But two years later when Manfredi published a paper intended as a supplement to his book, he discussed further observations made at Bologna, in the context of the problems of parallax and in the light of Bradley's new theory of aberration.

Both the 1729 and the 1731 publications are important in the history of attempts to measure parallax. The 1729 book was significant in its own right, as it provided for the first time in print a detailed geometrical analysis of the problem of parallax (18). But the principal importance of the works lies in the fact that Manfredi was writing at precisely the same time as Bradley. By the time the second piece was written Manfredi had read Bradley, and he acknowledged that what he had observed could essentially be explained by the theory of the aberration of light. He supplied in addition new observations of the difference in right ascension between some of the brightest stars: Sirius and Vega, Capella and Arcturus amongst others. What Manfredi did not comment on was whether or not he felt it was impossible, in the light of Bradley's remarks, ever to measure parallax. It is far from clear, in fact, that Manfredi actually accepted Bradley's theory unreservedly. He did write that his observations could be explained by the theory provided by Bradley, but he was not completely happy with such an explanation because it involved accepting a finite velocity for light. That Manfredi was ever willing to accept this is uncertain, and so therefore is the question of Manfredi's acceptance of the theory of aberration (19). However, he was unable himself to provide an alternative explanation to

aberration for the effect he had observed, and after his 1731 publication he appears to have been completely silent on the subject.

Manfredi's work was nevertheless discussed in the correspondence between Peter Horrebow and the Dutch astronomer and theologian Johan Lulof, who lectured on ethics and astronomy at the University in Leyden, and who in 1740-1 worked on a translation into Dutch of Horrebow's Copernicus Triumphans. It was in this book that Horrebow had claimed that certain of Roemer's observations showed the existence of annual stellar parallax and thus that Copernicus was vindicated (20). Lulof was not convinced, and between November 1740 and March 1741 he wrote to Horrebow three times (21) pointing out that Manfredi's conclusions differed completely from Horrebow's: Manfredi was certain that the annual variation he himself had detected was not due to parallax. But Horrebow was not unduly worried; when he replied to the first of Lulof's letters he argued that there were inconsistencies in the observations used by Manfredi, and that as far as he, Horrebow, was concerned, Copernicus was still triumphant. The significant point with respect to the impact of Bradley's paper on aberration is that neither Horrebow nor Lulof mentioned it. They had both read (or claimed to have read) Manfredi's 1731 paper so they should both at least have heard of Bradley's work and known of its implications for the status of the Copernican theory. However both chose to ignore it: Lulof perhaps because he seems to have been very impressed with Manfredi's work. But it is difficult to understand why Horrebow paid no attention to Bradley's theory: it was not mentioned at all in his 1735 book, Basis astronomiae. Being so ardent an admirer of Roemer and his methods, Horrebow should have had no qualms over the acceptance of a finite speed for light (22), and he clearly believed that the Earth moves round the Sun. Perhaps he could not read Bradley's original paper because it

was written in English; it is at least obvious that he was not influenced in any way by its contents, and Bradley's imposed limits on the possible size of parallax did not deter Horrebow from maintaining that Roemer's observations demonstrated the phenomenon.

The only other astronomer to show interest during the late 1720s in the detection of parallax was the Frenchman Joseph Delisle, to whom Horrebow sent a copy of his Copernicus triumphans in 1728. In July 1728 Delisle wrote to Horrebow acknowledging receipt of the book (and also a copy of Horrebow's Clavis astronomiae) and giving his opinion of it (23). He was pleased by the novelty of the method Horrebow used to demonstrate parallax - comparing the difference in right ascension between two stars at different times of the year - but pointed out that this could not give a definite value for the parallax of one particular star. Delisle was clearly impressed with the accuracy of Roemer's observations, but claimed that they could not easily be checked in Paris as the French astronomers lacked comparable instruments. But the most significant point made by Delisle about his fellow astronomers was that:

"Since the beginnings of the Academy of Sciences in Paris MM Picard, de la Hire and Cassini have diligently applied themselves to the observation of the differences in transit [times] between the fixed stars and the planets with very exact pendulum clocks; Mr Flamsteed has done the same thing in England. But as these astronomers had in sight in their observations only the theory of the planets, and the catalogues of the fixed stars, without thinking of determining the parallax of the annual orbit, it can hardly be hoped, despite the great number of their observations, to find among them many of use in the circumstances as dictated by the method of Mr. Roemer ... " (24)

This letter was written before the publication of Bradley's paper on aberration, and assuming that Delisle's observations of the endeavours of his contemporaries were based on fact (as the evidence of

their publications suggests) Bradley's paper could not have had the effect claimed for it on his immediate contemporaries. If astronomers were already uninterested in parallax, the most Bradley's pronouncements could have done was to confirm them in their views.

Perhaps however Bradley's paper should be regarded as a deterrent to later astronomers who might otherwise have been prepared to devote their time to the problems of parallax. But in the late 1730s there was certainly hope in the mind of the French astronomer Alexis Claude Clairaut that parallax was measurable, as can be seen from the title of his paper on the subject: "On the simplest way of discovering if the fixed stars have a parallax, and to determine it exactly". Although he acknowledged all the way through that there was a possibility that parallax was too small to be measured, he still believed an investigation to be worthwhile.

In his paper Clairaut gave his opinion of the claim made about parallax by Bradley 8 years previously. He wrote:

"As for the parallax, it appears that one can conclude, after the observations of Mr Bradley, that there is no sensible [effect] in any of them, since that astronomer did did not detect 1" when he had done the reduction necessary for aberration; however as the stars that he has observed are few in number, and since the instrument of which he made use had a measuring edge of only 12° , it seems to me that one is not right to conclude that no star has a parallax, as [in the way that] one **can** assert that all the stars are subject to the aberration of light ... " (25)

Undoubtedly, therefore, Clairaut had reservations about Bradley's statement; he believed astronomers should continue the search for parallax and gave an account of his ideas on the time of year at which the most useful observations could be made. In doing this he made use of Bradley's work on aberration; having explained correctly that the phenomena of aberration

and parallax are 90° out of phase (that is, in a specified direction/coordinate the size of the displacement resulting from aberration is maximum when that resulting from parallax is minimum) he pointed out that the obvious times to look for parallax were the times when the apparent displacement it caused was maximum and that caused by aberration was minimum.

Clairaut did not himself try to measure parallax. He was interested in the theory of astronomy more than in its practise, and like John Wallis in 1693 he offered no ideas about how the necessary observations might be gathered. Since his main argument was that parallax was not proven to be less than 1", he would presumably have expected his fellow astronomers to study the observations that they already possessed, particularly those made at the times of year he had suggested.

Moreover the period after Bradley's paper was not completely lacking in practical attempts to detect parallax either. Clairaut provided the first theoretical suggestions to avoid the predicament which faced Bradley, and at about the same time the initiative was taken on the practical side by Roger Long, then master of Pembroke Hall in Cambridge. Long was a keen astronomer who lectured for several years on the subject; his most important contribution to astronomy was his text book, Astronomy in five books, which was eventually published in two volumes in 1742 and 1785 (26). In the earlier volume Long devoted two chapters to a discussion of the parallax of all heavenly bodies; the second of these chapters concerned the parallax of the fixed stars in particular (Book II, ch. 18).

Long wished to try to detect parallax for two reasons: "this enquiry", he wrote,

"very well deserves our attention, upon two accounts; 1. if such a parallax could be discovered, it would demonstrate the truth

of the Copernican system; 2. if the quantity of the annual parallax of any of the fixt stars could be found, the distances of those stars from our earth might be known". (27)

He was naturally completely familiar with Bradley's work, and he discussed in detail the concept of aberration, coming to conclusions similar to those of Clairaut about the time of year at which to observe in the hope of detecting parallax. However, Long differed from Clairaut in addressing the problem of the selection of which stars to observe. Combining an idea he found in David Gregory's Elements of physical and geometrical astronomy with Galileo's and Wallis's thoughts on the method of double stars, he selected specific pairs of stars for his study. Gregory had claimed (28) that some stars appeared to be single at certain times of year but double at other times. Among the three possible reasons for this phenomenon, Long suggested that it might be a parallactic effect caused by the Earth's annual motion (29). He resolved therefore to observe the stars mentioned by Gregory, which he did through "two very good telescopes, one of 14 and another of 17 feet" (30).

Immediately after this Long wrote:

"I have not yet repeated my observations often enough to determine truly in the affair, but from those I have made I am persuaded they will be found to appear always the same without any sensible change of distance or situation; this will not seem strange after what has been said about the smallness of the annual parallax". (31)

Long wrote no subsequent account of having repeated his observations; presumably if any further observations were made they did not produce any evidence to change his conclusion that parallax was undetectable.

Long was unsuccessful therefore in his attempt to establish the truth of Copernicanism and to measure stellar distances. However, the important point about his work is that he actually tried to measure parallax

after reading Bradley and despite Bradley's pessimism. He recognised the validity of some of Bradley's claims - that the parallax of the stars Bradley had observed could not amount to 1" - but also perceived their limits. In addition he suggested a way of approaching the whole problem differently, by putting into practice the ideas of Galileo, James Gregory and John Wallis.

After Long's work in the 1730s and early 1740s, there appears to have been nothing written about parallax for nearly 20 years. If Bradley's work on aberration did result in a complete lack of further investigations into the problems of parallax during any period in the eighteenth century, it was during the time between Long's work and the subsequent reappraisal of the problem in the 1760s by Nevil Maskelyne in England and Jérôme le Français Lalande in France. This does not necessarily mean that the question was not considered, merely that no new ideas evolved. It was still very much the case that astronomers had many other interests: for example, in France, their main concern was for the major projects that were being organised to survey vast areas of Peru and Lapland (32); at Greenwich Halley was still engaged in the search for the solution of the longitude problem (33). There was no reason for any astronomer to pay particular attention to the problems of parallax, and indeed none did.

3.3 The mid-century: Maskelyne, Lalande and Michell

In the early 1760s the vexing problems of parallax were tackled once more, this time by Nevil Maskelyne in England and Jérôme Lalande in France. Both based their work on observations made at various times by the French observational astronomer the Abbé de Lacaille, who published in 1758, in his Fundamenta astronomiae, the observations he had made of Sirius from the Cape of Good Hope during 1751 and 1752 (34). The book was read by Maskelyne and it seems very likely that it was these

observations of Sirius which inspired him to think seriously about the possibility of measuring parallax. In his paper "A proposal for discovering the annual parallax of Sirius" in the Philosophical Transactions for 1760 he claimed to have been thinking of this for some two years. He made the point that just because Bradley could not detect a parallax for γ Draconis, that was no reason to suppose it would be undetectable in every other star: such a claim could not be made until every star, and particularly the brightest, had been observed closely. The star in which he was especially interested was Sirius, the brightest star in the heavens. From Lacaille's observations he calculated that its annual parallax could be as much as 8", a surprisingly high value for Maskelyne to consider likely in the light of Bradley's conclusion in 1728. The major advantage of Lacaille's observations was that, observed from the Cape, Sirius culminates at about $17\frac{1}{2}^{\circ}$ from the zenith and at such an altitude the effect of refraction is far less important than it is when Sirius is observed from this country. However, ideally the star should be observed from somewhere where it culminates exactly at the zenith. Maskelyne's suggestion, therefore, was that Sirius should be observed during the trip to St Helena planned by the Royal Society for the observation of the transit of Venus in 1761, for at the latitude of St Helena Sirius "passes only half a degree south of the zenith" (35). This suggestion was followed by a table showing the results of his analysis of Lacaille's observations, as a justification of his request.

The annual discrepancy noticed by Maskelyne was in fact illusory; the observations were subjected to a further examination a century later by Peters who concluded:

"It is thus to be assumed, that the parallax indicated by Maskelyne must be attributed mostly to the imperfection of the observations of Sirius, which were moreover very few in number". (36)

In the 1760s, however, the idea of observing Sirius from St Helena clearly impressed the Royal Society and it seems that they agreed to pay for a zenith sector to be built and taken by Maskelyne, who was to be in charge of the expedition. But the instrument which was taken specifically for the purpose of observing Sirius turned out to be inoperable when the party reached St Helena. As E. G. Forbes remarks in his book on the origins of the Greenwich observatory:

"... a defective zenith sector was thought to have been responsible for the inconclusive attempt by Maskelyne to determine the parallax of Sirius, which Lacaille's Cape observations had led him to anticipate would be detectable". (37)

So it seems an attempt was made at St Helena to measure parallax, but it was fruitless.

Near the beginning of his paper, Maskelyne significantly commented that

"The astronomical object, in favour of which I desire to engage your attention, is no less than the determination of the annual parallax of the Orbis Magnus; the finding out of which, from observation, would be the fullest and directest proof of the Copernican system". (38)

He admitted that failure to detect parallax did not prevent any astronomer from accepting the Copernican hypothesis, but claimed that:

"The actual demonstration of it, from observation, would be a direct and convincing proof of the truth of that system". (39)

How far this was intended as propaganda in order to win the approval and financial support of the Royal Society, and how far Maskelyne genuinely believed that a proof of Copernicanism was necessary is difficult to say, but the first quotation suggests that parallax was still in the thoughts of astronomers, reminding them intermittently that despite advances in many branches of astronomy, they were yet to overcome the difficulties of this particular enquiry.

The other person to use Lacaille's observations was his fellow countryman Jérôme le Français de Lalande. In the final volume of his three-volume work Astronomie he devoted eight pages to a discussion of stellar parallax despite beginning with the words:

"It has now been shown that the annual parallax is insensible and as nothing in the fixed stars". (40)

He gave a detailed account of the geometry of parallax - acknowledging the great contribution to this study made thirty years earlier by Manfredi - and following this he discussed previous attempts to measure the phenomenon made by, amongst others, Tycho, Picard, Hooke, Flamsteed and Jacques Cassini. He finished his brief "history" with some remarks about Lacaille's observations both from the Cape and from Paris. Of the observations used by Maskelyne he said:

"... but these observations of Sirius only go from the Summer of 1751 to the following Winter; and there could have been some local cause which had produced in these observations the differences of 8" ". (41)

After thus disposing of Lacaille's Cape observations, Lalande referred to a series of observations made at Paris between the summer of 1761 and early 1762, during which time Sirius appeared to have been displaced by a more realistic $2\frac{1}{2}"$; but this displacement could not be owing to parallax because it was in the wrong direction. Generally speaking, Lalande was not very impressed by the accuracy of his colleague's observations, an opinion borne out by Peters's analysis a century later (42).

To complete his section on parallax Lalande mentioned some observations made from Greenwich in the early 1760s, which John Bevis (an amateur astronomer with his own observatory at Stoke Newington, who had on several occasions worked in collaboration with Halley and Bradley) had allowed him to see in 1763. It is unclear who actually made the observations but they were reduced by Maskelyne. Again the observations revealed an apparent movement of Sirius but Lalande could justifiably not accept this as

evidence of parallax because he suspected that refraction had not been sufficiently allowed for - always a difficult thing to do. Hence he remained convinced that parallax was insensible. It is nevertheless important that he devoted a section of his book to a discussion of parallax, for the book was well known and widely read. In the second edition, in 1772, this section was retained and in the third edition, in 1793, it was extended to include Lalande's views on Herschel's 1781 paper concerning parallax. This shows that Lalande was at least still interested in the problem, even if he was unable to suggest new ways of solving it.

The possibility of detecting parallax was also of interest during the 1760s to the Reverend John Michell, whose paper "An enquiry into the probable parallax, and magnitude of the fixed stars ... " was published in the Philosophical Transactions for 1767. Michell was a very talented individual who, before he became rector at a church in Thornhill, Yorkshire, in 1767, lectured at Cambridge in Hebrew, Greek and Mathematics. He also wrote on the subjects of magnetism, earthquakes, matter theory, scientific instruments and astronomy. His most important contribution to the last of these was the 1767 paper on parallax and probability. It was entirely theoretical and, in fact, not as concerned with the detection of parallax as the title might suggest; Michell wished in fact to examine wider cosmological issues, in particular the distribution of the stars through space and their intrinsic properties, especially their sizes and their distances from the Earth.

He began with a discussion of a model of the universe based on the assumption that the stars are all intrinsically similar and that the Sun is one of the stars. From such an assumption certain astronomers had estimated particular stellar distances by comparing the brightness of the star in question with that of the Sun (43). Michell showed that the estimates obtained

photometrically agreed with the hypothesis that the stars were all too far away to exhibit a measurable parallax, but the main purpose of the paper was to undermine the validity of the uniformity assumption. His basic argument was probabilistic. The question he tried to answer was: what was the likelihood that the observed groupings of the stars resulted merely from the position of an observer placed randomly in the midst of thousands of intrinsically similar objects? Beginning with a consideration of the double star

β Capricorni he wrote:

"If we now compute ... what the probability is, that no stars, in the whole heavens, should have been within so small a distance of each other, as the two stars β Capricorni, ... we shall find it to be about 80 to 1". (44)

This was followed by an estimate of about 500,000 to 1 against for the chances that, from a random scattering of essentially similar stars, the six brightest among the Pleiades should lie within so small a distance of one another. Michell continued:

"And the natural conclusion from hence is, that it is highly probable in particular, and next to a certainty in general, that such double stars, etc. as appear to consist of two or more stars placed very near together, do really consist of stars placed near together". (45)

In other words Michell gave a mathematical argument which contradicted the very basis of the uniformity assumption. For if, as he insisted, pairs or groups of stars which appeared to lie close together actually were so situated, since so many pairs obviously consisted of two stars of very different brightness, the components must be intrinsically different. Michell's argument did not of course mean that no pairs of stars occurred merely as the result of a line of sight effect, but it did mean that many pairs did not, and given that, how were astronomers to know which did? Even if it were possible to discover which were optical doubles, without the basic assumption that brightness indicates distance

how could astronomers know that the actual distance between the stars was enough to show a relative parallax, or even which of the two was the nearer? This aspect of Michell's paper was to be of significance some fifteen years after its publication when William Herschel wrote his paper on the possibility of measuring stellar parallax (see below, section 3.4).

Michell's paper was important for a number of reasons, some of which have been commented on several times by historians. It has long been recognised as the earliest application of probabilistic arguments to astronomy (46), but it also brought to the attention of contemporary astronomers the possibility of using proper motions as a new criterion of nearness in the search for parallax. The study of proper motions increased through the eighteenth century and became very significant for the development of the problems of parallax during the early nineteenth century (see below, chs. 5 and 6). Michell wrote:

"Many of them [that is, the stars] also have been observed to have a proper motion of their own, which with several other concurrent circumstances tends to make it highly probable, that they are some of the nearest to us". (47)

He neither referred to any specific values of proper motion nor commented on the implication of such values for the assumption of uniformity, but he was one of the few eighteenth century astronomers to appreciate that high proper motion might indicate nearness [see below, note 82]. Michell also discussed the idea that the solar system formed part of a larger system of stars; although he was not the only individual to consider the composition of a local group of stars (48) his important contribution in this paper was to subject the information he had - and which was readily available to all his contemporaries - to a mathematical analysis.

But perhaps the most important point of all about Michell's paper was that it was among the first written

in English to consider the system of the stars to the exclusion of all else (49). Michell was interested in the stars as individuals: their sizes, their distances from the Earth and from one another, and their distribution and grouping in space. His interest in establishing parallax was subordinate to his greater ambition to understand the structure of the universe. Because of this, Michell was different from all his immediate contemporaries, who were still predominantly interested in the celestial mechanics of the solar system. Admittedly, astronomers at the main observatories were devoting much of their time to the accumulation of stellar positions, but their reason for doing so was to provide an accurate background chart against which to plot the movements of objects within the solar system. Occasionally individuals showed interest in the way in which certain stars seemed to vary in brightness and they offered possible reasons for this. In the main this type of work was carried out outside the major observatories by such people as John Goodricke and Nathaniel Pigott (50). But in general the bulk of astronomical work involved the study of the solar system, with a particular emphasis on the study of comets, eclipses and during the 1760s of course the transits of Venus.

Michell was not the first to undertake an investigation of the arrangement of the entire heavens: as far back as the 1710s Jacques Cassini's attempt to measure parallax had stemmed from his desire to try to understand the nature of the stars. But such interests were far from commonplace among astronomers during the first half of the eighteenth century, as is confirmed by a consideration of the subjects of the papers read before the Royal Society and the Academy of Sciences. Of course the most important deliberate campaign to establish a type of astronomy fundamentally different from that undertaken at the major astronomical centres in Europe did not start until the work of William Herschel (51), but that does not detract from Michell's achievements in his consideration

of the distribution of stars through space. Unlike Herschel who was a first rate observational astronomer as well as an innovative theoretician Michell was concerned mainly with theory. His paper was a significant contribution to the development both of stellar astronomy in general and the problem of parallax in particular.

Thus the mid-eighteenth century was not wholly lacking in interest in the problems of parallax, and it actually produced certain new ideas. Maskelyne's theory of observing Sirius from St Helena combined constructively the two oldest criteria for the selection of stars likely to have detectable parallaxes: the criteria of brightness and of high transit altitude. And although Michell's 1767 paper might at first appear to have made only a negative contribution, as it argued very conclusively against the possibility of using double stars as a means of detecting relative parallax, the paper did at least focus attention on the question of parallax, and possibly provoked astronomers into thinking about some of the wider implications both of the large scale structure of the universe and of the uses of probability arguments in astronomy.

3.4 William Herschel and the method of double stars

It was during the late 1770s that William Herschel began his life-long endeavour to understand the three dimensional construction of the heavens (52). Of fundamental importance to such an endeavour was a knowledge of stellar distances, and Herschel wished to be able to estimate the distances of all the stars. He realised that in order to do this he needed a workable model of the heavens and chose to make use of the assumption of uniformity among the stars because of the extent to which this would allow him to speculate; the model he used was simple yet revealing. If he could measure the distance to certain stars, he could then

calculate the distances of all others using the assumption of uniformity together with the inverse square law of photometry. Herschel resolved therefore to find stars which might have a detectable parallax; he gave the problem much thought and following in the tradition of Galileo, Wallis and Long, he concluded that the best way to proceed was to observe close pairs of stars. But Herschel considered the advantages of the use of such pairs far more thoroughly than any of his predecessors had done, for he was the first astronomer to explain just how many difficulties could be avoided by studying them. To appreciate this it is necessary to consider certain eighteenth century developments in positional astronomy (53).

The eighteenth century was certainly the period when the work of astronomers was dominated by their wish and need to know as accurately as possible the daily positions of celestial bodies. At Paris and Greenwich in particular vast quantities of data were gathered; the movements of the Sun, Moon, planets and their satellites, and comets were closely followed so that their paths through the solar system could be discovered and described mathematically. The positions of the stars were plotted regularly, principally to provide a fixed background against which any anomalous motion of solar system objects could be noticed. As the eighteenth century passed the problems of tracing the movements of celestial objects worsened, despite the improvements made in instrumentation - in fact probably because of those improvements. At the turn of the seventeenth century the known factors which affected celestial positions were refraction and precession, and astronomers did their best to allow for these by applying empirically derived results to them. But as astronomers increased the care and number of their observations and used instruments with improved scales and mechanisms, further influences on stellar and planetary observations were revealed. Two effects were discovered by James Bradley -

aberration and nutation - and it became necessary to allow for these in all subsequent observations. Also, certain astronomers were becoming increasingly convinced that not all stars were as "fixed" as had always been supposed. In 1719 Edmond Halley published a paper in which he pointed out that three bright stars had moved by a considerable amount (of the order of half a degree or more) since the records of their positions made by the Greeks (54). Following this, the French astronomers Jacques Cassini and P. C. Le Monnier (55) and, more notably, the German Tobias Mayer each compiled lists of motions peculiar to specific stars: their proper motions (56). By the end of the eighteenth century, astronomers agreed that proper motions existed and as a result they had to allow for ^{them} when stellar positions were reduced. The reduction of data became more and more complex as the century passed.

If Herschel's selection of double stars is assessed within this context, it is clear how many problems of reduction could be avoided and hence why he made his choice. For if he observed two stars which appeared to lie close together on the celestial sphere, since he was primarily interested in the variation of the angle between the two, the effects of refraction, precession, aberration and nutation would cancel out of the calculations. The two stars might of course have had different proper motions, but although these motions were certainly known to exist by the time Herschel was considering the problems of parallax, they were not yet allowed for in any systematic way during the process of reduction - if indeed such a process could be said to exist in any formal manner at this time.

Although Herschel was not the first astronomer to suggest a study of double stars, he was the first to appreciate fully the advantages of such a study. Similarly, he was not the first to observe double stars in the hope of detecting the parallax of the "nearer" component, but his grand project to survey all the heavens to which he had access, to gather as many suitable pairs as possible, far surpassed the

previous practical work undertaken by Long. The search for doubles was the first of a life-time of similar endeavours; Herschel was always determined to understand the whole of the observable heavens, by classifying all that was revealed in his great telescopes. His double star survey marked the beginning of his work on classification and was undertaken specifically with the intention of finding suitable objects to study for parallax (57).

Herschel published his important paper "On the parallax of the fixed stars" nearly three years after starting his survey of double stars (58). He began the paper by justifying his attempt to detect the phenomenon and explaining the type of instrumentation he believed was necessary. He then discussed Bradley's failure to detect the parallax of γ Draconis and made an important point previously alluded to by Maskelyne and Clairaut, but never before clarified. This is that γ Draconis is of the second or third magnitude and thus its parallax is probably considerably less than that of a star of the first magnitude. Hence, he maintained, "for aught we know to the contrary, the stars of the first magnitude may still have a magnitude of several seconds" (59)

This possibility was considered by Herschel to be a sufficient reason on its own for a renewal of the search, but he also commented on the fact that since Bradley's time there had been improvements in astronomical instruments, emphasising one aspect of this which he thought extremely important: his ability to use large telescopes with high powers of magnification. As higher and higher powers were employed, the images produced might be of low quality, but Herschel felt that this disadvantage was outweighed by the fact that fainter stars are revealed by higher powers; as he said:

"... a lower power may show more of the object; it may show it brighter, nay even distincter, and therefore upon the whole, better; and yet the greater power may,

in a particular case, be preferable: for if the object is so small as not to be at all visible with the lower power, and I can, by magnifying more, obtain a view of it, though neither so bright nor distinct as I could wish, is it not evident, that here this power is preferable to the former?" (60)

An additional advantage of using Herschel's arrangement was that the stars could be observed at any time while they were above the horizon: his telescope was not necessarily in the plane of the meridian and hence observation time was not restricted to instants of transit. However, one major disadvantage was encountered by Herschel: observation of the double for long enough to measure its separation involved being able to track it, as at the sort of magnification Herschel was using the speed of the object across the field of view was considerable. Herschel saw this rather than the distortion of the image as the limiting factor in determining how great a magnification could be used, and acknowledged that great skill was needed to control the telescope while following the double star. However, this was still, as far as he was concerned, the only way in which parallax was likely to be detected. He was aware that Roger Long had failed in his attempt some forty years earlier, but he could supply various reasons for this. Long's telescopes were insufficiently powerful and the stars he chose to observe were not suitable: the components of Υ Arietis were too far apart, those of Castor and Υ Virginis too similar in magnitude, and the components of the double in the middle of Orion's sword both too faint.

After all this and a detailed geometrical analysis of how the brighter star appears to move relative to the fainter, Herschel's paper disappointingly contained no observational results. Nor is there any evidence that he ever produced an analysis at a later date. Following the reading of his paper to the Royal Society Herschel produce two catalogues of double stars: the first early

in 1782 (61), followed by a more comprehensive one in 1785 (62). There was no way in which either catalogue could be used to check for parallax as Herschel entered values for the position and separation of the stars averaged over the period during which he observed them. Hence despite all Herschel's thought and practical work he did not obtain any results. The paper was read to the Royal Society on 6 December 1781; although already a Fellow of the Society Herschel was not present - at that time he was still living in Bath and earning a living as a musician - and he did not hear of any reaction to his work until he received a series of letters from his friend William Watson (63). According to Watson, the Society was generally very pleased with Herschel's paper, but they questioned his claim to have used successfully such high powers, and as a result to have seen the stars clearly as discs; there was no reference at all either to his parallax theory or to the lack of observations. In his reply (64), Herschel gave a defence of his claims on the grounds that his skill as an observer had come from years of practice, but the central theme of the original paper was not mentioned.

Some months later the paper came under review by the Royal Society's Committee of Papers, after which Nevil Maskelyne wrote to Herschel (65) explaining that they were indeed greatly impressed with Herschel's work, but they wished to raise certain points about it. The committee could not accept Herschel's assertion of the uniformity assumption, or his comparison of apparent magnitude with distance (Herschel had suggested an inverse proportionality between the two). Maskelyne brought to Herschel's attention the paper written some fifteen years earlier by John Michell. Michell's conclusion (see above, section 3.3) was compatible with Herschel's list of "nearly equal double stars" (that is those with components of similar brightness); the threat to his postulates lay with the "very unequal"

doubles, the very ones Herschel wished to use for parallax determination. For if two stars did actually lie close together and yet were of very different brightnesses (or apparent magnitude), Herschel's theory that brightness was inversely proportional to distance was plainly wrong.

Had Herschel accepted Michell's arguments this might have been his reason for not analysing his data for parallax. However, it transpires that Herschel in fact chose to ignore Michell's paper (66), so the problem of why Herschel produced no results cannot be solved that way. He did not return to his work on double stars until the early 1800s, although there is a brief mention of parallax in a paper read to the Royal Society in 1783 (67). The main subject of that paper was the possible motion of the Sun and solar system through space and how to detect this; when Herschel came to discuss the quantity of motion involved he remarked:

"From the annual parallax of the fixed stars, which, from my own observations, I find much less than it has hitherto been proved to be, we may certainly admit (without entering into a subject which I reserve for a future opportunity) that the diameter of the Earth's orbit, at the distance of Sirius or Arcturus, would not nearly subtend an angle of one second ... " (68)

This was written just two years after he had said he still believed it possible that the brightest fixed stars might have a parallax of several seconds. Presumably then he did analyse his data for parallax, but found nothing conclusive: all he could do was set the stars even further away than Bradley had done. The remark in brackets suggests that Herschel intended to write something more on the subject of the distance of the stars, but there is no evidence that such a work ever materialised. His later papers in the Philosophical Transactions certainly do not mention the smallness of the annual parallax. On the other hand his inability

to measure parallax did not prevent him from continuing his surveys and studies of the distribution of celestial objects. The 20 years following his paper on parallax were spent by Herschel searching the skies for nebulae and working on his construction of the heavens; his time was always very fully taken up in the pursuit of an understanding of the heavens, and his failure to detect parallax in no way changed his attitude to his life-long ambitions.

3.5 Conclusions: Piazzzi and the end of the 'gap'

The work which is generally regarded as marking the end of the eighteenth century silence with respect to parallax was that carried out during the 1790s and 1800s by the Italian astronomer ~~Giuseppe~~ ^Giauseppe Piazzzi, and that supposed resurgence of interest must now be assessed.

After being awarded the Mathematics Chair at the University of Palermo in 1780, Piazzzi pressed for permission to build an observatory there and sought financial support for a visit to France and England to obtain advice and suitable instruments. Both his requests were met. He first visited Paris, where he met Lalande and Delambre amongst others; from Paris he accompanied Cassini, Méchain and Legendre to England where he met Maskelyne, Herschel and Jesse Ramsden. It is uncertain when he left Palermo, but while he was in England he observed the solar eclipse of 3 June 1788 (69) and he certainly returned to Palermo in 1789, taking with him the 5' vertical circle which he had commissioned Ramsden to build for him (70). The observatory opened officially in 1790 and throughout the 1790s Piazzzi devoted much of his time to positional astronomy and the compilation of stellar catalogues. Probably his most famous contribution to the history of astronomy was his discovery of Ceres, the first asteroid, in 1801 (71), but he also played an important part in the history of attempts to measure

parallax.

According to Piazzzi's own account of his work in this field (72), in 1802 he noticed from observations taken over a period of 10 years a very slight variation in the position of Vega, which he could not permit himself to suppose was evidence of parallax (73). But this encouraged him to spend time observing other bright stars at transit, at the times when their parallax should be maximum, which he did for the following two years. He then reduced his data - allowing for all known phenomena and eliminating vertical errors by averaging readings taken after reversing the instrument on alternate evenings - and looked for evidence of parallax. His results were mixed, and as such unconvincing, but he published all of them, not just those he felt truly indicated parallax. Of the seven stars he considered, for only one (Procyon) did he give an unqualified answer: he claimed that star exhibited a parallax of 3". He tentatively suggested 4" for the parallax of Sirius but was uncertain of his summer observations because of high outside temperatures; the winter observations of Vega were dubious because of high winds; he felt his observations of Aldebaran needed to be rechecked, and that the parallaxes of both Capella and Arcturus were too small to be detected; finally, he gave a negative value for the parallax of Altair which he blamed on "accidental errors". It is thus not altogether surprising that Piazzzi's results were not considered to be convincing: he admitted as much himself.

Piazzzi's work did however mark the beginning of more than thirty years of serious work on parallax in observatories all over Europe: he himself later reduced data for the Pole Star (74); and his fellow countryman Guiseppe Callandrelli by reading Piazzzi was inspired to study the movements of Vega more closely. On this side of the channel there was a long controversy between John Pond, the Astronomer Royal at Greenwich, and his counterpart in Dublin, John Brinkley. On the continent the battle against parallax was fought with zest by

Baron de Lindenau, and, to greater effect, by Friedrich Bessel (in Königsberg) and F. G. W. Struve (in Dorpat). The eventual outcome of all this work was that acceptable values of parallax were produced in the late 1830s independently by Bessel, Struve and Thomas Henderson, who had spent some years observing from the Cape and so possessed observations of the star with the second highest annual parallax, α Centauri. Why the results these men produced were acceptable when all previous ones were not will be considered below; what remains to be done here is to draw some conclusions from the evidence already produced, and try to assess the importance of parallax and attitudes adopted to it by astronomers during the eighteenth century.

There are difficulties involved in trying to explain an apparent gap: in this particular case it must be decided who, if anyone, should have been interested or active in the detection of parallax, whether they were, and if not, why not. As Michael Hoskin pointed out when he assessed the effect of Bradley's discovery of aberration, very few people possessed instruments capable of anything like sufficiently sensitive measurements. The obvious places to consider first in this respect are the major European observatories, of which there were few in this period, the most important being those at Greenwich and Paris. The movements of the main figures at Greenwich (Bradley and Maskelyne (75)) have been considered; in Paris the astronomers most closely involved with parallax were Clairaut and Lalande and their work has been assessed in the context of the larger geodetic and geographic surveys which formed the bulk of work undertaken in Paris. At Bologna, interest in parallax was clearly expressed by Manfredi; the other important observatory of early eighteenth century Europe, at Copenhagen, was destroyed by fire in 1728, along with all its instruments. Apart from those astronomers at main observatories the only others in a position to try to measure annual parallax were the few independent

astronomers who managed to build or could afford to buy their own equipment: Roger Long, Samuel Molyneux (with whom Bradley worked during the 1720s) and William Herschel fell into this category.

If these are the people who should have been interested (and they are), then it is clear that there was not a gap in interest in the problem; at least, not one which lasted right through the second half of the eighteenth century. Admittedly, no results were produced in this period, but if that is how a 'gap' is measured there was one continual gap right up until the 1830s.

It is more revealing to accept instead that Bradley's paper did not have quite such a negative effect on his fellow astronomers; the next question to ask is what effect it did have. Bradley himself continued to take observations of stars at transit before and after he was appointed Astronomer Royal; he was obviously not looking for parallax, but by 1748 he was able to announce the reason for another apparent motion of the stars: this time it was the nutation of the Earth's axis caused by the gravitational attraction between the Earth and the Moon (76). The attention of some observational astronomers was diverted from the parallax question by Bradley's paper: during the 1730s they were trying to confirm the new theory of aberration. Bradley received a few letters from Maupertuis at the Paris Observatoire and from John Bevis in England showing that aberration had been detected in right ascension (77) (Bradley's original paper dealt with declination only).

It is possible that the initial reaction to the aberration paper was such that some astronomers decided against pursuing the problem, but this was only a temporary setback. Parallax had posed problems in the past: it was almost as though it was expected to pose them! The one exposed by Bradley was perhaps worse than usual. But it was not long before tentative solutions were suggested: Clairaut's paper in 1739 contained the first theoretical idea, and at about the

same time Long in Cambridge was trying out his alternative method. The general feeling seemed to be: if one method failed, this does not mean the question must be abandoned; try a new approach.

The longest individual gap of the period followed Long's work; although Lacaille's observations were made in 1751 and 52, they were not analysed for parallax for another eight years. If Bradley's work on aberration resulted in a complete lack of further investigation into the parallax problem during any period in the eighteenth century it was during the time between Long's work and Maskelyne's, but this does not necessarily mean that the question was not considered, merely that no new ideas evolved.

By the time Maskelyne and Lalande were working another objection to Bradley's argument had been produced: that Bradley had really only studied the one star, γ Draconis. When Maskelyne and, later, Herschel drew attention to this they were each disinclined to criticise Bradley, but there were hints in their writing that they felt they were part of a new generation, with new ideas and methods, so perhaps the question of parallax could be worth a new investigation. Herschel certainly felt that his instruments were a vast improvement on any Bradley possessed. The new hope expressed by Herschel was very similar to the attitude taken by Jacques Cassini in 1714 when, despite the failure of his father and his father's contemporaries to detect parallax, he declared that he would try again: the passage of time does much to restore hope!

Looked at in this way, Bradley's 1728 paper on aberration appears as a stumbling block in the history of attempts to measure parallax, but not as a dead end. The hunt for parallax persisted, even if only spasmodically, for good reasons. Firstly there was what could perhaps be called the "established" or "establishment" reason: the professed belief that

measurement of parallax would somehow prove the Copernican theory. As explained above when speaking of Maskelyne's statement concerning this, it is unlikely that any astronomers either believed that the existence of parallax proved the Copernican theory or felt they still needed to be convinced of its truth; it is equally unlikely that anyone in the scientific world needed to be converted. Perhaps the "truth of Copernicanism" propaganda was to be aimed at anyone else who still had doubts. (For example, some cartographers in the eighteenth century still portrayed the Tychonic system). However, if any other people were still debating the issue, it is unlikely that those who were not convinced by Bradley's aberration paper were going to be converted by the supposed hard fact of parallax determination. Still, this must be included as a reason for seeking parallax, as the astronomers themselves used it.

The second, and more acceptable reason for measuring parallax was that such a measurement could be used to establish certain stellar distances. It was in fact the most direct and simple way of measuring distance. The only other method available was based on the commonly, but not universally held assumption of uniformity among the stars, for which for a long time there was no counter-evidence, but equally for which there was no supporting evidence. It was a reasonable working model, but there was no way of confirming it and hence no way of knowing whether the distances discovered from it were accurate, or even of the right order. And all the results obtained photometrically depended on the acceptance of the uniformity assumption which could not be confirmed. A direct measurement of parallax was still likely to produce the most convincing value for stellar distances.

A knowledge of stellar distances became more important as the eighteenth century proceeded and as it was appreciated more and more that the "sphere" of the fixed stars did indeed possess a third dimension. Its importance also grew with the development of sidereal astronomy as a separate enterprise from planetary

astronomy. Once it was accepted that the so called "fixed" stars were not fixed at all, interest in them as individuals grew, albeit very slowly at first, and one of their most important properties was their distance from the Earth. For William Herschel, who wished to establish an accurate picture of the universe in three dimensions, a knowledge of distance was fundamental.

Finally, astronomers were driven to look for parallax because it should have been exhibited by the stars. It was possibly over anxiety to do this which led them into making mistakes: it would have been a great achievement for any individual astronomer to be hailed as the one who had at last overcome the many difficulties. So the battle against parallax went on at least partly because the individuals involved did not like to admit defeat.

Piazzi's role in the development of the problem must be considered now. If nothing was done between Bradley's work and Piazzi's surely the latter's new interest requires an explanation. Yet none has been produced. Now if, instead of seeing the second half of the eighteenth century as a gap in work done on parallax, it is seen rather as a continuation of late seventeenth and early eighteenth century work, during which time there were a number of theoretical and practical ideas, but no definite results, Piazzi's work is not so anomalous. He was in contact with other important European astronomers, in particular Lalande, Maskelyne and Herschel, who had all shown interest in parallax, so it is not impossible that he too should have been aware of the problem and interested in solving it. It is unlikely that he was consciously looking for parallax when he was studying his observations of Vega in 1802, but the excitement he felt when he noticed the periodic motion must have been considerable, and quite possibly comparable with that felt by Flamsteed a century earlier when he studied his observations of the Pole Star (78).

This picture of similarity throughout the eighteenth century exposes the artificiality of the role of Bradley's paper as the great divide in pre-nineteenth century attempts to measure parallax. It leads instead to a different question, namely what was it about Piazzzi's generation of astronomers compared with their predecessors, that made his work mark the beginning of the final onslaught on the problem of parallax? For it is true, as noted above, that astronomers were very active in the field, compared with previous periods, for the following thirty years.

The obvious first answer to this is that the instruments available in the early nineteenth century were of a higher quality than any earlier ones. Developments were not unique to the beginning of the nineteenth century, however. As we have seen Herschel spoke of his improved optics, by which he meant his production of high-power optical systems, but his contemporaries did not start looking for parallax after reading his 1781 paper. Admittedly, it might not have been easy for them to have built telescopes similar to those constructed by Herschel (79), but there is no evidence that anyone wished to. One breakthrough in optics - the cure of chromatic aberration - had begun to take place some time before, indeed as early as the 1750s with introduction of the Dolland achromatic lens, but such improvements on their own were insufficient.

There were undoubtedly other instrumental improvements as well. Allan Chapman in his thesis Dividing the circle (80) argues that the introduction of circular instruments marked the third great breakthrough in the history of astronomy, because their use eventually enabled parallax to be measured, by providing sufficiently accurate background charts against which to measure relative movements of the nearest stars. The instrument built by Ramsden for Piazzzi was a vertical circle, the reversible properties of which Piazzzi could and did exploit. Apart from the

circle built by Ole Roemer very early in the eighteenth century Piazzzi's instrument was the first of its kind to be used at a major European observatory. However, although highly accurate instruments were essential for the measurement of parallax, improvements in this area alone were not enough to lead to its detection. If this were so, why did it take as long as forty years from the general introduction of circular instruments to the final measurements?

One reason why astronomers in the early nineteenth century were more persistent than their predecessors was that by this time there was a greater need to detect parallax, to produce reasonable estimates of stellar distances. The model of the heavens based on uniformity was no longer adequate: in 1767 Michell had produced convincing theoretical reasons for rejecting it, and ironically enough, it was William Herschel who provided observational evidence refuting the assumption. In a paper written in 1803 (81) Herschel announced that several of the double stars he had originally observed in the early 1780s were now differently orientated with respect to each other. In other words the two components had moved relative to each other and this could only be explained by assuming the two were physically associated and moving with respect to each other because of their mutual gravitational attraction. The natural conclusion was that brightness was not necessarily an indication of distance.

A further observational reason for rejecting uniformity was supplied by the growing number of proper motions which were being detected. By the early nineteenth century it was becoming obvious that proper motion values did not agree with the uniformity principle. It was logical to assume that nearer stars would exhibit higher proper motion than more distant ones, and if the brightest stars did not possess the highest proper motions this would be another factor against the uniformity assumption. It was not for some

time that high proper motion was considered directly as an indication of nearness (82), but in the 1800s it was already realised that evidence of proper motion was not compatible with the assumption.

Eventually it became clear that stars with high proper motion were likely candidates for parallax considerations but the realisation came only gradually. In 1812 Bessel confirmed the extraordinarily high proper motion for the faint star δ Cygni and following that there was a new criterion for astronomers to work from. Inability to agree on which stars to choose to observe for parallax was certainly a difficulty for all astronomers in the eighteenth century: was it better to work on the very bright stars which possibly never got sufficiently high in the sky to avoid the problems of refraction, or to study fainter stars whose parallax might be less, but which culminated at altitudes which allowed refraction to be ignored? With the growth of information concerning proper motion there was a new factor to consider, and it is very significant that two of the three parallaxes which were measured in the late 1830s were for stars with appreciable proper motions.

All these factors: improved instrumentation, greater need for parallax detection, and new selection criteria, taken together, may explain why the efforts of the first thirty years of the nineteenth century were eventually so much more successful than previous ones, and also why astronomers persisted until the fight was over.

Before finishing this account of the eighteenth century 'gap' in stellar parallax measurements, it must be stressed that it is important to keep this study in perspective. In other words it must always be remembered that parallax was not a dominant problem at all during this period: it was a small part of the still young study of stellar astronomy. It was always of interest to astronomers, but each time it proved elusive there were plenty of other projects for astronomers to work on. Throughout the eighteenth century the most important

field of study to almost every astronomer was the solar system: even William Herschel, who contributed far more to the new stellar astronomy than any other eighteenth century astronomer was perhaps most famous for his solar system work, that is, the discovery and study of Uranus. In France the greatest emphasis was on the study of geodesy, as can be seen for example from Maupertuis's collected works (83), or even from Lalande's Astronomie.

The position was to change only gradually during the early years of the nineteenth century but, having said that, it is still true that a study of eighteenth century attempts to measure parallax is essential if we wish to appreciate the more rapid developments which led in the end to a correct identification of the phenomenon.

Chapter Four

PROBLEMS OF PRECISION IN THE EARLY NINETEENTH CENTURY

4.1 Introduction

In section 3.4 some account was given of developments in positional astronomy during the eighteenth century, and it is easy to see that as the century passed the difficulties of calculating the exact position of a celestial object at a given time increased rather than decreased. By the turn of the century astronomers had to devote far more time to the process of reduction than had ever previously been necessary. There was an ever increasing demand that new celestial positions recorded in catalogues be more accurate than any already in existence, and an associated increase in astronomers' awareness of the problems involved. Not only did they have to allow for each of the physical effects referred to above (see section 3.4), they also had to take account of instrumental errors: misalignment of the instrument, errors inherent in its design, and the effects of temperature changes on any metal parts. Astronomers were continually striving to better their own particular methods of coping with the vast quantities of data which they had accumulated, but it is glaringly apparent that during the first 15 to 20 years of the nineteenth century what they lacked more than anything was a common method of reduction. It was very difficult for any two astronomers to compare usefully their reduced data because each might have introduced different variations into their respective calculations; the calculations themselves could not be compared effectively, as the methods of reduction differed to such

an extent. The overall picture of positional astronomy in the early nineteenth century is one of much hard work, but of that work resulting in a mass of uncertainty.

Early nineteenth century attempts to measure parallax must be considered within this context if they are to be properly understood. By this time astronomers knew that parallax could not amount to more than, at the very most, a small number of seconds of arc, and before any value could be accepted all other disturbing phenomena had to be eliminated from the observed data. In fact, a study of papers written on parallax gives one of the clearest indications possible of how grave the problems of precision in positional astronomy were. Conversely, it is in terms of the general increase of interest in stellar astronomy and the associated problems of precision that both the failures to detect parallax during this period, and the persistence of astronomers despite such failures, must be explained. The wish for accurate catalogues forced astronomers to seek ways of improving their techniques, both of observation and of reduction; it encouraged them to press for highly accurate measuring devices from the instrument makers, and to think very carefully about the analysis of their data, all of which was necessary before astronomers could realistically hope to detect parallax. But, as successive improvements were made, what better way of testing them than by trying once more to measure the elusive phenomenon of parallax? After all success in making such a measurement would have been an excellent demonstration of the outstanding achievements possible within the precise study of positional astronomy. However, until sufficient refinement of both the instrumentation and the methods of reduction was reached it is clear, with hindsight, that each attempt was bound to end in failure. Before proceeding any further with the history of attempts to measure parallax, therefore, the prevailing problems in positional astronomy during the first quarter of the nineteenth century must be assessed.

4.2 Instrumental problems: the introduction of the altazimuth

By 1800, as mentioned above (see section 3.5), a preference was being expressed by astronomers for circular instruments: that is, for telescopes attached to a full, graduated circle rather than to a quadrant or any other sector, as had been the usual practice throughout the eighteenth century. Why the superiority of the circular instrument should have been appreciated when it was is a difficult question to answer. The design was not completely new in the late eighteenth century; in the first decade of that century the Danish astronomer Ole Roemer designed and built his Rota Meridiana or meridian circle, which became his most useful instrument (1). Roemer was also responsible for introducing the transit instrument into astronomical observatories, and it is very probable that the two instruments - transit and circle - were closely linked together in his thoughts. The transit instrument was built as an interim measure to be used while Roemer's new observatory was under construction. Once the latter was completed Roemer virtually abandoned the transit, favouring instead the meridian circle. However, it was only the transit instrument which was tried at other European observatories during the early decades of the eighteenth century; James Pound erected one at his private observatory in Wansted and Edmond Halley built one to be used on a temporary basis at Greenwich during the 1720s. Perhaps it was the relative ease of construction of the support for a transit instrument that was the reason for its adoption in observatories other than at Copenhagen. Certainly, engraving a full circle would have been no easy matter before large dividing engines were developed in the later eighteenth century. Whatever the reason, the idea of a solid vertical circle to carry a telescope was, it seems, ignored for many years. However, it is possible that the potential step

from a transit instrument, which can rotate through a full 360° in the plane of the meridian, to a telescope attached to a fully graduated vertical circle was realised again sometime during the mid-century. Of course, that cannot be the whole story; from Allan Chapman's work on the eighteenth century instrument makers (2) it is apparent that they, rather than the astronomers, were the first to consider the use of a full circle to support a telescope. The earliest reference to a publication on the division of full circles given by Chapman is to a book written by the Duc du Chaulnes in the 1760s (3). Chaulnes appears to have been fascinated by the mathematical problem of scale division and it was probably this fascination rather than the wish to improve astronomical instruments which motivated him. However the book was read by Jesse Ramsden, who, according to Chapman, became interested in the graduation of full circles during the 1770s.

Ramsden's first successful and important circular instrument was the surveyor's theodolite which he built at the request of the Royal Society. By the 1780s Ramsden's reputation as an instrument maker was second to none, and hence the Fellows of the Royal Society naturally turned to him for the theodolite they required. By this time also Ramsden had perfected his design for circular instruments, and was prepared to use the design for the new instrument. The Royal Society's request for the theodolite was pressed once more after a visit to London by Cassini III in 1785, and his re-emphasis of the need for an accurate knowledge of the difference in latitude and longitude between the observatories at Greenwich and Paris, which would be beneficial to both British and French astronomers. Cassini and several of his colleagues employed at the Observatoire had already extended their survey as far as Dover and were prepared to continue to Greenwich. However, General Roy of the Royal Engineers had been employed since 1783 by the British Government to carry

out the triangulation from London to Dover, and he continued the work in this country. After measuring a base-line across Hounslow Heath during the summer of 1784 he extended the triangulation to the Kent coast during the summer of 1787 using the theodolite eventually built for him by Ramsden (4).

It was the following year that Ramsden met Guiseppe Piazzi, who had come to England to obtain advice about astronomical instruments for the newly founded observatory at Palermo. As a result of this meeting Ramsden was commissioned to build a 5-foot circular instrument for Piazzi, which the latter took back with him to Palermo in 1789. It is reasonable to suppose therefore that, during the course of their conversation, Ramsden spoke of the design of his most recent instrument, the theodolite, and suggested it might be enlarged and modified to be used as an astronomical instrument.

According to Chapman,

"Ramsden's altazimuth instrument built for Piazzi's Palermo observatory, was the first really important astronomical circle". (5)

It was certainly well used. During the fifteen years following its installation, Piazzi catalogued over 8000 stars, observing each one several times before entering its position into the catalogue. As it was an important, newly designed instrument, it would be useful to study its innovations fairly closely, and then compare it with the other important instrument designed by Ramsden (but not completed during his lifetime): that built for John Brinkley at the Dunsink observatory in Dublin. By doing this it should be possible to show what standard of accuracy was to be attained by 1800, where improvements were thought to be necessary, and how any improvements were achieved in the second instrument.

There is an account of the Palermo circle written by Piazzi himself in Italian (6), and there are also two written in English. The first of the English

descriptions was published in 1790, soon after the circle was built, by the Rev. Samuel Vince, who included the description in his series of lectures on practical astronomy given in Cambridge (7); the second was contained in a book on practical astronomy written in the 1820s by the Rev. William Pearson, who ran his own private observatory in South Kilworth in Leicestershire (8). In the preface to the published version of Vince's lectures he acknowledged that he was greatly helped by Ramsden with certain sections of the book, so presumably his information came directly from Ramsden. On the other hand Pearson's account was taken from Piazzzi's book. The opinions of both the user and the builder of the circle as to the reasons for its superiority over any other any other instruments then in existence are therefore documented; using these sources it is possible to draw a number of conclusions about the introduction of this new instrument into the general practice of astronomy.

The basic design of the instrument may be seen in Fig. 1 (taken from Pearson, who copied it directly from Piazzzi): the telescope (AB) was fixed securely to a horizontal axis and fastened between two parallel, equal, connected brass circles free to rotate in a vertical plane, and this whole framework rotated in azimuth also. There were two important new features (Roemer apart) of such a design. Firstly, and most obviously, the telescope was attached to a complete, fully graduated circle; secondly, and perhaps even more importantly, for the first time two mutually perpendicular motions of the telescope were combined effectively in one instrument. Previously it had been perfectly possible to observe stars out of the meridian, using any moveable instrument (usually a quadrant) unrestricted to one particular plane. But such a procedure was not very useful for obtaining accurate stellar positions. The most effective method for doing that was to use either a transit instrument or a mural instrument placed in the plane of the meridian to measure declination or zenith

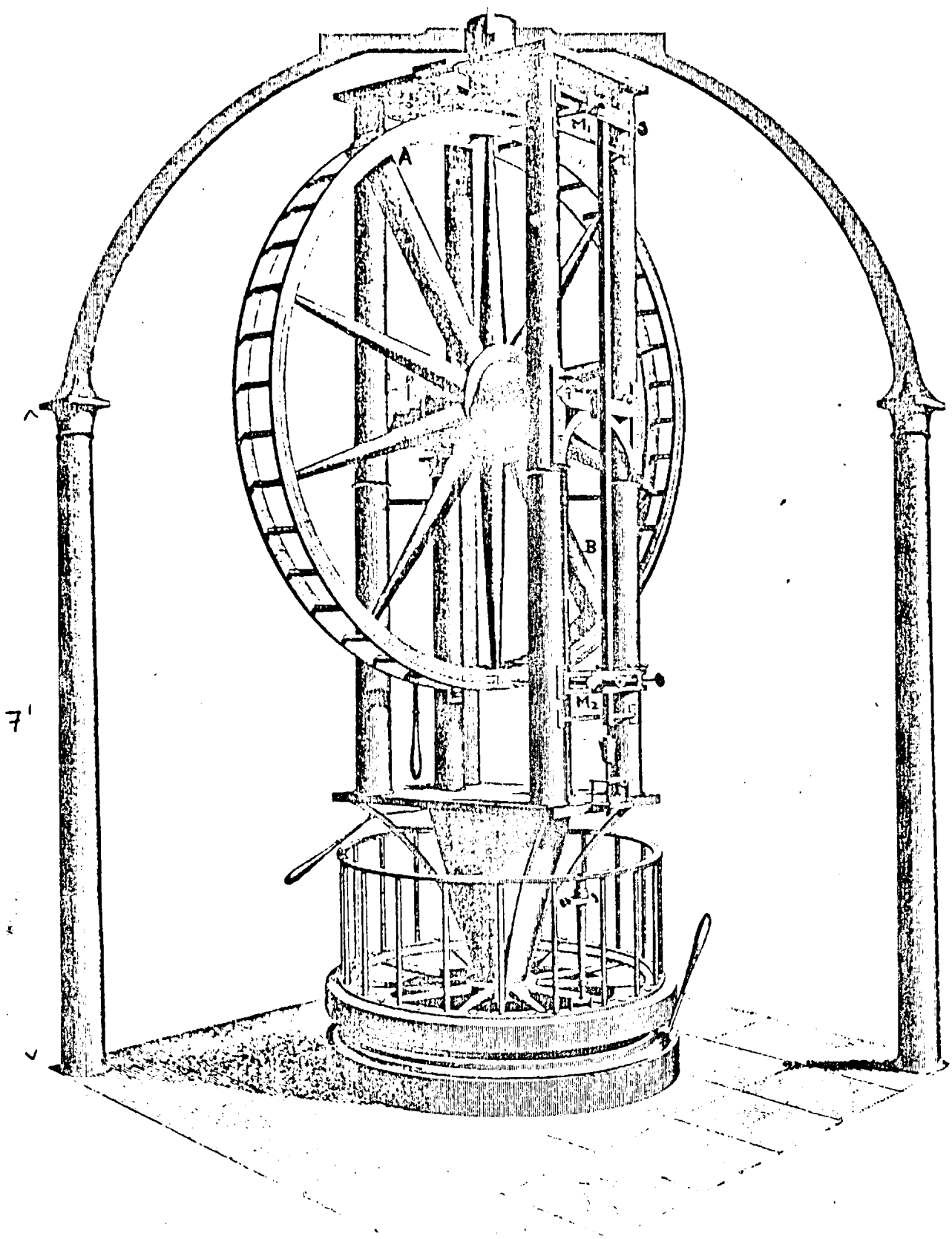


Figure 1 - Piazzini's altazimuth; from W. Pearson, An introduction to practical astronomy (1824).

distance, together with a clock to measure the right ascension. The only important measurements made out of the meridian during the eighteenth century at Greenwich were made with equatorial instruments which measured angular distances between two objects. Such measurements were particularly useful when tracing the path of a comet, but had to be used in conjunction with measurements of the right ascension and declination of stars lying near the comet's path. These latter measurements still had to be obtained from observations made at transit. With the introduction of the altazimuth it became possible to map the stars accurately at any time, which meant that problems such as the star's being hidden by cloud at its time of transit could be overcome (9).

The development which is of greatest relevance to this study was the use which could be made in the cross checking of observations of the reversible property of the circle. This property was of course applicable to both the vertical and the azimuth circles. The alignment of the instrument could be checked easily, and an ability to do this clearly affected the accuracy of the observations obtained with the instrument. Both Vince and Pearson stressed repeatedly the importance of the initial alignment of the instrument, together with the importance of the methods of adjustment to be applied whenever any discrepancy was observed. Pearson devoted several pages of his description to an explanation of how various devices were used to check the alignment, and followed this with a step-by-step account of how to make a particular observation, showing how important he felt Ramsden's innovations were to the development of practical astronomy. The devices employed by Ramsden were all simple in themselves, and all acquired their importance in combination with another simple property: the symmetry of the circle.

Before the instrument was used, the first thing to check was that the vertical circle really was vertical, which could be accomplished by measuring the distance

between one plane of the circle and a plumbline at two points sufficiently far apart. Assuming the plumbline was itself vertical (and it was enclosed within a wooden framework with the bob immersed in water to prevent draughts from disturbing it) the two measured distances should be identical. Because of the way in which the plumbline was attached to the framework of the instrument, it could be used in all azimuthal positions. It was also possible to check regularly, between observations, that the altitude circle was still vertical. Once the vertical axis had been checked, the horizontality of its axis was immediately affirmed, because as Pearson said,

"... the circle was formed in the lathe upon the pivots of its own axis [thus] its plane stands by construction at right angles to the line passing through the axis, that joins the centres of the pivots; and therefore when the plane of the circle is adjusted to become vertical, its axis necessarily becomes horizontal". (10)

It was at the next stage in the checking of the instrument that the symmetry or reversible property of the circle was exploited. Fig. 1 shows two microscopes attached to part of the vertical structure (M_1 and M_2); they were deliberately situated opposite the lowest and highest points of the altitude circle. The circle itself was graduated into four successive quadrants; it therefore possessed two sets of zero points 180° apart. The observer was to focus M_1 and M_2 on each of an opposite pair of zero points and clamp the microscopes in these positions. Then, by turning the circle until the zero point which had been seen through M_1 could now be seen through M_2 , he could check whether or not the other zero point was visible through M_1 . Ideally of course it should be.

However, the important point was not that the instrument should be absolutely perfect, but that any error should be measurable. In this case it clearly was: the error could be read directly from the mark

on the scale visible through M_1 once the circle had been turned. This point, that certain instrumental errors ceased to be important once they could be predicted and allowed for, is mentioned nowhere in the literature concerned either with the history of instruments or with the more general history of astronomy (11). Yet it was significant to the astronomers themselves, as is obvious from Vince's 1790 account; he included this feature in his list of advantages of the circular instrument over the quadrant (12). It was a point which was also singled out by Pearson once he had described the detection of and allowance for collimation errors, which must be considered next.

Errors of collimation were also detected through a consideration of the symmetry of the circle, this time in conjunction with the use of a distant marker. The marker would be observed with the telescope fixed in a known position; then by turning the vertical axis through 180° (that is, until one particular microscope reads the opposite zero point) the marker should be brought back into the centre of the field of view by swinging the telescope in the vertical plane only if there were no collimation in azimuth. Finally, collimation in zenith distance was detected by observing the transit of the Pole Star; its zenith distance as measured before and after reversing the circle and inverting the telescope should be identical for there to be no collimation. The Pole Star was chosen because, since it is close to the north celestial pole, its apparent motion through the field of view of the telescope is very slow, allowing time to move the telescope and to take the two readings before the star appears to move an appreciable distance.

Following the discussion of collimation errors, Pearson emphasised the importance of knowing the extent of an error rather than being able to correct it. According to his account,

"... it is found from experience, that the adjustments will not be permanent, and that good results may be obtained, particularly

with this instrument, rather from the application of known corrections, than from a dependence on the continuance of perfect adjustments for any considerable time". (13)

Pearson's remarks were rather more sophisticated than those made by Vince, who merely pointed out that errors in division of the scale or in the reading off of the measurement could be discovered easily with a circular instrument, and it must be remembered that Pearson was writing as late as 1829, by which time it was usual to allow for instrumental errors in the way that he described. However, in the section quoted, he was referring specifically to the Palermo circle, and described sets of data collected by Piazzzi with this instrument, suggesting that the Italian was conscious of this important new aspect of the vertical circle. It is true that accounts of Piazzzi's work have remarked on the importance of the reversible property of circular instruments (14), but no one has explained in more than vague terms where the importance lay. Surely the significance of the introduction of this instrument was that thereafter certain instrumental errors could be calculated, and therefore allowed for in the reduction of data. Such errors then became unimportant and it was no longer vital to be able to correct them. They were not eliminated completely; they were reduced to a level where they could be dealt with effectively.

The other advantages of the Palermo instrument were, according to Vince: that it could measure azimuths as well as altitudes; that the scale could be divided more accurately than the scale on the quadrant could be; that it was easily adjustable and could be turned without difficulty, thus helping to keep all parts at a similar temperature; that it was less likely to change its figure than was a quadrant; and perhaps most importantly for the process of the reduction of data, that it could provide useful information about refraction, as this phenomenon does not affect azimuths. It is important here to notice that most of the advantages noted by

Vince resulted directly either from the symmetry of the circle, or from the combined use of two mutually perpendicular circles.

The introduction of circular instruments was, and was recognised by contemporaries to be, a very important development within the practice of astronomy. But what did it mean in terms of accuracy? According to Pearson, an inclination of the axes as small as one second of arc could be detected, and when zeros on the circle and microscope were lined up,

"... these points did not deviate more than a quarter of a second from their true places". (15)

However, Pearson also noted that the vertical circle was liable to be out by up to 3" and the azimuth circle by as much as 6"; these were errors of the division of the scale which could be detected and allowed for, but there was the additional hazard which resulted from exposing the structural parts of the instrument to sunlight. This last effect was less predictable than the scale errors and taking Pearson's value once more, might have affected readings by as much as 4 or 5".

Nevertheless, the achievement of this kind of accuracy was something of which both astronomers and instrument makers were justifiably proud. Moreover, the problem of temperature variation and its effects on the metal parts of the instrument was recognised, and attempts were made to cope with it. As pointed out above, Vince included in his list of advantages the rotatability of the circles as a means of keeping temperature gradients as insignificant as possible, indicating an awareness of the problem. When the second important circle, that built for Brinkley, was designed by Ramsden (16), many of the main structural sections were built in masonry, instead of metal, thus eliminating at least some sources of error (17).

There were also other minor improvements made to the second instrument. When the Dublin circle was first used it possessed four microscopes around the rim of the circle instead of two, but after a short time

Brinkley gave up using the fourth as it was difficult to read, being so far from the ground. But using three microscopes still meant that he was able to cross check readings to an even greater extent than Piazzi had been able to do. The second circle was 8 feet in diameter compared with the 5 feet of the Palermo circle, which meant that the divisions on the scale were easier to distinguish. It had been thought that a circle much larger than 5 feet would cause problems of stability and rigidity, and in fact Ramsden refused to build the 10 foot circle that Brinkley originally ordered. He tried to make one of 9 feet but apparently was not satisfied with the resulting circle, and eventually he and Brinkley settled on 8 feet as the diameter.

When the circle had been in use for only a short period of time (about a year) Brinkley asserted that he was very well satisfied with the instrument, and by 1810 he believed that he possessed observations accurate enough both to show a parallax in Vega, and to show that Bradley's formula for refraction, which had been in use for some decades, needed to be corrected (18). Throughout the 1810s he claimed that the overall accuracy of his observations was within a single second of arc, an opinion endorsed by Pearson in 1829 (19). However, Brinkley's claims were disputed by the British Astronomer Royal John Pond during their celebrated controversy over whether or not Brinkley had detected parallax. As that dispute illustrates clearly all the various problems of precision prevalent in positional astronomy during the early nineteenth century, it will be discussed in detail below (section 4.4). For the present all that remains to be said is that it seems likely that the Dublin circle was sufficiently accurate to expose a difference in position of 1", but that this did not mean that the final position allocated to an object would have such a degree of accuracy.

At this stage it should be pointed out that Ramsden was not the only person producing circular instruments at this time: Edward Troughton was also very famous for the highly accurate circular instruments

that he constructed (20), the most famous of which was probably the Westbury circle which he made for John Pond's private observatory before the latter succeeded Nevil Maskelyne as Astronomer Royal (21). Concentration in this section has been on the two instruments made by Ramsden partly because his work was slightly earlier than Troughton's, and partly because the Palermo and Dublin instruments both played a significant part in the history of attempts to measure parallax. Also, Ramsden's work is as representative as Troughton's of the best instruments available in the period under discussion, and in an assessment of problems of precision it is the highest degree of accuracy available which is significant.

To sum up finally, the importance of observational or instrumental problems in early nineteenth century positional astronomy, it may be said that they were obviously known to exist, and much had been done to allow or correct for them. The advantages of the circular instrument were fully acknowledged, and the only remaining important problem which astronomers were to argue about at length was the effect of temperature on the figure of the instrument. By 1820, according to Allan Chapman, instruments existed which consistently gave readings to one tenth of a second (22). In other words if the detection of parallax had relied simply on the availability of sufficiently sensitive instrumentation, the phenomenon should have been measured by 1820, since it is now known that the largest parallaxes are of the order of several tenths of a second. However, parallax was not detected for a further 18 years. In order to explain why this is so, problems related to the other aspect of the acquisition of accurate stellar catalogues must be analysed: that is the problems of the accurate reduction of data.

4.3 Problems of data reduction

The period 1790 to 1820, therefore, saw the introduction of a new and effective instrument which, once it had been

refined, could technically give readings which might demonstrate the existence of parallax in several stars. This being so, why was it that parallax was not detected until nearly 20 years later? It cannot be argued that astronomers were not trying to find it; Pond, Brinkley, F. G. W. Struve and John Herschel, amongst others each wrote on the subject during this time. The fact is that technical accuracy of the bare observations is not enough; the reduction has to be accurate as well. That is, astronomers needed to know to a high degree of accuracy how to allow for each physical phenomenon which might be causing an apparent shift in the position of a star. It is because astronomical constants were not sufficiently accurately known, and certain physical effects were not sufficiently well understood, that reduced data was not precise enough to show the existence of parallax. Astronomers of the early nineteenth century were aware that they needed to come to terms with these problems and they attacked them with great energy. A study of their work in this area reveals much to explain why parallax remained elusive.

Firstly, the phenomena which affect celestial positions must be described. The one which had been known longest, precession, was first described by Hipparchos in the second century BC; it is the result of the regular movement of the point of intersection of the celestial equator and the ecliptic, the first point of Aries (23). Since all celestial positions are measured from this point, stars are subject to an apparent motion in celestial longitude, as the first point of Aries moves along the ecliptic. By the late eighteenth century values of the precessional constant, that is the annual displacement of the first point of Aries, were being quoted to a quarter of a second of arc: Jérôme Lalande, in the third edition of his Astronomie gave a value of $50\frac{1}{4}''$ per year. He claimed to have calculated this value from a comparison of positions in Lacaille's stellar catalogue with those

in Flamsteed's (24). In stating the value $50\frac{1}{4}''$ for the constant of precession, Lalande did not consider at all whether the value might be subject to any error, as a result either of errors inherent in the two catalogues he used, or of the effect of any other phenomena. He appears to have made two unconscious assumptions: firstly that the effect of precession could explain fully the difference between the stellar positions allocated in the two catalogues, and secondly that $50\frac{1}{4}''$ was the absolute value of the precessional constant. It was not only Lalande who made assumptions of this kind during the closing years of the eighteenth century, and it was in such assumptions that many of the problems of positional astronomy lay. There was no concept of degree of accuracy as far as astronomical constants were concerned, and the difficulties of isolating one phenomenon from all others were far from being recognised, and were certainly therefore not being coped with. The tendency was to calculate a value for a constant and from that point onwards to assume that this was its correct value. Astronomers naturally dealt with one phenomenon at a time, but they did so by assuming at each stage that the phenomenon they were studying was the only one affecting the observations under consideration. As a result there was little agreement between astronomers over the values to ascribe to astronomical constants; it is hardly surprising, therefore, that the reduced data of one did not agree with that of any others to any great degree of accuracy.

The value for annual precession was in fact one of the least controversial constants that astronomers used. The value already mentioned was that calculated by Lalande; according to Robert Grant, writing in 1852, the only other value suggested around the turn of the eighteenth century was $50''.1$, which was calculated by Jean Baptiste Delambre from a comparison of the observations of Bradley, Mayer and Lacaille with his own (25). Brinkley, in his second major contribution on parallax, read to the Royal Society in March 1818, claimed that

precession was "... determined with sufficient accuracy" (26) and since in this paper he was replying to criticism from John Pond (27) to his first paper, Brinkley would have been likely to explore the tiniest possible source of variation in stellar positions. Thus it may be assumed that variation in the value of the constant of precession was not large enough to appear to have any significance in the parallax problem. However, although it is clear that Brinkley was aware by 1818 that he needed to consider how accurately he knew the constant of precession, such insight was not a general feature of astronomical work of the early nineteenth century. And although the degree of accuracy of the constant was perhaps not such a problem when precession was considered (28), it certainly was a problem when various other constants were used in astronomical calculations, particularly the constants of the aberration of light, of nutation and of refraction.

Of these three phenomena, two, aberration and nutation, were still comparatively new discoveries in the late eighteenth century, having both been first recognised by James Bradley, who wrote about them in 1728 (29) and 1748 (30) respectively. Towards the end of the eighteenth century and during the early nineteenth century it was fairly common for astronomers to try to reevaluate the constants of aberration and nutation. This tendency was in accordance with the general increase of interest in stellar astronomy taking place at this time, but the point relevant to this discussion is that there was still no general agreement over the values of these constants.

In his original paper on aberration, Bradley gave a value of $40\frac{1}{2}''$ for the maximum value of aberration, that is for the maximum possible displacement in celestial longitude over a period of 6 months as a result of aberration. Then, 20 years later, in his paper on nutation he wrote that he had

"... again examined [his] observations that were most proper to determine the transverse axis of the ellipsis, which each star seems to describe and [had] found it to be nearest to $40''$ ". (31)

Thus Bradley's value for the constant of aberration (the length of the semi-major axis of the aberrational ellipse) changed from $20\frac{1}{4}''$ to $20''$. In Bradley's calculations this difference was insignificant since he was only working to the nearest second and never claimed any greater accuracy than this for his observations. However, by 1810, astronomers were giving their observations to a tenth of a second, and the constant of aberration was calculated to this accuracy also, so differences of $\frac{1}{4}''$ became important.

A further problem arose from astronomers' reluctance to generalise. Values for the constant of aberration were derived from observation, and being necessarily cautious, astronomers would only quote the value obtained for each individual star. For example, when the German astronomer von Zach produced his work on aberration and nutation in 1806 it was given in the form of tables; in other words a specific value for each separate star observed was presented (32). Von Zach's more extensive work, published in 1812, was in a similar form (33). According to Brinkley, certain values in von Zach's tables "differed considerably" from those given by Bradley (34).

The earliest signs that astronomers might be prepared to generalise came in 1814 when Delambre gave a value of $20''.253$ for the constant of aberration, which he had derived from observations of the satellites of Jupiter (35). He believed this value was applicable to calculations of stellar positions. Nevertheless, although this value was adopted by some astronomers, it was not done so without reservation because, as Brinkley wrote in 1818,

"The strongest argument for $20\frac{1}{4}''$ [was] derived from the researches of M. Delambre, respecting the reflected light from Jupiter's satellites; which certainly cannot be considered conclusive as to the direct light of the stars". (36)

In the same paper Brinkley argued further that a difference of $\frac{1}{4}''$ in the constant of aberration could produce an uncertainty in the position of the Pole Star of about $0''.2$ or $0''.3$ in one direction in July, and that it could produce a similar uncertainty in the opposite direction at the other end of the year. What Brinkley does not seem to have realised is that, as far as parallax is concerned, and that was after all what he was discussing, the important times of year for measuring parallax occur when the aberrational effects are minimum (for the Pole Star this happens in April and October). Nevertheless, Brinkley was right when he summed up his and his contemporaries' knowledge of aberration by saying "at present there exists an uncertainty" (37).

The same was true of nutation, which was actually fairly difficult to distinguish from precession. After Bradley's discovery of the phenomenon it was studied during the eighteenth century by J. H. Lambert and Nevil Maskelyne; it was then studied extensively during the early nineteenth century by Delambre and Laplace, who each devoted long sections to it and precession in their major astronomical publications (38). According to Bradley in 1748 (39), the maximum effect of nutation, over a semi-period of a little more than 9 years was $18''$, and, as in the case of aberration, for his purposes such a value was sufficiently accurate. In fact his main intention had been to show that the effect actually existed rather than to obtain an accurate value for the constant. However, by the early 1800s astronomers needed a value accurate to within a tenth of a second; unfortunately, by 1815 Laplace still could not evaluate it more precisely than somewhere between $9''.31$ and $9''.94$ (40). Although the annual effect of nutation was fairly small, it was necessary for astronomers to be able to allow for it to the same degree of accuracy as for all other effects, and Laplace's work indicates that in common with the

knowledge of other astronomical constants the degree of accuracy to which that of nutation was known was not high enough to say precisely how much of a discrepancy in an observation might be explained by it.

Of all the phenomena affecting stellar positions, perhaps the most difficult to cope with was refraction by the Earth's atmosphere. Like precession, it had been known to exist since Antiquity, but it was not until Tycho's time that astronomers attempted to estimate empirically the extent to which it affected astronomical observations, and it was not until the seventeenth century that any theories of atmospheric refraction were suggested. By the early nineteenth century however two rival sets of refraction tables were in use: the Greenwich tables, compiled in the mid-eighteenth century by Bradley (41), and the tables more usually favoured on the Continent, the French tables, first supplied by Lacaille but modified in the first decade of the nineteenth century by the theoretical considerations of Laplace and numerous observations made by Delambre (42).

The main difference between the two sets of tables was in the method whereby the possibility of variation in temperature was incorporated into the formulae. Other than that the two are difficult to compare because Bradley's appear to have been derived from a trial and error method depending entirely on his observations, whereas the French tables were derived from Laplace's model of the Earth's atmosphere and modified by observations made by Delambre and Piazzzi. Both incorporated terms to deal with atmospheric pressure and temperature. Bradley's formula was simpler. For him:

$$R = \frac{a}{29.6} \tan(z - 3r) \times \frac{400}{350+h} \times 57''$$

where a = atmospheric pressure in inches of mercury

z = observed zenith distance

$r = 57'' \tan z$

$57''$ = mean refraction at 45° latitude

h = temperature in $^{\circ}\text{F}$.

On the other hand the French formula gave:

$$R = \frac{\alpha(1+y)\tan\theta}{(1+.00375x)\left(1+\frac{x}{5412}\right)} + \frac{\frac{1}{2}\alpha^2(1+y)}{(1+.00375x)\left(1+\frac{x}{5412}\right)} \times \frac{(1+\cos^2\theta)\tan\theta}{\cos^4\theta}$$

$$- \frac{\alpha(1+y)}{(1+.00375x)\left(1+\frac{x}{5412}\right)} \times \frac{.00125254\tan\theta}{\cos^2\theta} - \frac{\alpha x \cdot .00375x \times .0125254\tan\theta}{\cos^4\theta}$$

where θ = apparent zenith distance

$.76(1+y)$ = atmospheric pressure in meters of mercury

x = temperature in $^{\circ}\text{C}$

$\alpha = 60''616$: "a constant quantity determined by a multiplicity of observations by Piazzzi and Delambre". (43)

Faced with the choice of which to use, it is not difficult to see why astronomers might have been tempted to chose Bradley's formula!

There is no need in this study to investigate the details of the derivation of each formula, but it is important to notice how the use of different tables could affect the reduction of observations. In his 1818 paper on parallax (which will be discussed in detail in section 4.4) Brinkley demonstrated how much a difference in the temperature of the atmosphere can affect the apparent displacement of a star, as computed with the two different formulae (44).

From Bradley's formula it can be seen that to obtain the displacement due to refraction at a temperature different from 50°F the multiplying factor used was $\frac{400}{350+h}$; according to Brinkley, the

equivalent multiplier used by the French was $\frac{500}{450+h}$,

the difference between the two therefore being $\frac{h-50}{2000}$.

Brinkley then applied this difference to his data for the star Procyon, the mean refraction (that is its refraction at 50°F and normal pressure, 29.6 inches of

mercury) of which he gave as 58". He chose Procyon because it culminates around noon in mid-summer and midnight in mid-winter, between which times the temperature could easily vary by as much as 40°F. The nett result of Brinkley's calculations was that the differences in zenith distance allocated to the star Procyon between French and British astronomers would be almost 0".6 at either end of a specified period of 6 months. The differences would also be in opposite directions at each of the two dates, so if the two positions were subtracted, as they would be if parallax were being sought, the difference would be almost 1".2. If that difference is considered in connection with the question of parallax - and Procyon was among the stars studied in the hope of detecting its parallax - it is obvious that 1".2 would be a very significant error, especially as parallaxes were generally believed to be of the order of one or two seconds of arc at most. This example was extreme, involving a temperature difference of 40°F, but a difference in temperature as small as 15°F could produce differences in the equivalent calculations of about 0".5, which is still a large error when parallax is being sought.

Although the French and the Greenwich tables were the most commonly used, they were not the only ones in existence. Piazzini used tables he had compiled from his own observations (45), and Brinkley himself had suggested a modification of Bradley's formula (see ref. 18). In 1814 he wrote a long paper on his method of allowing for refraction in his calculations (46). All of this demonstrates once more that there was little agreement among astronomers over the best means of reducing data.

The final effect which was usually considered by astronomers during this period was the proper motion of the stars; the main problem here was whether or not such motions were uniform. As with contemporary discussions of other astronomical variations, no consensus was reached during the early years of the nineteenth

century. As far as the question of parallax was concerned, the amount by which any proper motion might have varied was insignificant, but disagreement over possible variation in proper motion is a further illustration of the general state of uncertainty in the study of positional astronomy in this period.

There was, however, general agreement over one aspect of the reduction of data, and that was that all astronomers were delighted with the new method of "least squares" for the mechanics of reducing a mass of observations. As Laura Tilling has explained (47), this method of coping with a number of observational results was initially applied in a crude form by the French mathematician Legendre, and later perfected by Gauss and Laplace. In its barest form the principle of least squares states that

"the most probable value of any observed quantity is such that the sum of the squares of the deviations of the observations from this value is least". (48)

In other words, if $z_1, z_2, \dots, z_n$ are the observed zenith distances of a particular star, then the most probable value of the zenith distance z is found by making $(z_1 - z)^2 + (z_2 - z)^2 + \dots + (z_n - z)^2$ a minimum. In the simple case of a random number of repeated observations the most likely value turns out to be the arithmetic mean of z_1 to z_n .

However, the principle had more powerful implications than that, as astronomers of the early nineteenth century perceived. It could be used to give the correct number of equations, from a large number of observations, to solve for a given number of unknown quantities, making it then possible to find each of the unknowns. It is difficult to judge precisely how astronomers used the principle in their reductions, as they tended to quote results without saying anything more than that they were obtained using the method of least squares. However, every astronomer working after about 1805 used the method, and when they referred to it they always appeared to regard it as a great blessing.

Finally, before leaving the problems of reduction, one further notable feature of the work of all astronomers in this period must be considered. Each of them gave their results, particularly for astronomical constants, to at least two places of decimal and often to three or more. The figures came directly from the calculations and appear to have been included quite blindly without any concern as to whether they meant anything or not, or any understanding of significant figures. Perhaps this feature more than all others demonstrates the general lack of any idea of limits of accuracy among astronomers working during the early years of the nineteenth century.

4.4 The controversy between Brinkley and Pond

The extent to which astronomers' problems of instrumentation and reduction affected their attempts to measure parallax during the first quarter of the nineteenth century is probably best illustrated by a study of what has been called "perhaps the most celebrated controversy in the history of the search for stellar parallax" (49). That was the protracted dispute between the ~~British~~ Astronomer Royal John Pond and his counter-part in Dublin John Brinkley, over whether or not the latter had managed, as he claimed, to have measured the parallax of several stars. The dispute dragged on well into the 1820s but by that time it had dwindled into a succession of attacks and counter-attacks concerning the instrumentation at each observatory, and contributed very little to the solutions of astronomers' problems of precision. Almost all the material significant for any insight into the general state of positional astronomy was published prior to 1822 and it is those papers which will be considered here.

It was in 1814 that Brinkley wrote his first major paper on the parallax of the fixed stars, although his

interest in the subject certainly dated back as far as 1810 when he wrote a letter concerning the parallax of Vega to Nevil Maskelyne (see ref. 18). The 1814 paper was read before the Royal Irish Academy in May of that year and published in the 1815 Transactions of the Academy (50). In this paper Brinkley claimed that the observations he had gathered since the acquisition of his altazimuth telescope tended

"to point out a parallax in α Lyrae,
 α Aquilae, α Cygni, α Ophiuchi
 and some others". (51)

The overall impression given by Brinkley throughout the paper was one of diffidence: it was almost as if he feared to have measured the phenomenon. Obviously, he was very much aware of the controversial nature of his results; the values he claimed for what he called the double parallax (52) were all at least 2", and that for α Aquilae was as large as 5".5. He was also very conscious of the need to demonstrate that his results could not be explained by any other recognised means, and consequently at least half the paper was devoted to a description of instrumental errors and a qualitative account of his reduction of the observations. Unfortunately, like so many of his contemporaries, Brinkley did not give either the bare observations or the mathematical details of his reduction so it is impossible to work out precisely how he obtained his results. However, as well as a general discussion of the effects that he knew he had to take into account, which he gave in the introduction to his observations, he added a table of results for each individual star, and explained why the result could not be caused by anything other than parallax. Then as a general conclusion to his results he made the point that

"if parallax be not admitted, it must appear very remarkable that in no stars have annual changes of zenith distance been observed by this instrument that cannot be explained by a parallax. It might be expected that in some stars the changes would have been quite opposite to the changes from parallax". (53)

Despite his reluctance to emphasise the significance of his observations, therefore, Brinkley was obviously convinced himself that he had detected parallax. In his calculations he claimed to have considered factors as small and uncertain as the possible variation in the proper motion of Arcturus, and the possibility of misjudging the moment of transit as the star crossed the relevant wire within the field of view. He had obviously considered very carefully every possible explanation for his observations and either gave plausible reasons for rejecting them, or implied that such effects were too small to have any significance. Quite clearly he wished to forestall as much criticism as possible.

Brinkley also urged his contemporaries to help him in the search for parallax - to convince themselves of the truth of his assertions. He seemed particularly anxious for John Pond to devote his time to the problem, because he wanted to compare his own observations with ones made with an equally reliable instrument; he believed with some justification that his and Pond's telescopes "... ranked among the best instruments that [had] been constructed" (54). That Brinkley himself continued to seek values of parallax is clear, since less than a year after his paper was read in Dublin, he published an appendix (55), in which he provided further observations and recalculated the parallaxes of three stars (Vega, α Aquilae and α Cygni).

It was, however, two years later before he got the response he had requested from John Pond; then in 1817 the ~~British~~ Astronomer Royal produced two papers on the subject of the parallax of the fixed stars (56). Pond certainly turned to the problems of parallax as a result of reading Brinkley's work, and the opening paragraphs of Pond's paper were full of praises for his colleague's endeavours. Unfortunately for Pond, the new mural instrument erected at Greenwich in 1812 (57) was found to be unsuitable for parallax work (58). In addition Pond pointed out that he could not allow the new

instrument to be used exclusively for the hunt for parallax, so he set about devising an alternative experiment. His idea was to use two instruments fixed securely to stone piers, and to keep them exclusively for the purpose of parallax detection; one was to be directed permanently at the transit point of α Aquilae and the other at that of α Cygni, these stars both being prominent features of Brinkley's list.

At the time when Pond wrote his first parallax paper he had not yet managed to use the instruments for their specified purpose, although they had been erected. In the meantime, however, he wished to present certain observations made with the mural circle. The interesting point about Pond's discussion of these observations and their implications is that it demonstrates his complete misunderstanding of certain aspects of the geometry of parallax. He had noticed variations of the order of $\frac{1}{2}$ " in the position of three stars - Vega, α Cygni and α Aquilae, but despite being convinced that these variations were not the results of instrumental errors, he was equally sure that they were not evidence of parallax. He believed this because he did not:

"... find these discordances sensibly increased by direct comparison with an opposite star [to] α Aquilae as Capella".

He added that:

"... the maximum in parallax is nearly as great in Capella, as in α Aquilae, at least in the proportion of 4 to 5 [and] it is very unlikely then, that the parallax of the one star should be above 5", and the other an insensible quantity". (59)

By "opposite star" he appears to have meant a star with right ascension about 12 hours different from α Aquilae and so opposite with respect to the north celestial pole. The ratio Pond quoted is more difficult to understand because in order to make any claim about the relative parallax of two stars some knowledge of their relative distances from the Earth must be assumed.

The only way in which Pond's figure can be obtained is by assuming that α Aquilae and Capella lie at approximately the same distance from the Earth, and that Pond was referring to the way in which parallax is manifested in celestial latitude (60). But Pond did not say that he was considering celestial latitude - his observations were bound to have been of zenith distance and right ascension - and of course he had absolutely no grounds for assuming that the two stars were at similar distances from the Earth. Pond's theoretical grounds for doubting Brinkley were, therefore, highly suspect.

By the time Pond's second paper was written in June 1817 he had made observations with his specially erected telescopes, and he concluded that he could find no trace of parallax in any of the stars he observed. However, his data were gathered from a period of less than 6 months which is not really long enough when an annual effect is being sought. Moreover, unlike Brinkley, Pond did not describe even qualitatively how he reduced the observations, nor did he assess the accuracy of either of the instruments he used; it is difficult, therefore, to give a critical evaluation of his denial of parallax. Derek Howse described these instruments among "the minor astronomical instruments" of the Royal Greenwich Observatory (61); it appears that they were constructed in a hurry when Pond realised the unsuitability of the mural circle for parallax measurements and cannot, therefore, be included among the instruments of great precision of the period. It is thus clear that Pond had no genuine observational arguments against Brinkley's claims either.

Furthermore, considering again the theoretical content of each of Pond's 1817 papers, it must be pointed out that the specific method he used in his attempt to detect parallax was based on another geometrical misconception. He chose to compare the declination of the star in which he was interested with the declination

of a star whose right ascension differed as closely as possible by 12 hours from the first one. He believed that one of the stars reached its maximum in declination in one direction while the other reached its equivalent maximum displacement in the opposite direction. Thus if the difference in the declination between the two were measured, that angle would vary with time by the sum of the two separate variations. But in making this assumption Pond was grossly oversimplifying the geometry: his assumption is only true of certain stars lying on the solstitial colure, where the magnitude of the variation in declination is identical to that in celestial latitude (62). β Aurigae, the star Pond selected because it lies "opposite" α Cygni, does in fact lie very close to the solstitial colure, but α Cygni itself is nowhere near this great circle; by comparing the differences between their respective declinations there was actually a danger that the two possible parallactic effects might cancel out of the calculation altogether (63).

There are advantages in measuring differences in angle when seeking very small parallactic effects, but generally speaking this is only so if the angle measured is a direct one between two stars lying in the field of view of the telescope at the same time, as explained in section 3.4 in the discussion of Herschel's choice of double stars. Choosing to compare two stars of opposite right ascension however meant that Pond lost most of these advantages. He would have had to calculate all physical effects separately for each star, the most significant calculation being that for refraction at different temperatures, particularly when one star transited at midnight and the other at midday. Pond's new approach to the parallax problem was therefore not especially useful.

Despite these possible criticisms of Pond's work, Brinkley did not dismiss it out of hand. In his next paper on the subject of parallax, read to the Royal

Society (64), Brinkley outlined certain of his earlier researches (for the benefit of those Fellows of the Royal Society who might not have seen his work published in the Transactions of the Royal Irish Academy) and claimed he had "... been able to obtain some results ... which appear to coincide with [his] former ones as to α Aquilae in a remarkable manner" (65). But he did not, just because of this, assume that he was right and that Pond's work had no validity. His attitude was rather that they should both pay greater attention to instrumental errors. If his own results were caused by such errors it was most important that the reason should be found, although he had no idea what it might be. He was even more worried by the possible instrumental errors of the Greenwich mural circle, particularly because Pond measured stellar positions relative to certain supposedly standard points, which of course must have been ascertained from observations made using the mural circle. In this paper Brinkley exhibited again his very clear awareness of the problems associated with the determination of parallax; and again he listed carefully every possible source of error, this time allocating a section of the paper to each, and paying particular attention to the problem of refraction (see above, section 4.3).

Of course, it was not merely a matter of knowing which factors would affect the apparent position; he also had to ascertain the extent to which each factor contributed to the apparent displacement. Moreover, and perhaps of even greater importance, the degree of accuracy of any formula used in the reduction of observations needed to be known. It was all very well to claim to have allowed for something, but when such small angles as those caused by parallax were being considered, the errors resulting from inadequate formulae might have been as big, if not bigger, than the actual angles sought. Brinkley pointed out that neither the constant of aberration nor that of nutation was known to an accuracy greater than $\frac{1}{2}$ " and,

of more significance, two different formulae for refraction were currently in use, and they could render results differing by more than 1" for parallactic values (as shown above, section 4.3).

From his list Brinkley hoped to expose the possible sources of error in any catalogue of stars which might be used as standard. At this stage of his argument he was not condemning Pond's actual work on parallax; rather, he said that he considered the Greenwich observations to be

"... inconclusive as to the existence or non-existence of parallax, merely from the uncertainty of the elements used in the reduction, not from any errors of the observations, or from any defects in the construction of the instrument". (66)

Elements used in the reduction would already have had some effect on the "standardised stars", so by comparing new positions with them there could be as much as double the error in the final answers. Brinkley did point out ways of avoiding large errors in the first place - for example, selecting only stars near the zenith as standards to avoid the major problem of refraction - but that did not alter the fact that Pond had already used his standard stars for comparison. Brinkley's aim now was to devise a new programme for further research; this was far more important than wasting time on what had already been shown to be inconclusive evidence. He certainly felt it was vital that Pond's work was continued.

Apart from showing that Pond's work did not disprove the existence of parallax, Brinkley gave new sets of observations which further supported his claims. His results for parallax this time were: α Cygni 1".56, α Aquilae 5".06, γ Draconis: a negative result, and α Lyrae 1".32. But he also suggested that he was less happy with certain of his more recent work. Near the beginning of the paper, after complaining that the previous two years (1816 and 1817) had been very unfavourable for astronomical observations, Brinkley

wrote of his results for α Lyrae and Arcturus that they had "not been so uniform as [he] had expected from [his] former observations" (67). As he had in his first long paper on parallax, Brinkley was still displaying great caution, and not over-emphasising his possible achievement.

Pond's reply was read to the Royal Society a matter of weeks later, on April 16 (68). In this he discussed his observations of α Aquilae which he had made using the second fixed telescope. Initially he had intended to proceed with his work on α Aquilae in a similar way to that on α Cygni, but he was thwarted in this because the "opposite" star he chose was β Canis Minoris which was too faint to be seen during daylight hours. At this point Pond discounted his criterion that the best pair of stars to be observed were those with opposite right ascensions, and looked for stars within a certain angle of α Aquilae such that they would always be visible at transit; he eventually chose the star 1 Pegasi, which was apparently "... much better suited for the purpose" (69). His basic idea was still to observe the variation in the difference between the declinations of these two stars and, having observed for a period of less than 6 months he reduced only the data from the days when he managed to obtain observations of both stars. He was obliged to restrict himself to those because he was still unsure of the stability of the instrument. He was, however, convinced that there could have been no significant alteration in the position of the telescope in so short a time as the three hours between the transit times of α Aquilae and 1 Pegasi. About his results he wrote that he could

"discern no appearance of parallax in the above observations, and indeed [he had] long considered it as a hopeless task to establish its existence by observations on a star so far from the zenith". (70)

As α Aquilae was the one star for which Brinkley was consistently obtaining a high value for its parallax, by saying this Pond was beginning to bring the dispute

between the two men to the surface. Further on in the paper he stated his disagreement with Brinkley more clearly. He admitted that it was Brinkley's recent paper (that read in March) which had persuaded him to look closely at his observations of α Aquilae; he was a little resentful of Brinkley's insinuations that the Greenwich data might not have been properly reduced and decided, therefore, to study the observations from his second fixed telescope, in an attempt to settle the vexing question of the parallax of α Aquilae. His conclusion was that he had

"the satisfaction to state, that [he found] the observations of α Aquilae, already made quite sufficient to establish this important point, namely that the parallax of this star [was] either an insensible quantity, or [was] so extremely small, that it [could not] possibly have had any share in producing the discordances observed by Dr. Brinkley". (71)

Unfortunately, although Pond objected that Brinkley's remarks about the reduction of the Greenwich observations were made because Brinkley did not know the details of Pond's analysis, he (Pond) still did not reveal them. Hence it was, and still is, impossible to say whether Brinkley was justified or not.

Immediately following Pond's paper on α Aquilae in the Philosophical Transactions was a second paper by him entitled "On the parallax of the fixed stars in right ascension", which was read to the Royal Society on May 28 1818 (72). Oddly enough, this paper appears to have been prepared quite separately from the first, and to be independent of the dispute with Brinkley. Pond's decision to seek parallax in right ascension as opposed to declination was apparently taken as early as 1806 after he had read an account by Delambre of the work of the Italian astronomers Calandrelli and Piazzzi, in the Connaissance des Temps for 1808 (73). Piazzzi and Calandrelli both believed they had detected parallax in declination and Delambre urged that their results be verified by finding parallactic variations in right

ascension. According to Pond, he had been prevented from making suitable observations by "accidental circumstances" and the passage of time had not encouraged him, because as time passed the upper limit of the possible value of parallax had continually decreased. He was not helped by the lack of suitable equipment or by the necessity to devote his time to other problems; but finally it occurred to him that all he need do was to use observations of the right ascension of stars which he had measured initially for other purposes, and scrutinise them in the hope of detecting parallax. The stars he chose were α Aquilae, Arcturus, Capella and α Lyrae. From his observations of these stars, however, he could only conclude that the major axis of the parallactic ellipse could not exceed $0''.5$. Then, at the very end of the paper, he added a remark concerning his work on α Cygni, the subject of his second paper in 1817. He explained that his

"observations of α Cygni, continued in the manner described in a former paper, [confirmed], in the most decided manner, the total absence of any observable parallax..." (74)

There was no reference at all to Brinkley or his work, and this paper was Pond's last published contribution to the subject of parallax for over four years. When he did publish anything further, it was only to defend himself against the criticisms which Brinkley continued to make, and his papers had little significance for the overall development of the problems of parallax (75).

On the other hand, Brinkley continued to interest himself in the problems and continued to worry about the accuracy of his instruments and how best to improve it. He was not just concerned about obtaining angles of parallax; in 1819 he wrote about a different small angle: the supposed seasonal variation in the obliquity of the ecliptic. The inspiration to work on such a subject came from his study of F. W. Bessel's

Fundamenta astronomiae, containing a comprehensive and thorough analysis of the Greenwich observations made by James Bradley in the mid-eighteenth century (76). One of the results of Bessel's work was that, contrary to the generally held opinions of astronomers in the early nineteenth century, Bradley's observations, when properly reduced, showed that the value of the obliquity did not change with season. After reading this Brinkley looked at his own observations and, having compared them with contemporary observations made at Greenwich, concluded that astronomers at both observatories should try to prepare:

"an extensive series of observations of many stars, referring each to the apparent zenith distance ... [since] ... conclusions as to the existence or non-existence of parallax, from comparisons of the relative places of stars taken indiscriminately, must be liable to much uncertainty, whether the comparisons be made by polar distances or right ascensions". (77)

This indeed was what Brinkley did, and two years later he published yet another paper on parallax in which he reiterated, with a confidence never previously assumed, his claims to have measured parallax (78). His confidence led him to make a blistering attack on Pond's work, backed up with his usual careful attention to the assessment of errors. It was perhaps at this point that the controversy between the two astronomers dwindled into little more than bickering with almost nothing emerging which was of relevance either to positional astronomy in general or to the question of parallax in particular. In his 1821 paper Brinkley made an attempt to cope with the effect on his instrument of changes in temperature, and he clearly understood where the problems lay. He did not, however, have any real means of solving them. Throughout the remainder of his time as Astronomer Royal for Ireland (79), he continued to be interested in parallax (80) and continued to believe that he had detected the phenomenon, but he was never able to convince his adversary: the dispute was never settled (see appendix 3),

it was merely forgotten.

The important question to be answered now is whether or not the controversy contributed anything to the history of attempts to measure parallax and, if it did, what form did any contribution take? The nineteenth century historian of astronomy Robert Grant made the first assessment of the dispute. He wrote of the controversy that:

"if it did not lead to the object originally contemplated by Brinkley, [it] was not unproductive of advantages of great importance, in so far as the ultimate accomplishment of that object was concerned; since it had the effect of placing in vastly clearer light than hitherto, the influence of various disturbing causes which go to complicate the observations of astronomy, and which completely efface the delicate variations arising from the parallax of the fixed stars, unless rigorously taken into account". (81)

The most recent but exaggerated account of the dispute is given by Susan McKenna, Brinkley's biographer in the Dictionary of Scientific Biography, who regards the controversy as

"of great value in stimulating the study of previously unappreciated factors affecting the measurements. Brinkley's results, although now discredited, thus led to the later successful detection of stellar parallaxes". (82)

It is certainly true that Brinkley knew which factors had to be considered, and that he worked very hard to devise formulae which he felt allowed adequately for each. That Pond was aware of the accuracy which was required is also clear, but whether or not he attained such accuracy is not. Awareness of the need for accuracy was not in itself enough to produce acceptable values of parallax. Brinkley **deserves** acclaim for recognising and clarifying many of the problems associated with the detection of parallax, but not for solving them. By the time the dispute petered out, there was still no general agreement as to the exact formulae for the reduction of data.

However, despite this perhaps disappointing conclusion, the controversy does have a role in the history of attempts to measure parallax because it is illustrative of all the problems existing in positional astronomy during the first quarter of the nineteenth century. It was not a dispute in which either man was proved right (83); it was one in which the overwhelming difficulties of the subject were exposed and they, if anything, were the victors.

4.5 Conclusions

It is clear that throughout the period under discussion astronomers were eager to continue to improve the methods and practice of astronomy with the positive aim of making it a more precise study than ever before. During this time great steps forward were taken with the introduction of the circular instrument on the practical side, and with the work mainly of Laplace on the theoretical side. It cannot be said that nothing of any significance or value was done in the early years of the nineteenth century; far from it. But the following point remains: until at least 1818 almost all astronomers were still struggling with the concept of degrees of accuracy and as a result there was conflict over the evaluation of astronomical constants. Not until the work of Brinkley was there any real grasp of the concept that the value of a constant could be known within specific limits only, and of the implications this cast on the status of the results obtained after the reduction of the data. But even in Brinkley's work the problems were not solved; his importance lay in his recognition and clarification of the problems. This assessment of the role of Brinkley in the history of stellar astronomy does not detract from his achievements in any way; on the contrary Brinkley's work has in the past been underestimated, if anything. In general he is only mentioned in connection with the protracted controversy with Pond over the question of parallax, but it is clear that

he contributed far more than that during the 30 years he spent as Astronomer Royal for Ireland. Perhaps he was unfortunate in that he was succeeded in his post and in his particular interests by such outstanding figures as William Rowan Hamilton and Friedrich Bessel.

As for the problem of the detection of parallax, in the light of our present knowledge of the largest parallaxes ever to have been detected - about $\frac{3}{4}''$ - it is obvious that it would have been impossible for any astronomers to have measured the phenomenon during the first two decades of the nineteenth century. By 1820, as Allan Chapman has shown, instruments were technically accurate enough to show a difference in stellar positions of $\frac{3}{4}''$; hence accurate instruments in themselves were insufficient to demonstrate parallax, and the lack of measurements cannot be explained wholly in terms of the state of the instruments. Astronomers failed to detect parallax because they could not calculate with any certainty how much of any discrepancy could be ascribed to which phenomenon. The first important breakthrough was to come with Bessel's Fundamenta Astronomiae in 1818, but even that, combined with highly accurate instruments, did not provide the formula for success. Further refinements in both theory and practice were needed, and it was to be another 20 years before success was achieved.

But still astronomers persisted: they continued to search their data for traces of parallax. During the first quarter of the nineteenth century the number of publications in which the problems of parallax were discussed exceeded the total number from the period 1660 to 1800. Astronomers from observatories as far apart as Greenwich and Palermo, or Dublin and Pulkowa, and many in between, all devoted some of their time to these problems. This should not be seen as an oddity on any of their parts, however. All aspects of stellar astronomy continued to grow in importance through the nineteenth century and parallax was far from being the only stellar phenomenon that was considered. Every other

effect mentioned in this ^{chapter} ~~paper~~ caused a good deal of interest within the widespread attempt to perfect even further the subject of positional astronomy. Moreover, by the early nineteenth century, William Herschel had become the grand old man of astronomy and although as has been suggested by certain historians (84), he and his work were perhaps misunderstood by almost all his contemporaries, his endeavours to come to grips with the three-dimensional structure of the universe were not completely lost on them. With Herschel's work as the standard it became perfectly acceptable to show interest in the stars as individuals, even if the interest was very far removed from Herschel's own. One of the inevitable manifestations of interest in the "fixed stars" at major observatories was that astronomers there went to great lengths to establish their positions as accurately as possible. It was inevitable because it was the job of astronomers at such observatories to plot celestial positions; when they turned their attention to the stars their first thoughts would be bound to be an extension of the type of work in which they were already well versed. The fixed stars provided a vast new realm in which the achievements of astronomers could be tested, and it was at this stage that the possibility of detecting parallax became significant. A definite measurement of parallax, well substantiated by reliable observations, would have been a sure indication of the many improvements being made at that time. Thus, every astronomer of note was trying to prove that it could be done. Unfortunately, owing primarily to the vexatious question of the reduction of data, final success was still a generation away.

Chapter Five

THE EARLY ROLE OF F. W. BESSEL

5.1 Bessel at Lilienthal: 1806 - 1810

The previous chapter showed that the early years of the nineteenth century were years of great uncertainty as far as precision in positional astronomy was concerned. The period was also one of immense activity on the parts of those astronomers who were trying to eliminate that uncertainty; and the man who was most successful in his attempts to overcome all the problems discussed in Chapter Four was the German astronomer Friedrich Wilhelm Bessel. Bessel was of course one of the three astronomers who managed, in the late 1830s, to measure the annual parallax of a star (see below, section 6.4). His interest in the subject, however, dated back at least to 1806, and in fact it was during the early years of his career as an astronomer that much of the vital groundwork for this achievement was accomplished.

Bessel's early life is fairly well documented and a number of accounts of it are available (1). His first important contribution to astronomy was his calculation of the orbit of Halley's comet, which he sent with a letter to H. M. W. Olbers (2), who, despite being a doctor by profession, was an important figure within the established astronomical community. Olbers sent Bessel's work to be published in the Monatliche Correspondenz (3), edited by Xavier von Zach, one of Olbers's many correspondents. Bessel had gained his first and most important patron: he and Olbers remained

firm friends for the rest of Olbers's lifetime. It was at Olbers's recommendation that Bessel was given his first appointment in astronomy when, in 1806, he accepted the position of assistant at Johan Schröter's private observatory in Lilienthal. It was also to Olbers that Bessel sent the results of his first investigation into the question of annual stellar parallax. In a letter written on 30 October 1806 (4) Bessel discussed results which concerned a possible measurement of the parallax of the star α Herculis among others, presented four years earlier by his new employer Schröter (5).

Bessel's main point in this letter was that he believed - rightly as it turned out - that Schröter's values of parallax for certain double stars were spurious. After examining the observations he felt that nothing conclusive could be said with respect to parallax, principally because of the uncertain observational errors (6). In itself the letter is not remarkable: it did not show any new insight into the problems or methods of parallax detection; but it does demonstrate that Bessel showed interest in the question far earlier than has been assumed in the secondary literature (7). It is hardly surprising that Bessel should have shown such an interest: the question of parallax was certainly under review in more than one astronomical centre at about this time. In 1805 the Italian astronomer Giuseppe Piazzi had published a series of observations which he believed showed signs of parallax (8), and it is very likely that Bessel had seen this certainly after he started to work at Lilienthal, if not before. That he was familiar with at least some of the contemporary German literature in astronomy is evident from his early letters to Olbers, and this literature contained references to parallax and its detection (9).

The letter to Olbers was not published until 1852, and it may be regarded perhaps as of little importance.

However, it is far from the only reference that Bessel made to parallax before 1812: the earliest date given for Bessel's interest in the phenomenon in any historical account of parallax. Early in 1807, in a further letter to Olbers (10), he referred to Lalande's reduction of stellar positions in which the latter made certain assumptions about parallactic angles with which Bessel could not agree. In this letter Bessel seemed to be more concerned with producing accurate and reliable stellar coordinates than with the question of the detection of parallax for its own sake; but whatever his reason for looking into the problems - and his reasons changed during his career - it is evident that he was looking into them.

Evidence of Bessel's interest in parallax also appeared in print early in his career. In the Monatliche Correspondenz for 1809, two pieces concerning stellar parallax written by Bessel were published: the first about whether or not the existence of annual parallax was indicated in his work on the reduction of James Bradley's observations made between 1750 and 1762 (11), and the second concerning the formula for parallax in right ascension (12). In fact during the years 1808 and 1809 Bessel clearly gave the problems of parallax some thought, corresponding on the subject with Olbers in Bremen, Gauss in Göttingen, and von Lindenau, who ran the Seeburg observatory near Gotha. From the correspondence with Olbers and with Gauss it is possible to piece together some of the background to the publications. After the two letters to Olbers concerning parallax mentioned above (refs. 4 and 10), the subject next definitely occurred in their correspondence in a letter from Bessel written in May 1808 (13). In this letter, Bessel thanked Olbers

" ... for the communication of the very easy method for reducing parallax in right ascension to a series. [The] formula, which contained the longitude of the Sun [was] very convenient ..." (14)

At first glance it seems that the letter to which Bessel must have been replying is lost, as the volume of correspondence is arranged chronologically, and no such letter occurs before this one from Bessel. However, a few pages further on in the edition (prepared some years after Bessel's death by A. Erman) is an undated letter from Olbers to Bessel, containing a formula relating parallax in right ascension to the longitude of the Sun (15). It was that same formula that Bessel quoted in a postscript to his paper on parallax in right ascension the following year (16), and the formula is to be found in none of the extant letters in the correspondence. It appears therefore that the undated letter belongs slightly earlier in Erman's volume.

Working from this assumption and from other printed evidence then, the sequence of events leading to the publication on parallax in right ascension was as follows. Bessel received a letter from Olbers with the latter's formula sometime before May 1808 which he acknowledged in his letter of 29 May that year. Then in late 1808 or early 1809, Bessel wrote to Lindenau on the subject, having presumably derived his formula in the meantime. The letter to Lindenau, or at least part of it, was published in the Monatliche Correspondenz for March 1809 (17). After writing to Lindenau, but before the extract was published, Bessel wrote again to Olbers on the subject of parallax in right ascension, this time with particular reference to the observations that he possessed of the Pole Star (18). He had not been able to detect any parallax for that star and he did not return to this specific subject for several months.

The published letter was important for the development of work on parallax: it was not only an early example of Bessel's extremely neat ways of dealing with problems of spherical geometry, it also marked the beginning of the foundation in rigorous theory of attempts to measure parallax. Before this time, attempts to identify

parallax - either relative or absolute - generally consisted of scanning lists of data from particular stars to see if there were any difference in their positions at either end of a specified period of six months. Of course, a theory depending on the coordinates of the star was needed before the relevant times of year for that particular star could be identified, and such a theory was certainly used by Jacques Cassini, at least as far back as 1702 (19). But not until the work of Bessel was there a rigorously derived formula showing how parallax in a given coordinate system (Bessel generally worked in equatorial coordinates in common with most of his contemporaries) varied throughout the year. It was the way in which Bessel's observations fitted the theory over the cycle of a whole year, among other things, which was decisive in the favourable assessment made by astronomers all over Europe of his work on δ Cygni in the 1830s. And Bessel began making his way towards the necessary rigour very early in his career. The work on parallax in right ascension was just one example of this search for rigour, resulting from his dedication to the production of a catalogue containing astronomical positions which were as accurate as they possibly could have been, from the observations made by Bradley about half a century earlier.

The other paper written about parallax by Bessel and published in 1809 (in fact published a little earlier than the paper on parallax in right ascension) was also an extract from a letter, written to Lindenau on 5 January 1809. But before Bessel wrote to Lindenau he sent his results to Olbers, writing on 15 December 1808 that " ... the investigation into the parallaxes of Sirius and α Lyrae is now finished" (20), suggesting that it had taken some time. In the published paper Bessel gave his justification for seeking parallax in right ascension, rather than in any other coordinate: the values of right ascension that he was using, those made by Bradley, were taken from a highly accurate clock.

He perhaps felt the need to justify his use of measurements of right ascension because it had been the general practice of astronomers for over a century to study observations of declination, or of zenith distance (easily converted to declination) when trying to detect evidence of parallax. This paper contained also an early example of a new method of selection of stars to be studied, and of how the analysis should be undertaken. Bessel chose to calculate the difference in right ascension between two stars lying approximately 12 hours apart, and to see how, if at all, this quantity varied through the year. From his letter it is apparent that the use of right ascension and the particular method that he favoured had both been suggested previously by Lindenau, as Bessel wrote that, as Lindenau had suggested, for his investigations he had

"made use of the right ascensions and [had] compared with each other such stars as lie 12 hours apart, whereby one always [obtained] the sum of the two parallaxes ..." (21)

The final remark in this quotation is of considerable interest and some relevance to the history of attempts to measure parallax, because it touches on the question of how parallax is exhibited in different coordinate systems. This issue had previously been a source of difficulties to John Flamsteed in the late seventeenth century (see Appendix 1), and was to be again to John Pond later in the nineteenth century (see section 4.4 above). Bessel (and presumably Lindenau as Bessel appears to have obtained this particular idea from his colleague in Gotha) was correct in saying that by studying the differences in right ascension of two stars lying 12 hours apart one would thus by studying the variation of the sum of the two parallaxes, and he presumably reached his conclusion from a consideration of the geometry. The idea of comparing the position of two stars was not new: it was precisely that method which was used in attempts to measure relative parallax

(22), but in that case it was a matter of comparing two very close objects, not two lying as far apart as possible along any particular coordinate. Also, the natural coordinate to compare when seeking parallax from observations of differences at different times of year would be celestial longitude, if only the geometry were considered. This is because the variation of parallax in celestial longitude with time is as straightforward as it can be, depending only on the sine of the angle measuring the difference in longitude between the Sun and the star in question (23). However, it was observations of right ascension rather than of longitude that astronomers possessed at this time, and this must have influenced their choice of coordinate. To have selected in particular stars lying as far apart in right ascension as possible shows good insight into the theory of parallax, as Bessel stressed that by this method one always obtained the sum of parallaxes. This was the important feature because his argument was that, even if individual parallaxes were too small to detect, the sum of the two might just be discernible. It is true that computing the difference in right ascension between two stars so situated will always render the sum of the two parallaxes in right ascension at the time of observation, but as parallax in this coordinate is governed by a more complicated geometrical rule than is parallax in celestial longitude, this might not appear to be obvious at first glance. [That it is true is shown in Appendix 4]. On the other hand, intuitively perhaps, stars 12 hours apart are on 'opposite' sides of the sky with respect to the north celestial pole, and so should be expected to behave in opposite ways, one reaching its maximum value of right ascension when the other reaches its minimum value. However, although, as pointed out above, this is so for parallax variation in right ascension, it is not so for the behaviour of parallax in declination for two stars 12 hours apart (24). Bessel could only have reached his conclusion from a careful study of the geometry of the apparent

parallactic shift.

Whatever Bessel's reasons for choosing which stars to study, he certainly contributed a new factor for consideration in the difficult issue of selection criteria (25). But when he had to decide precisely which observations to reduce he chose to compare first Sirius and Vega, then Procyon and Altair. In making such a choice it might be thought that he was guided by the criterion most commonly adopted, that of brightness. However, his selection was in fact governed by the number of observations that he possessed of each star; in his letter to Lindenau he was keen to stress that he worked on stars for which he had a sufficient number of observations to make his results acceptable. Indirectly, of course, that meant he was bound to end up with the brighter stars as they were the ones most commonly observed.

Unfortunately, however, having done the extensive reduction of over 400 observations, Bessel had to conclude that there was absolutely no evidence for the parallax of Sirius or Vega, and only a very tentative possibility of the phenomenon's being exhibited by Procyon and/or Altair. He had reached this conclusion by the time he wrote to Olbers in December 1808; by the time he wrote to Lindenau, some three weeks later, he had also studied more than 250 observations of the Pole Star, but he was obliged to say that he had not been able to observe "the slightest trace of a parallax therein" (26). Following his additional work on the Pole Star he wrote again to Olbers to inform his friend of his lack of success (27); then early in February 1809 he wrote to Gauss giving the details of his investigations (28). The letter to Gauss was the last that Bessel wrote for some time on the subject of parallax, but in it Bessel showed his familiarity with current work in that field by stating categorically that the results of the Italian astronomers Piazzzi and Calandrelli for the parallaxes of various stars were too large (29). This letter was the last to refer to

the question of parallax before Bessel moved from Lilienthal in 1810, firstly to supervise the building of a new observatory at Königsberg and finally, in 1813, to become its first director (30).

After this early work by Bessel on the problems of parallax detection there is no evidence that he gave the subject serious consideration again until 1812, or possibly late 1811, so before proceeding to a discussion of his revived interest, an assessment of his earliest work on the question should be made. The first point to stress is that parallax was certainly not the only interest Bessel had during his years at Lilienthal. In the third volume of his collected works there is a list of all his publications (31), and those for the years 1804-9 (of which there are over 50) cover topics varying from the type of micrometer that he used, to observations of eclipses and papers about the phenomena of aberration and nutation. Judging from this list, his most extensive study during this period was that of cometary orbits: about half his published works were concerned with observations of comets and the reduction of such observations. This view of Bessel's interests is borne out by a consideration of the correspondence between Bessel and Olbers and between Bessel and Gauss; at least half of Bessel's letters to each touched on the subject of comets, and many dealt exclusively with the subject. Other branches of astronomy in which he was clearly interested included the newly discovered minor planets Ceres, Pallas, Juno and Vesta (32), the shape of the planet Saturn, and lunar occultations. In other words Bessel was busily establishing himself at the forefront of astronomical research: he was in direct contact with other German and French astronomers, and through them, though principally through Olbers, he learned of contemporary work being carried out in England. Because of his contacts he was aware of the many problems facing astronomers during this period and was continually

striving to provide adequate solutions. Inevitably then he would have encountered the problems of parallax determination and, being a particularly gifted mathematician and astronomer, he was able to contribute to this enquiry as well as to all the others in which he showed an interest. Admittedly he did not produce any values of parallax from his early investigations, but he gave convincing evidence, particularly in his comparisons of Sirius and Vega, of the smallness of the angles being sought, thereby rendering the work of the Italians Piazzzi and Calandrelli open to further doubt than that already expressed. But his most important achievement at Lilienthal with respect to the question of parallax detection was his derivation of the formula for parallax in right ascension, as this was one of the first steps that he took towards providing a comprehensive theory of the apparent motion resulting from parallax, which was to have such significance in his eventual identification of the phenomenon.

5.2 The study of 61 Cygni: 1810-1816

Although, as pointed out above, Bessel did not consider in depth the problems of parallax detection for some time after his early papers, he did not ignore the subject completely. Writing to Olbers in April 1810 he gave his views of the claim made by Lindenau that the latter had measured a parallax of 3 or 4 seconds for the star μ Cassiopeiae. Bessel was not convinced; when he gave an account of his colleague's work he wrote that

"Herr von Lindenau has observed a parallax for μ Cassiopeiae; he has 14 observations which give him 3-4 seconds, but I have derived only 1!18 from them. You see that the number of observations is too small to be able to calculate the result to that degree of certainty". (33)

Bessel was undoubtedly only led to think of the question of parallax as a result of having seen Lindenau's work

(34). By this time he was preoccupied with the problems of the new observatory and was unable to devote as much time to parallax as he might perhaps have wished. Nevertheless, he was perfectly prepared, even anxious, to comment on Lindenau's work, and it is important that his main complaint was that the number of observations was too low. Already he was setting new standards within the study of positional astronomy, by insisting on the need for a large number of observations if astronomers were to produce significant results. It was this insistence which had guided his choice of which stars from Bradley's data to study in the hope of detecting parallax, and the sheer quantity of data presented in his papers of 1838 announcing the value of parallax for 61 Cygni was a significant factor in the acceptance of that work.

After the letter to Olbers in which he commented on Lindenau's work, Bessel was silent on the subject of parallax for over two years. His main commitment during this time appears to have been the reduction of cometary data, which he received for the most part from his correspondents. Obviously while the new observatory was being built - in the years 1810-14 - this would have been his only source of new data. Perhaps he would have written on parallax during this period had any of his contemporaries brought the subject to his attention.

It was in a paper published in August 1812 that Bessel returned to the question of parallax determination, with particular reference to the parallax of double stars (35). Although the paper was not concerned for the most part with the question of parallax, Bessel devoted several pages at the end to a discussion of the phenomenon. His discussion is of historical interest in that it centres on the method of estimating parallax by dynamical means (36). The paper consisted mainly of a detailed study of past observations of the double star 61 Cygni, together with Bessel's reduction of them. He was interested in this particular star partly because

it showed convincing evidence that its two components did not merely appear to lie close together, but did so in reality, and partly because the evidence - that the two stars shared a large proper motion - was interesting in itself. Bessel began by repeating the by then well known argument that there are more pairs of stars lying close enough together to be classed as double stars than was predicted by simple probabilistic arguments applied to a random distribution of basically similar stars (37); the implication of this was of course that most pairs of stars lying close together formed physical systems or binary stars (38). This was not Bessel's primary concern, however: his 1812 paper was written with the intention of showing what might be learnt from 61 Cygni in particular about the realm of the fixed stars. Before giving his analysis of observations of the star Bessel wrote that

" ... this remarkable pair of stars ... is worthy of the very attentive consideration of astronomers in that it can lead [them] to interesting conclusions about the structure of the fixed stars". (39)

Bessel followed this with a study of observations of the star dating back to those made in 1659 by Hevelius; his discussion also included other observations made by Flamsteed, Lalande, Piazzzi and many made by Bradley. The remarks he made concerning each set of observations and the instruments used to obtain them show that he had a masterful understanding of his predecessors' problems of reduction, but the most interesting part of this paper as far as parallax is concerned is Bessel's discussion of the purpose of measuring the parallax of this star. Addressing his fellow astronomers, he wrote

"It will pay us to observe the annual parallax of this pair of stars, ... thus we will be able to calculate from that the sum of their masses". (40)

To support his claim he quoted the following formula relating parallax to the sum of the two masses:

$$\tau^2 \mu = \left(\frac{a}{\pi} \right)^3$$

where a = semi-major axis of the ellipse of rotation of the binary,

τ = period of revolution in sidereal years,

μ = sum of the masses in solar masses,

π = annual parallax.

Clearly, if a , π and τ were known, so was μ (41).

Having made this point, Bessel then neatly turned the problem around into one where, from certain assumptions, the value of π could be estimated. Re-writing the equation in the form

$$\pi = a (\tau \mu)^{-3}$$

to calculate π he needed to know, or to be able to estimate, the other three parameters. Given a sufficiently long period of time both τ and a are measurable; Bessel gave them values of 400 sidereal years and 25" respectively, the second of which is very accurate by modern standards, although τ for 61 Cygni is still not known with any degree of accuracy (42). The interesting step taken by Bessel was to assume that the sum of the masses could be set equal to the mass of the Sun, in which case $\mu = 1$ and $\pi = 0".46$. By assuming the value of the mass of the Sun for μ it might be thought that Bessel was, in a way, returning to the long-established working model of the universe consisting of thousands of basically similar stars, even though he was not using this arrangement in the straightforward way adopted by his predecessors. But what, in fact, he was doing, was attempting to estimate an upper limit for the parallax of 61 Cygni. By allocating a plausible value to μ , he was able to carry out an order of magnitude calculation giving him some idea of the size of the angle he needed to seek. Bessel nowhere suggested that 0".46 was the value of the parallax of

61 Cygni, but he did say that it was likely that the angle sought was in the region of this figure. Furthermore he pointed out that binary stars with larger values of a and smaller values of τ and μ would yield higher results for π . In other words suitable candidates for observation were those binary stars with a high ratio of a to τ , which was to become one of the three main selection criteria attributed to F. G. W. Struve in 1837 (43). (See below, section 6.3).

The other very important fact about this star was, as Bessel had established beyond doubt in this paper, that 61 Cygni had a curiously high proper motion. Curious because a high proper motion suggested that the star was near us, yet 61 Cygni is not a very bright object, being only just visible to the unaided eye. However, unlike so many of his predecessors, Bessel seemed perfectly prepared to accept a criterion other than brightness as an indication of nearness, and he urged his fellow astronomers to make the necessary careful observations, in an attempt to measure the star's parallax. He thought the endeavour would be worthwhile despite the fact that 61 Cygni is displaced most in right ascension as a result of parallax early in May, when its transit cannot be observed because it occurs during the day. However, with very careful observation Bessel believed the parallax of 61 Cygni could be measured.

By 1812, therefore, Bessel had demonstrated a sophisticated and complete understanding of annual stellar parallax. He had already decided what type of star, indeed which particular star, to concentrate upon (he always returned to 61 Cygni when he was contemplating the problems of parallax), he had produced mathematical formulae to describe parallax, and he had selected the coordinate in which to work. By this early date he had obviously not been able to detect parallax, but, unlike most of his contemporaries, he had not claimed to have done so; all he had claimed

was that the parallax of δ Cygni might be of the order of half a second, and that with careful observations made with particularly well constructed instruments this tiny annual variation might be detected.

During the following two years Bessel was silent on the question of parallax and its detection, although in February 1813 he wrote to Lindenau on the subject of the right ascension of δ Cygni, commenting on the high standard of the latter's observations of that star (44). Bessel was unable to make observations himself during this period as he was still awaiting the completion of the Königsberg observatory, but he was nevertheless very busy, working partly on any observations sent to him by his colleagues at other observatories, and to a larger extent on his long-term project: the reduction of Bradley's data. He continued to correspond with Olbers and with Gauss, but from this correspondence we can see that his letters were less frequent between mid-1812 and mid-1814 than during the periods before and after these dates (45). It is also clear from the contents of astronomical journals that during this time no other European astronomers showed any interest in the possibility of detecting parallax either: there was definitely a lull in activity as far as parallax was concerned throughout the two years from the summer of 1812 to that of 1814. The emphasis at observatories appears to have been on the study of comets and of asteroids and this trend is certainly reflected both in Bessel's published work and in his correspondence.

Astronomers' silence with respect to parallax was not broken until May 1814 when John Brinkley's paper, in which he claimed to have measured the parallaxes of several stars, was read to the Royal Irish Academy (46). However, this paper appears not to have been known on the Continent for at least two years. In 1816 it was translated into French by Delambre (47), but it

appears never to have reached the German journals. However, at about the same time as the publication of Brinkley's work in Ireland, there was a renewed quest for parallax in a number of different observatories in Germany. In Bode's Astronomisches Jahrbuch for 1818, published in 1815, there were three separate short entries concerning parallax, by Bessel, Lindenau and F. G. W. Struve (48). Again Bessel was involved early on in this general return to the problem; in fact, from his correspondence with Olbers it is clear that his resumption of the question dates back at least to November 1814 when, in a letter to his friend, he discussed Piazzzi's value of the parallax of the Pole Star and compared the Italian's observations with Bradley's (49). Bessel's conclusion was that Bradley's observations, which were greater in number and better than Piazzzi's, completely contradicted the latter's claim: Bessel could find no trace of parallax in Bradley's observations of the Pole Star. This view was further endorsed in Bessel's published paper in Bode's Jahrbuch.

During 1815 Bessel did not publish anything further on the problem of parallax detection, but he referred to the subject several times in his correspondence with Olbers. Unfortunately, one of the first of these letters, in which Bessel discussed his reduction of observations of the stars Sirius, Vega, Procyon and Altair, is lost and known only from Olbers's reply written six weeks later in January 1815 (50). From the reply and Bessel's next letter (51), it seems that in the first letter Bessel claimed a possible parallax of half a second for these stars, a value which Olbers thought was unlikely. Without Bessel's earlier letter it is impossible to tell how he arrived at this conclusion, although it is evident from the letter that we have that Bessel believed parallax could amount to this much (and this view of course accords with his 1812

work on 61 Cygni). He was still, nevertheless, very much aware of the difficulties associated with claiming definite values for parallax. When writing, in the extant letter, of a slight anomaly which he had detected in the position of 61 Cygni he stressed that

"... it would be rash if [he] wished already to establish from these observations that the star really has a noticeable parallax [since] the parallax of the fixed stars is a subject in which one must tread very carefully as many mistakes have been made in it". (52)

Whatever further work Bessel did on the four stars he had originally studied in 1808 did not appear in print until the publication of the Fundamenta Astronomiae in 1818, and will therefore be discussed below.

Bessel's remarks concerning 61 Cygni are significant, however, as he studied this star in some detail throughout 1815 and he published his conclusions in two accounts in 1816 (53). By the time he wrote to Olbers in February 1815, he had prepared a table of observations of the right ascension and declination of 61 Cygni during the period August to December 1814, all reduced to the epoch 1 January 1814. These results suggested that the right ascension was decreasing very slowly over this period. There was insufficient evidence to pronounce positively whether this was the result of a parallactic shift, but there was sufficient to produce the hope that it was so, and to persuade Bessel to continue his study of 61 Cygni. Unfortunately, according to his testimony in a letter to Olbers in June 1815, he was prevented from pursuing his examination of 61 Cygni because of bad weather (54). He had not, however, been idle as regards the quest for parallax: he had reduced observations of the difference in right ascension between two neighbouring stars in the constellation Cassiopeia - μ and ϵ . His conclusion from this investigation was in favour of a noticeable parallax, but he was reluctant to publish without further confirmation, and actually asked Olbers to keep the information

in the letter to himself, for, he said, he believed that certain other astronomers (whom he would not name) would not be willing to accept the possibility that his measurements were genuine.

Sadly, further confirmation was not forthcoming. By November 1815 Bessel was writing, again to Olbers, that "61 Cygni appeared from [his] observations of this year also to have no parallax" (55). But Bessel by no means gave up hope: he intended to compare the position of 61 Cygni with those of six nearby stars in the hope that one set of comparisons might at least yield reliable results. By doing this he was of course reverting to Herschel's idea of seeking relative parallax, and making use of the method which was eventually going to bring him success. It is at this stage that it becomes clear that Bessel was no longer merely toying with the question of the detection of parallax: by the end of 1815, convinced that the phenomenon could be found, he was prepared to put a great deal of effort into determining it. In all his references to parallax before 1815 he was very tentative about his suggestions, and content to provide a theory and framework within which the hard work of detecting parallax from a mass of data could be done at a later date. By 1815 the time had obviously arrived for putting the theory into practice (56).

But Bessel was to be disappointed again: when he had finished the reduction of the position of 61 Cygni relative to the six different stars he was forced to the conclusion that its parallax was negative! The paper in which this result was announced took the form of a report only, with no explanation of why a negative answer might have been obtained. Bessel gave tables of observations, and results of the application of the method of least squares to these observations for each comparison in turn; he followed this with exactly the same information for μ Cassiopeiae which he had compared with θ Cassiopeiae. For this star also he had a negative, though numerically smaller result. In volume III of the

Königsberg observations Bessel gave another set of observations, this time of the difference of right ascension between α and 61 Cygni from which he deduced an even larger negative result for the parallax of 61 Cygni (57).

A different account may be constructed from Bessel's private correspondence. In a letter to Olbers written at about the time that the first set of negative results for 61 Cygni was published, Bessel stated that

" The negative parallax which one [found] here and there and which [he had] in fact found for the Pole Star from Bradley's observations [was] of course the result of observational errors". (58)

He pointed out further that the size of the parallactic angle was probably of the order of the errors prevailing in the constants used for reduction (notably those of nutation and aberration), so that it was hardly surprising that occasionally a negative answer for parallax was obtained.

Bessel's arguments appear to be perfectly acceptable: it is not in the least surprising that negative answers should have occurred. It is actually more of a problem to explain why so few negative answers were obtained by others, unless of course astronomers who found them were not inclined either to publish them or to inform their colleagues of them. However, the astronomer C. A. F. Peters, writing in the early 1850s, regarded Bessel's 1815 results as a paradox, about which he consulted F. G. W. Struve. Struve's explanation was interesting, if perhaps unlikely. He wrote of

" ... another point, yet more dangerous, in the observations made expressly for the determination of parallax in right ascension. The astronomer, knowing the time of maximum and minimum, is in danger at these times, of making wrong estimates in one of two opposite directions, and thus producing apparent parallaxes which do not really exist, or destroying real parallaxes. The last of these appears ... to have been the case with Bessel. Wishing to avoid the danger of finding parallaxes which were

too large, he involuntarily fixed the fraction [of the angle] of 61 Cygni at transit at too small [a value] at the instant of maximum parallax and too high [a value] at that of minimum..." (59)

In other words Struve believed that Bessel's negative result derived from overcompensation on Bessel's part because he feared he might tend to bias his observations in favour of a positive parallax. This seems extremely unlikely for a number of reasons. To begin with, Bessel did not observe 61 Cygni at the time of maximum parallactic effect in right ascension; as he had indicated in his 1812 paper (ref. 35) this star was unobservable at that time, and indeed there are no observations of it within almost two months of the date in question (given by Bessel as approximately 4 May). Apart from that it seems impossible that any observer taking a transit measurement could keep in mind, even involuntarily, all the phenomena affecting the apparent position - nutation, aberration, refraction, proper motion and possible instrumental corrections - and still be able to predict how the tiny effect of parallax is going to alter that position, and what is more be able to compensate for this. Bessel's own diagnosis, that there were unaccountable observational errors and uncertainties in the reduction, is a far more reasonable explanation.

Bessel's opinions concerning the state of positional astronomy around 1815 are certainly borne out by a study of any of the work of his contemporaries (as discussed in some detail in section 4.3). Between any two papers or sections of books discussing the reduction of observational data, of which there are several (60), there are disagreements over the values of the constants to be used. The number of decimal places claimed by different astronomers for their constants and the ways in which they each applied the method of least squares to the data also vary considerably from astronomer to astronomer and from observatory to

observatory. The natural consequence of this was that the catalogued positions of stars differed in every available list, and much of the correspondence between astronomers at this time concerned such differences. Of particular significance to the detection of parallax and its history are the differences between observations catalogued in Dublin by John Brinkley, in Greenwich by John Pond, and in Königsberg by Bessel, as these differences were pointed out repeatedly by both Pond and Brinkley during their long controversy about the possible size of parallax. (See above, section 4.4). What is glaringly apparent throughout this literature is the lack of a widely applicable method of reduction. The first signs of such a method did not appear until the publication in 1818 of Bessel's Fundamenta Astronomiae, in which he produced the results of about 10 years work on the reduction of data gathered by James Bradley at Greenwich between the years 1750 and 1762 (61).

5.3 The Fundamenta Astronomiae

The whole of this important volume was concerned with the best method of handling a large number of observations in order that the position finally allocated to each individual celestial object was as accurate as possible. In order to do this, Bessel clearly had to apply himself to each of the problems prevailing in positional astronomy during the early years of the nineteenth century, and consequently the Fundamenta Astronomiae deals with instrumental errors, clock errors, the determination of the latitude of the observatory, refraction, aberration, precession, nutation, proper motion and of course parallax. It would be impossible to give a detailed analysis of the whole of the book here; as this thesis is concerned with the development of the problem of the detection of parallax, the discussion will be restricted to that section (62).

From that it should become clear how significant Bessel's work as a whole was for the development of the problems connected with parallax.

Bessel began his chapter on parallax by demonstrating that to seek the parallax of the bright stars Sirius, Vega and Procyon from Bradley's measurements of zenith distance was a profitless endeavour, for two reasons. Firstly the maximum value of parallax for any star always occurs in celestial latitude and the effect in zenith distance will be less by a conversion factor depending on the star's coordinates; for the three stars mentioned these factors were 0.312, 0.882 and 0.640 respectively (63). So even if the full effect of parallax were as much as a second of arc, only these small fractions of a second would be exhibited in zenith distance. Bessel's second reason for rejecting zenith distance measurements as a source of evidence for parallax was concerned with Bradley's instruments: the collimation errors were too uncertain. Because of this, when it came to the consideration of so small an angle as parallax Bessel preferred to work in right ascension, believing the clock errors to be calculable to a sufficient degree of accuracy.

The stars upon which he concentrated were those about which he had written one of his earliest parallax papers, in 1809: Sirius, Vega, Procyon and Altair. He had no new observations to present, but it was not for developments in practical astronomy that the book was so important. It was after all a detailed account and reduction of Bradley's observations, not of any new data, so Bessel was bound to work with what was available. As he had pointed out before, there were many observations of the stars he selected, because they are bright. But as a contribution to observational attempts to measure parallax this chapter in the Fundamenta had little significance: Bradley's observations would not demonstrate the existence of parallax. The importance of the work was that it represented the

maturation of several of Bessel's earlier thoughts on the problems of parallax, and the accumulation of all his ideas into a coherent system of analysis. Clearly, the next questions to be tackled are what form did the improvements take, and what significance did they have for the long-term problems of parallax detection?

To begin with Bessel simplified the formula for the difference in parallax in right ascension between two stars lying almost 12 hours apart. It is clear from his second 1809 publication (ref. 12) that he had derived a formula for parallax in right ascension and also that he knew of Olbers's formula relating parallax in right ascension to the longitude of the Sun. It was in fact from a modification of Olbers's formula that Bessel proceeded in order to formulate the difference in parallax that he sought (64). The equation that he acquired was long, and by making certain substitutions it became even longer. But at this stage the reason for the substitutions became apparent: combining them with the fact that the difference in right ascension for the pairs of stars under consideration was very nearly 180° , Bessel was able to eliminate several of the terms he had introduced as they approximated 0 or 1. In making these eliminations Bessel showed himself to be an extremely skilful manipulator of trigonometrical equations. Dealing with so small an effect as parallax he might have been expected to be afraid of making so many approximations, in case the effect was actually lost within them. However, he was clearly aware of what was and was not permissible, and the nett result was a very simple, but accurate expression for the difference in which he was interested.

This particular piece of work on parallax, despite its neatness and the fact that it illustrates Bessel's insight into the particular problem it was designed

to solve, was not of lasting importance, because after Bessel only one further attempt was made to measure the parallax of stars so situated (65). Of far more enduring significance was Bessel's treatment of errors, in particular his introduction of the idea that errors existed within the constants used for reduction. In other words, Bessel suggested that these constants could only be known within certain limits, but that the limits could be determined from observations. Prior to Bessel's work specific values for constants were deduced from observation and then applied to subsequent reductions; Bessel however calculated constants to within precisely determinable errors and his subsequent reduction was carried out in terms of these errors. This was a feature of the whole of the Fundamenta, and the impact of such a technique on astronomical calculation in general could only be appreciated fully from a consideration of the book as a whole. However, from Bessel's treatment of parallax, which he calculated in terms of the error in the constant of aberration, we can see how the method works and perhaps appreciate its potential when applied to the larger issues of the reduction of astronomical data.

Once Bessel had derived the formula for the quantity he sought, he calculated the relevant expression for each of the pairs under consideration, leaving the final formula in terms of the declination of each star and the longitude of the Sun at the instant of observation. For the difference in parallax in right ascension between two stars lying nearly 180° (12 hours) apart he obtained the formula

$$= u [\cos \lambda \cos \alpha + \sin \lambda \sin \alpha \sin w]$$

where $u = \pi \sec \delta + \pi' \sec \delta'$

$$\lambda = \frac{1}{2}(\phi + \phi')$$

$$a = \frac{1}{2}(\alpha + \alpha')$$

$$w = \text{obliquity}$$

and (α, δ) , (α', δ') are the equatorial coordinates of the two stars,

$\odot$ the longitude of the Sun when the first star was observed,

$\odot'$ the longitude of the Sun when the second star was observed,

π the parallax of the first star,

π' the parallax of the second star.

This was followed by specific equations for the pairs of stars involved. For example, Bessel wrote the difference for Procyon and α Aquilae (Altair) as:

$$+ 0.9874 [\pi \sec \delta + \pi' \sec \delta'] \cos \left(\frac{\odot + \odot'}{2} - 21^\circ 26' \right)$$

His next step was to introduce a term to allow for the effect of aberration, in which he assigned the value $20''.255 + \Delta$ for the constant of aberration. The introduction of Δ was important; for the first time he was breaking away from the idea that a constant had to be given a specific value. His contemporaries all appreciated that the values they used for constants might be inaccurate, but no one before Bessel tried to assess the extent to which they were inaccurate. With use of the term Δ , Bessel was doing precisely that. He wrote the difference in right ascension between Procyon and α Aquilae resulting from aberration as

$$+ 1.9900 \Delta \sin \left(\frac{\odot + \odot'}{2} - 21^\circ 26' \right)$$

using a formula he had derived in an earlier chapter (66).

Bessel now had a formula for the difference in right ascension between Procyon and α Aquilae which took account of the difference resulting from aberration and the difference expected as a result of parallax. At this point he turned to the observations that he possessed of the differences in right ascension between the stars in question, giving a table of Bradley's observations from the years 1750 to 1762. Bessel had reduced the observations (allowing for refraction, precession and nutation) to the epoch 1 January 1755. Any remaining differences over an annual cycle could therefore only be explained by

parallax or aberration. By applying the method of least squares (see above, section 4.3) to the observations of Procyon and α Aquilae he obtained the following three equations:

$$0 = -16.34 + 200p - 60.10q + 22.42r$$

$$0 = -2.93 - 60.10p + 148.78q - 41.35r$$

$$0 = 0.00 - 22.42p - 41.35q + 51.22r$$

in which p represented the correction of the difference in right ascension obtained from the fundamental catalogue, q the coefficient of the circular function for parallax [that is, the coefficient of $\cos\left(\frac{\vartheta + \vartheta'}{2} - 21^\circ 26'\right)$], which depended on the sum of the parallaxes π and π' , and r the coefficient of the circular function for the correction of aberration [that is, the coefficient of $\sin\left(\frac{\vartheta + \vartheta'}{2} - 21^\circ 26'\right)$], which depended on Δ .

Using a method taught to him by C. F. Gauss (67), Bessel proceeded to solve the equations, and to express the values of p , q and r within certain limits (or errors) derived from the original observations. He obtained the value 0.09952 with a probable error of 0.01074 for p , 0.06162 with a probable error of 0.01379 for q , and 0.00618 with a probable error of 0.02260 for r . These values were fed back into the original equations and Bessel showed that the sum of the parallaxes of Procyon and α Aquilae was 0.9313 with a probable error of 0.2085, and that the uncertainty in the constant of aberration was 0.0466 with a probable error of 0.1703.

Despite having obtained a value of about one second for the sum of the parallaxes of these two stars, Bessel did not claim that he had at last successfully detected traces of parallax. He was still too unsure of the value of the constant of aberration to make any positive claims. For although from the observations of Procyon and α Aquilae he obtained a low value of Δ , this particular result was not repeated when other observations were considered. When the observations of Sirius and Vega were reduced the value of Δ obtained

was $0^{\circ}6247$ with a probable error of $0^{\circ}1417$, and when dealing with observations of the Pole Star (in which case the single parallax of the star was sought) Bessel calculated a value of $0^{\circ}5001$ for Δ with a probable error of $0^{\circ}0928$ (68). If the uncertainty of the constant of aberration could amount to half a second, then the claim that the sum of the two parallaxes might amount to one second was not a particularly well supported claim, as Bessel well knew.

It is again true that the significance of the particular answers obtained by Bessel was not high, but these calculations illustrate what could be made of a large selection of data by someone prepared to devote his time and energy to them. And the real importance of Bessel's work lay in the attention he paid to the errors. The significance of his calculating errors at various stages, and carrying them through the remainder of the calculation was that the final errors allocated were far more realistic than any previously offered because they had been derived as rigorously as possible. In each section of the Fundamenta Bessel dealt with just one or two phenomena - as in the chapter on parallax and aberration - but if the whole book is taken into consideration, it is easy to appreciate how much Bessel's treatment of the reduction of raw data was an improvement on anything that had preceded it, at least in positional astronomy. His methods were applicable to any sets of observations: for the first time a complete system of analysis was available to any astronomer who wished to use it. The Fundamenta Astronomiae set a very high standard, rendering obsolete previous, usually haphazard methods of reduction, and it was clear that in future observations would have to be treated with similar rigour if they were to have any real meaning.

In every account of the Fundamenta, the book is recognised as an outstanding achievement in the history of astronomy, but the reasons for its excellence are

never fully explained. The generally accepted reason is that Bessel's work on Bradley's observations meant that the beginnings of precise astronomy now dated back to the mid-eighteenth century. Thus in her standard work on the development of astronomy during the nineteenth century, Agnes Clerke wrote that

"The eminent value of the [Fundamenta Astronomiae] consisted in this, that by providing a mass of entirely reliable information as to the state of the heavens at the epoch 1755, it threw back the beginning of exact astronomy almost half a century". (69)

And similarly, Bessel's biographer in the Dictionary of Scientific Biography, Walter Fricke, claimed of the Fundamenta that

"It constitutes a milestone in the history of astronomical observations, for until then positions of stars could not be given with comparable accuracy: through Bessel's work Bradley's observations were made to mark the beginning of modern astronomy". (70)

This claim is undoubtedly justified, but it does not convey completely the enduring quality of Bessel's work: it merely emphasises the significance of the results of his endeavours. Bessel did indeed make available to all astronomers a set of data which was to prove invaluable for all processes of comparison: for example, in checking the constant of precession, or in the estimation of proper motions. But the truly remarkable aspect of Bessel's work was the way in which he dealt with the bare observations and manipulated them into a form which could be useful. The observations were published in the early nineteenth century, but no other astronomer took the trouble to find out what, if anything, could be made of them, or even really realised that a wealth of information might be contained in them. It was Olbers who informed Bessel of the existence of the observations (71), but it was the latter who saw the potential of Bradley's data and who managed to combine his exceptional insight into the problems of

reduction with the persistence required to work through the mass of observations that Bradley had left. The undertaking was enormous in terms both of the time needed and of the dedication to what must sometimes have been extremely dull and repetitious arithmetic. Bessel's work cannot be adequately estimated if only the standard of the catalogue is assessed. Of as much, if not more importance were the methods he used to produce the catalogue. The reason that the results were so useful was that, because of the minute attention paid to the assessment of errors, the final values allotted to the positions of the stars were reliable and convincing. It was the rigour of Bessel's analysis that made this possible, and that deserves at least as much recognition as that given to the final results.

In the section of the Fundamenta devoted to parallax can be found the culmination of several years of intermittent thoughts and ideas on the problem. The conclusion was disappointing but definite: Bessel could not detect parallax for any of the stars he had studied, working from Bradley's data. After the publication of the book Bessel did not return to the problem for a number of years. It was not until well into the 1830s, after the publication of his second major work on precision in positional astronomy, the Tabulae Regiomontanae, and the acquisition of his Fraunhofer heliometer, that Bessel renewed his attempts to detect parallax. Why this gap should have occurred and what caused Bessel to return to the problem are questions which will be discussed below (see section 6.2). Suffice it to say for the present that, given the data he possessed in 1818, Bessel had done as much as was possible towards a solution of the old question of parallax, but it was still not enough to supply the answer.

5.4 Conclusions

From the above account of Bessel's early life, it is evident that he was concerned with the problem of parallax early on in his career and his interest in it, although not continuous, recurred several times during the years leading up to the publication of the Fundamenta. But what was the significance of this interest for the long-term development of concern about parallax? And to what extent is Bessel's work a good indicator of contemporary trends and interests in astronomy? Answers to these questions can be produced from a study of Bessel's motivations: a consideration of what he thought he was doing and why he was doing it.

Bessel's reasons for considering the questions related to parallax were not always the same: sometimes as in his reference to it in 1806, he was led to a consideration of the phenomenon after reading the thoughts of various of his contemporaries on the subject. At other times it was Bessel who produced the thought-provoking material, as with his work on 61 Cygni in 1815. These two separate types of contribution reflect different periods of Bessel's career; in the early years while he was still at Lilienthal, much of his time was spent working on problems pointed out to him by other astronomers, and this is true of his early work on parallax. Nor is this surprising; working as an assistant to Schröter at the beginning of his career in astronomy, Bessel would be expected to apply himself to prevalent problems in the hope of proving his ability; he was still establishing himself and being guided by others, particularly Olbers. The work Bessel produced in this early period was often outstanding and obviously instrumental in his being suggested for the directorship at Königsberg, but it was usually not self-motivated.

Bessel's appointment was the second of two factors which combined to transform him from a bright assistant astronomer into one who led in his profession, reforming positional astronomy with his hard work and brilliant insight. The earlier factor was the suggestion by Olbers that Bessel might like to reduce Bradley's newly published observations. In the years between Bessel's leaving Lilienthal and the opening of the Königsberg observatory, almost all his work in stellar astronomy derived from his analysis of Bradley's data, and from this period also date certain projects which he intended to undertake once he had his own equipment, most notably for parallax his decision to observe 61 Cygni.

The early work on parallax and its detection illustrates this picture clearly. His papers of 1809 were worked out after Bessel's attention had been drawn to the existence of problems by Lindenau and Olbers. Then, the next important publication on parallax - that of 1812 - evolved from Bessel's study of Bradley's observations of 61 Cygni, which led Bessel to a consideration of the observations of that star by other astronomers, thence to an appreciation entirely his own that this double star might be a very suitable candidate for the detection of parallax. As he had no instrument at his disposal at this time he had to wait until the new observatory was open before he could actually make the necessary observations, but a study of 61 Cygni was one of the first projects to be undertaken once the observatory was ready. This resulted in the papers in which he allocated a negative parallax to 61 Cygni. This last work was the first to be undertaken by Bessel with the aim of measuring parallax for the sake of knowing he could do it, and for the sake of finding out something new about the structure of the heavens.

It was also the last time for many years that Bessel attempted to measure parallax in this way and for these reasons. The work on parallax in the Fundamenta Astronomiae was carried out for a completely different reason. In the Fundamenta Bessel was concerned with the production of a highly accurate stellar catalogue, so his interest in parallax derived from his concern about how it might affect the apparent positions of celestial objects. He needed to try to calculate the effect so that he could either allow for it or, as it turned out, demonstrate that it was undetectable and hence could be ignored. This does not mean that he would not have been delighted to have found traces of the effect; merely that such success would have been a by-product of the more important need to obtain accurate stellar positions. It is perhaps ironic that of all Bessel's early publications on parallax, the most important one for the development of precision within stellar astronomy and thence indirectly for the identification of parallax, was the one which at the time of its publication was least concerned with the measurement of the phenomenon for its own sake.

As has been stressed throughout this chapter, the most important aspect of the Fundamenta was the minute attention paid to the assessment of errors of all kinds. But, as Bessel was well aware, before he could make any categorical statements about the errors, he had to have at his disposal a sufficiently large number of observations. Bessel's recognition of this need was significant for the enduring success of the Fundamenta; and such recognition was also novel. Although his predecessors and contemporaries always tried to obtain as many observations as possible, no one before Bessel would ignore data simply because there were not enough of them. But for Bessel the number of observations analysed was always a vital criterion in his evaluation of results. It was for failing to have enough data that

he criticised Lindenau in 1814, and it was primarily the number of observations that he himself possessed which guided his choice of the stars to analyse for parallax. Bessel clearly was very much influenced by the statistical ways of reasoning which were beginning to be important in a number of the natural sciences during the early nineteenth century. It is obvious from his chapter in the Fundamenta on parallax and aberration that he was familiar with and impressed by several of Laplace's ideas (72), and he also corresponded with Gauss throughout the period when he was writing the Fundamenta. He was bound therefore to be aware of the immense amount of work that was being done at this time on the theory of statistics and probability. The concept that results had to have statistical significance remained very important to him for the rest of his life, and clearly influenced the work he undertook in the 1830s which led to his successful identification of parallax.

Finally, the question of how Bessel's work on parallax fits into both the rest of his own work and that of his contemporaries must be raised. All through Bessel's early work on parallax it continued to be only a very small proportion of his total commitment; he devoted his time to it when the occasion arose, but he was hardly obsessed with the idea of detecting the phenomenon, in the way that for example John Brinkley was. Not until the period leading up to Bessel's final success was parallax ever the most important part of his work; he was content to turn to the question every now and again, and to contribute to it what he could.

The early nineteenth century saw the establishment of stellar astronomy as a major component of astronomy as a whole, and Bessel certainly played a very important part in this: indeed by the time of the publication of the Fundamenta Astronomiae he was devoting a larger part of his time to sidereal astronomy than were almost any of his contemporaries. The stars had been studied as

individuals very occasionally as far back as the late seventeenth century and this study continued in a small way all through the eighteenth century; but it was not until the early nineteenth century that the investigations of proper motions, variable stars and double stars became systematic and were the rule rather than the exception. If we consider the number of papers dealing with parallax, as many were written during the first 20 years of the nineteenth century as were written during the whole of the eighteenth century, and this statistic is repeated in other branches of stellar astronomy. The astronomy of the solar system was still the most widely studied part of astronomy well into the nineteenth century, but with the publication of Bessel's Fundamenta the balance was beginning to swing in favour of sidereal astronomy.

By the time Bessel finally measured parallax in 1838 as much stellar astronomy as planetary was undertaken, certainly at the major European observatories, but for this period we cannot consider publications about parallax as illustrative of general trends. By then (but indeed only by then) it was a particularly important study within stellar astronomy. It gave Bessel yet another chance to demonstrate his skills both as a practical and a theoretical astronomer, but it must be remembered that much of the vital work which enabled him to detect parallax had been done over 20 years earlier, during his reduction of Bradley's observations and the preparation of the Fundamenta Astronomiae.

Chapter Six

THE SUCCESSFUL IDENTIFICATION OF ANNUAL STELLAR PARALLAX6.1 Introduction: the 1820s

The 1820s and early 1830s were not without significance for the eventual successful measurements of parallax. However, before the achievements made during these years are considered, it would perhaps be useful to assess contributions to the parallax problem made during the period 1800-1820, and to review the problem as perceived by astronomers in about 1820.

Interest in parallax was widespread in Europe during the early part of the century. The earliest signs of such interest appeared in the work of the German astronomer Johan Schröter (1), and that of the Italian Guiseppe Piazzi (2). As explained in Chapter Three, it is not surprising that either astronomer should occasionally, amongst all their other concerns, have turned their attention to the question of parallax. And indeed the publications of each brought responses from their fellow astronomers. Schröter's work caused Bessel to consider the problem of parallax and according to the Italian Guiseppe Calandrelli, it was his own reading of Piazzi's announcements of the parallaxes of certain stars which prompted him to investigate the movements of Vega (3). The work of Piazzi and Calandrelli was the immediate stimulus for Jean Baptiste Delambre's short paper in the Connaissance des temps pour 1808, in which he discussed the results from observations of declination made by the Italians and

urged other astronomers to confirm them with observations of right ascension (4). In its turn Delambre's paper focussed the attention of the British astronomers John Pond and John Brinkley on the question, although neither astronomer wrote at length on the subject until the mid-1810s.

In the meantime, at the Seeburg observatory near Gotha in Germany, Lindenau began a systematic investigation into contemporary interest in parallax. His findings were published in three parts in the pages of the Monatliche Correspondenz for November 1808, January 1809 and March 1809 (5), the last of which included part of a letter from Bessel explaining the derivation of the latter's formula for parallax in right ascension (6). It was also at about this time that Bessel began his search for evidence of parallax among Bradley's data, the results of which he published in the Monatliche Correspondenz for February 1809 (7). The continuation through the years 1810-1818 of Bessel's interest in parallax was discussed in depth in the previous chapter; but Bessel was not alone in his concern during this period. It was he who published most of the German material, but it is clear that Olbers, Gauss and Lindenau all kept in touch with Bessel's work on parallax. Lindenau himself tried to deduce a value of parallax from his observations (8), and Olbers and Gauss both corresponded on the subject with Bessel and with each other (9).

As will be discussed more fully below (see Chapter Seven) the interaction between these four astronomers is indicative of a determined effort on their parts to perfect positional astronomy. Together with the help of certain other German astronomers (notably von Zach and H. C. Schumacher, a student and life-long friend of Gauss), they wished to ground the study of astronomy more firmly than ever before in rigorous mathematics. Annual stellar parallax was not their main concern, but it was a study in which the success of their campaign

to perfect astronomy could be tested. Indeed, the fact that they wrote about parallax at all is most easily explained in this way: their wish to detect the phenomenon was a symptom of their wider endeavours in observational and theoretical astronomy. Nevertheless the publications which resulted played a significant part in the history of attempts to measure parallax, by focussing attention (their own and others') not only on the problem itself but also on possible reasons why parallax remained elusive.

By 1815 their younger contemporary from Dorpat, F. G. W. Struve, had started to investigate the possibilities of detecting parallax, and he published his first short remarks on the subject in the Astronomisches Jahrbuch für 1818, which appeared in 1815 (10). At this stage of his career Struve had to conclude that the observations he possessed of the Pole Star did not indicate a parallax, and he did not return to the problem for about three years. His interest in parallax might well have emerged from his knowledge of similar work being undertaken in the German states, but his connection with other German astronomers is difficult to establish. Although German by birth, until the late 1830s he worked at Dorpat, in Estonia, then under Russian rule, and perhaps as a result of this Struve was always outside the main early nineteenth century movement in astronomy (11). His work was often published in the major German journals (before 1823 these were the Berliner astronomisches Jahrbuch and the Monatliche Correspondenz) and usually read with interest. However, as far as it is possible to tell from the printed correspondence, his endeavours were parallel to but independent of those being carried out by Gauss, Bessel, Olbers, Lindenau and Schumacher. It is clear that Struve corresponded occasionally with Bessel and with Olbers (12), usually exchanging observations with them, and he was always very respectful of Bessel's work when it was mentioned in his publications. Nevertheless,

his programmes of research at Dorpat - and subsequently at Pulkowa when he became the first director of the new observatory there - were devised himself, and were independent of any being undertaken in the German observatories. Also, since he lived and worked in Russia, the results of his research were generally first made public at the Imperial Academy of Sciences at St Petersburg, of which he became a corresponding member in 1822 and a full member in 1832, rather than at the Berlin Academy.

Struve's relative isolation could explain in part why he persisted with the quest for parallax well into the 1820s, while everyone else in the German states remained silent. For, after three publications, in 1816 and 1817, by Bessel (see section 5.2) and one in 1816 by Lindenau (13), nothing further was published by any German astronomer, apart from the work of F. G. W. Struve, until the early 1830s. Interest in the problem was maintained during the 1820s, but it was primarily in Britain, where the controversy between Brinkley and Pond continued. Both Bessel and Olbers referred in their private correspondence to this controversy (14), but other than that none in their circle of correspondents had anything to contribute directly to the study of parallax. Since the development of precise positional astronomy, which was so important for the eventual identification of parallax, took place on the whole in Germany, it would be appropriate, before proceeding to an account of Struve's 1820s work on parallax, to attempt an explanation of why other German astronomers apparently lost interest in the problem.

As has been stressed several times already, the most important aspect of the quest for parallax that must be borne in mind whenever lack of interest in it needs to be explained is that the problem was never of paramount importance to the astronomers involved. Obviously during the early nineteenth century certain astronomers considered

the problems of parallax very seriously, but only as part of their researches, and never to the exclusion of all else. If we look closely at the contributions of German astronomers it is clear that only two of them - Bessel and Lindenau - actually analysed their data for parallax and published their results. Olbers's interest may be detected in his correspondence, but his main role was that of informer: he was very active in passing on results to the astronomers he believed would most like to see them. Gauss was often a recipient of such information, but he rarely referred to parallax either in his published work or in his letters. He would undoubtedly have been very interested to hear of a successful measurement of parallax, as would most of his contemporaries, but measuring it was not part of his own particular enterprise. The same is true of von Zach, Schumacher and Johann Elert Bode, although Zach and Bode played more direct roles than Gauss as they were responsible for the publication of results in the Monatliche Correspondenz and the Astronomisches Jahrbuch respectively. They were nevertheless interested by-standers in the story of attempts to measure parallax, rather than active participants. The explanation of lack of interest required therefore becomes one in which it can be shown why, after mid-1817, the two central figures - Bessel and Lindenau - stopped trying to detect parallax.

The explanation of Lindenau's actions is not difficult. In 1818 he was obliged to return to his post in the civil service and he left the Seeburg observatory to recommence his duties with the government. By doing this, not only did he remove himself from his means of gathering data, more importantly he lost contact with others in the astronomical world. Unlike Olbers who combined his interest in astronomy with his profession as a physician, once Lindenau left Seeburg it seems that he left astronomy also.

The situation as far as Bessel is concerned is not so straightforward. From a study of his publications and

the contents of his letters in the period 1817-30 it is obvious that he continued to be deeply interested in the problems surrounding parallax: the establishment of accurate astronomical constants, the reduction of the positions of certain fundamental stars and the precise calculation of latitude occupied a large part of his time. But he did not address the specific problem of the detection of annual parallax. This need not however be regarded as paradoxical. Bessel had pointed out in 1816 and 1817 that negative values for parallax occurred because the size of the angle sought was, at most, of the order of the errors in the observations and in the constants. It would therefore be pointless to try to find evidence of parallax before he could be sure that he had reduced the errors by at least one order of magnitude. In the Fundamenta Astronomiae he had developed methods of analysis which allowed him to obtain the best possible reduced values from his data; what he needed to do next was to refine the data and to gather as much of it as he could. This would mean in turn that he could improve the reduction by calculating constants and the errors inherent in them to an ever increasing degree of accuracy. Within the realm of positional astronomy this was certainly the type of work in which he was engaged throughout the late 1810s and the 1820s (15). It is extremely unlikely that the issues connected with parallax were actually forgotten during this period; rather they were in abeyance until Bessel could believe that the study of astronomy was sufficiently further perfected to make the detection of parallax a feasible proposition again. Why he might have come to such a decision when he did will be discussed below.

Thus, by about 1820, specific interest at the German observatories in the detection of parallax had certainly declined. In France few astronomers had shown any great inclination to study the problem at all during the early years of the nineteenth century. Apart from

Delambre's short publication in 1806, and his 1816 translation of and commentary upon Brinkley's first major paper claiming genuine measurements of parallax, there are no obvious references to the phenomenon at all. Appealing once more to the argument that parallax was not necessarily an important issue to any astronomer, we need not be unduly surprised by such inactivity in France (16). Similarly in Italy, after the endeavours of Piazzzi and Calandrelli neither of their successors paid particular attention to the question of parallax (17). By 1820 then parallax was not on the whole perceived by many astronomers as an urgent problem; as had happened at every previous occasion when astronomers had failed to measure the phenomenon, they chose for a time not to pursue the problem any further, but to concentrate instead on projects which reaped better rewards.

However, a certain amount of interest was maintained: in Great Britain John Pond and John Brinkley continued well into the 1820s to write about parallax, and at Dorpat F. G. W. Struve wrote two substantial accounts of his attempts to detect parallax from observations made between the years 1818 and 1821 (18). From both publications it is clear that Struve was familiar with Bessel's work in the Fundamenta Astronomiae and the Königsberg volumes of observations. He used a method to detect parallax similar to that favoured by Bessel in the Fundamenta: he calculated the difference in right ascension between two stars lying almost 12 hours apart. In the first instance he applied the method to the observations he possessed of the following pairs of stars among others: α Cassiopeiae and ϵ Ursae Majoris; δ Cassiopeiae and ζ Ursae Minoris; α Persei and β Ursae Minoris; γ Ursae Majoris and α Cephei; and finally ι Ursae Majoris and α Cygni. He also carried out a detailed analysis of his many observations of the Pole Star; in doing this it is possible that Struve was again following Bessel's lead: the parallax of the Pole

Star was one of the few single parallaxes discussed in the Fundamenta Astronomiae. For the other stars chosen, Struve deliberately restricted himself to pairs of circumpolar stars, so that the superior culmination of one occurred within a very short time of the inferior culmination of the other (and vice versa), thus avoiding all the problems of possible changes in the instruments over a period of 12 hours. The specific pairs he chose also reflect his wish that both culminations of each should be visible at opposite seasons of the year.

Struve's results were not encouraging: for several pairs of stars he obtained a negative value for the sum of their parallaxes, and for all but one of the remaining pairs the results obtained, though positive, were so small that they could not be considered as convincing evidence of parallax. In the earlier set of calculations given (in the second volume of Dorpat observations published in 1820) Struve did not provide any evaluation of errors at any stage in his reduction, or even an indication of their extent in his final answers. This is surprising since he had obviously seen Bessel's Fundamenta and might perhaps be expected to have appreciated the need at least to quote the possible errors in his results. However, when he published the second set of calculations, two years later, he outlined the stages of his reductions in some detail and did give his answers in terms of the errors. It is perfectly possible, therefore, that he had done much of the work on the earlier observations before he had read Bessel's work, but by late 1821 or early 1822, when he was preparing volume 3 of the Dorpat observations, he had had sufficient time to assimilate and apply Bessel's methods of reduction. The second set of results was unfortunately no more convincing than the first. Struve still had a mixture of positive and negative values for the sums of parallaxes and the size of the values was generally of the order of the size of the errors. However, he produced one result he thought worthy of attention:

he obtained the value $0''.434$ for the sum of the parallaxes of α Cygni and ι Ursae Majoris (19).

This value caught the attention of the reviewer who wrote an account of volumes 2 and 3 of the Dorpat observations for the Edinburgh Journal of Science in 1824 (20). He commented that

"it must be considered a remarkable coincidence that Dr. Brinkley, by observations of zenith distance with the Dublin circle, makes by 228 observations the parallax of α Cygni = $0''.46$... and although the above is the sum of the parallaxes, there is no reason for assigning a visible parallax to ι Ursae Majoris to disturb the coincidence of the Dublin and Dorpat results in this respect". (21)

No such conclusion was drawn by John Pond, however, when he wrote a short account of Struve's work for the Quarterly Journal of Science in 1823 (22). He mentioned the value Struve had obtained, but only casually and without any reference to Brinkley's work. Pond's main point was that Struve's results supported strongly his own view that to seek parallax, particularly in declination,

"must be entirely hopeless; since in this case we can only measure the shorter axis of the Ellipse, and the uncertainty of refraction must amount, at least, to twenty times the quantity we are in search of". (23)

This remark must be interpreted in the light of Pond's disagreement with Brinkley (most of whose claims were derived from observations of declination), in which case the British Astronomer Royal was bound not to mention the coincidence of Brinkley's and Struve's results for the parallax of α Cygni.

As far as general interest on a continental basis is concerned, it is significant that it was only in Britain that astronomers took any notice of Struve's work on parallax. It was certainly not mentioned in any of the journals published in France or the German states, and although in a letter to Olbers in 1821

Bessel mentioned Struve's observations of the right ascension of the Pole Star, he did not comment at all on their relevance or otherwise to the problems of parallax (24). Presumably Bessel would not have been surprised by Struve's failure to detect a significant parallax from his observations; the fact that Struve had failed would only have convinced Bessel even further that an increase of precision in astronomy was necessary before parallax could be sought with any real hope of detecting it. Indeed, after his early enthusiasm it is clear that Struve too turned away from the problem of parallax. In volume 3 of his observations he had said that he hoped to make more observations of his pairs of stars with the aim of detecting a measureable parallax. But there is no evidence that he actually returned to the question until the mid-1830s. Precisely what he was doing in the meantime will be discussed below.

Thus the 1820s was a period of relative quiet in the nineteenth century history of attempts to measure parallax. However, it was not a period lacking in relevant domains of enquiry: as stressed repeatedly, before astronomers would again have the confidence to seek evidence of parallax they needed to be sure, firstly that they were observing suitable objects, secondly that they had the means of obtaining observations which were more accurate than any already in existence, and finally that they had the theoretical tools for undertaking the necessary reductions. Each of these requirements were met, or were at least perceived to have been met, by Bessel, Struve and Henderson in the late 1830s, and the significant final developments were made during the 1820s and early 1830s. It was during that time that interest in double stars grew, that Fraunhofer produced his magnificent astronomical instruments, and that Bessel worked on his second important volume of observations, the Tabulae Regiomontanae (25); it is these developments which must be considered next.

6.2 Developments in the study of double stars

The importance of the study of double stars to the history of attempts to measure annual stellar parallax was explained in part above, in connection with William Herschel's 1781 paper on the parallax of the fixed stars (see section 3.4), and with Bessel's study of the particular double star δ Cygni (see section 5.2). By about 1820, only these two astronomers had made significant contributions to the study of doubles, but when, in the late 1830s, three measurements of parallax were independently announced, each of the three stars in question was a double: δ Cygni and α Centauri are both binary (that is physically associated) systems and Vega is an optical double. The study of double stars had become commonplace at many observatories by the mid-1830s, and it is easy to see that they were objects of extreme interest by then. It is thus understandable that such stars were very likely to be selected for study when parallax was again sought. However, no matter how understandable the choice of doubles in the mid-1830s might be, it is necessary to explain how and why the study of them emerged during the 1820s as a major part of astronomical enquiry. The specific question of why they were regarded as appropriate for parallax work must also be addressed: by the time parallax was successfully identified the reasons for observing doubles had become more complex than those offered by William Herschel in 1781, and these new reasons must be investigated.

The emergence of the study of double stars is particularly associated with the work of Struve in Dorpat and that of William Herschel's son John, carried out in collaboration with the astronomer James South who ran private observatories in Borough, Lambeth and subsequently in Kensington. This much is revealed in many if not all of the available general histories of astronomy; but in almost none of these accounts does the historian feel the

need to explore this new interest any further. It is as if they believe astronomers were inevitably going to study double stars, but that is hardly an adequate explanation. The only historian who attempts to broach this problem is A. Pannekoek who writes that

"double star astronomy became the first field of application for the refined nineteenth century instruments, with their higher standard of accuracy. It began with F. G. W. Struve ... who in 1819 had already measured doubles, in 1824 introduced the new 9" refractor, the biggest Fraunhofer instrument to double star work". (26)

It is certainly true that Struve began his twelve year search for and measurement of double stars as soon as the new Fraunhofer refractor had been erected at Dorpat and suitably tested. But it is difficult to tell whether Pannekoek believes Struve turned to this study because he suddenly possessed an excellent new telescope, or whether the instrument was required because Struve wished to make a detailed survey of double stars. Either interpretation is moreover inadequate, in common with all explanations of events in the history of astronomy which rely solely on instrumental developments. As Pannekoek points out, Struve's interest in double stars antedates his acquisition of the Fraunhofer refractor. His earliest publication on doubles appeared in 1817; he wrote on the subject to Lindenau at the Seeburg observatory and Lindenau published the letter in his new (and short-lived) journal, the Zeitschrift für Astronomie (27). In the following years, but before Struve received the new telescope from Fraunhofer, he published at least five papers or letters on the subject of double stars, and four catalogues of them (28). As well as these, he devoted sections of the first three volumes of observations made at Dorpat specifically to accounts of his latest observations of double stars (29). There can be no doubt, therefore, that Struve made the study of double stars one of his most important concerns early on in his career.

From his first publications it is clear that Struve was familiar with earlier work on doubles: Bessel's 1812 paper and William Herschel's catalogues of doubles, particularly those from 1803 and 1804 in which Herschel showed that the components of some pairs of stars had moved relative to each other over the preceeding twenty years (30). Herschel's papers had not gone completely unnoticed at the time of their publication: for example, they were mentioned several times by Olbers in his correspondence with Gauss (31). But Herschel's work on doubles did not create much of a stir in the astronomical world, despite the fact that the type of movement Herschel had detected could have vital implications for the application of Newtonian dynamics beyond the confines of the solar system (32). Indeed the earliest paper to suggest that conclusions about the fixed stars could be drawn from the application of Newton's laws to the motions of a double star was Bessel's 1812 study of 61 Cygni. Bessel was full of praise in this paper for Herschel's catalogues of doubles, but he was one of the very few before the 1820s to regard double stars as objects worthy of individual attention. In fact the only other astronomers who were definitely fascinated this early by double stars were Struve and John Herschel.

Like so many of his contemporaries, Struve was concerned with the problems of precision in positional astronomy: his early publications, other than those on double stars, mainly covered topics which could be defined as being within mainstream astronomy. Among other things he wrote about lunar occultations, Jupiter's satellites, the minor planet Vesta, and about comets. In each of his papers he was careful to point out that his observations and reduction had been carried out with the utmost care. As Bessel had been about 10 years earlier, by the late 1810s Struve was eager to establish himself at the forefront of astronomical

research. However, as explained above, Struve was never a central figure in the enterprise of German astronomers to perfect astronomy; he shared many of their aims, but was especially keen to establish precise astronomy at his own observatory at Dorpat. He was rather more of an individualist than were his counterparts in Germany, and this is illustrated by the way in which he organised his own research towards the end of the 1810s and during the early 1820s. His persistence with the quest for parallax despite the fact that most other astronomers had turned their attention elsewhere is one facet of his individual approach. And this approach is most clearly demonstrated by his detailed study of double stars. He certainly wished to prove his ability as an expert observer and manipulator of data and perhaps perceived that his best means of doing this was to turn his attention to a field of enquiry which had, until then, only been little studied: that of double stars.

Looking at Struve's early work in this way, it is possible to extend Pannekoek's remarks about the early study of double stars. Rather than merely being a field for the application of refined instruments, it was one for the application of refined practice of astronomy as a whole, and it was exploited fully by Struve. Once he had decided to devote much of his time to the observation of doubles, he would of course have used the best instruments that he possessed and that would quite clearly have included the excellent new refractor he obtained in 1824.

Struve certainly persisted with his chosen programme of research. In 1826 he received an honorary medal from the Astronomical Society of London for his work on double stars (33); in 1827 he published his first catalogue based on observations made with the Fraunhofer refractor (34), and after that he devoted his time to his major survey which culminated in the important 1837 catalogue in which Struve claimed to have measured the parallax of Vega (35). (See below, section 6.4).

We must now consider how his work was received and how it fitted in both with similar studies undertaken in Britain by John Herschel and James South and with general trends in positional astronomy during the 1820s.

The greatest interest in and reaction to Struve's work took place in Great Britain, perhaps not surprisingly since it was in London that South and Herschel were beginning their searches for double stars, and nothing of a similar nature was undertaken at any other European observatory at that time. However, it was in Edinburgh rather than in London that most of Struve's early work was reported, in the pages of David Brewster's Edinburgh Journal of Science (36) and the Edinburgh Philosophical Journal (37). In the volume of the latter for 1823 it was reported that

"Mr Struve, an able and active astronomer at Dorpat in Livania, conceived the very laborious design of making a new catalogue of all the Double Stars which were observed about forty years ago, by William Herschel - to compare these observations with his own, and especially to examine if any change had taken place in the relative position of the two stars". (38)

This was followed by a summary of some of Struve's observations, and the summary was continued in the volume for the following year (39). In subsequent years various of Struve's later observations were also published; the observations usually appeared without any editorial comment, although one of the entries in the journal for 1824 was introduced with the words

"the greatest degree of interest which is now attached to the accurate observation of the colour, magnitude, and relative position of double stars, gives a particular value to the observations of Mr Struve". (40)

Unfortunately, precisely who showed this "greatest degree of interest" was not disclosed. However, it is clear that James South's surveys of doubles began at least as early as 1820, as he read a paper on the subject

to the newly formed Astronomical Society of London in May of that year (41). Also, according to South and John Herschel in their first major publication on double stars, written in 1824, the latter began in 1816 a re-examination of the doubles catalogued by his father in 1803 and 1804 (42). It was subsequently at South's suggestion that, in March 1821, he and Herschel started a combined programme of observations, the results of which were produced in the 1824 paper. The authors of the paper made it clear that they knew of Struve's work, writing that

"the comparison of his observations of such of [their] stars as have been measured by him with [their] own, will not be found the least interesting part of the paper. So far as it goes, the coincidences of [their] results, with very few exceptions, are striking; and afford the most satisfactory ground for reliance on the methods employed by both". (43)

They did, however, point out that they and Struve had commenced their surveys independently and in ignorance of one another. They also referred to similar (and again independent) work being carried out by Professor Amici, a teacher of mathematics at Modena (famous for his remarkable work on the optical systems of microscopes), but they had been unable to obtain many of his results (44).

The picture of contemporary interest in double stars portrayed by Herschel and South is thus one of a very few individuals at widely separated locations in Europe, gathering observations of double and multiple stars each for their own independent reasons. Nearly two years later, in November 1825, South presented a second detailed analysis of his observations of doubles, and from his introduction we learn that some of the observations had been gathered in Paris, where South had been greatly encouraged in his endeavours. The astronomers he had met and apparently conversed with at length during his stay in Paris included Laplace, Humboldt and Arago (45). From this it can be seen how interest in double stars began to spread: South's work

was known in Paris and Struve's in Germany. Apart from Struve's papers little was published in the German journals, but the subject of doubles was referred to during the mid-1820s in the various correspondences between Bessel, Olbers, Gauss and Schumacher, showing that they too were beginning to take notice (46).

It is also apparent from South's second catalogue that William Herschel's original motive for identifying doubles - that was, in order to use them to seek parallax - had not been entirely lost sight of. South mentioned in a footnote that Laplace had stressed its importance to him (47), and during the year after South's second catalogue appeared, John Herschel worked out a new way of using doubles specifically for the purpose of detecting annual stellar parallax. In December 1825 he sent a paper explaining the method to the Royal Society (48). The novelty of Herschel's method lay in his exploitation of the effect that a parallactic shift might have on the angle of position of an optical double: that is the way in which parallax could affect the orientation of an imaginery line joining the two components of the double. Because the star nearer to the Earth should, in the course of a year, appear to move around an ellipse, the direction of the imaginery line could vary to a large extent. The effect would be particularly obvious if the double were chosen in such a way that at the mean position of the nearer star the imaginery line pointed to the north ecliptic pole (that is, perpendicular to the ecliptic). Obviously also, the closer in angular distance that the two components were, the more pronounced any variation would be. Herschel explained this in the following way:

"To estimate the extent of this variation ... conceive two stars so situated as to have their apparent line of junction in the direction of a secondary to the ecliptic, and therefore at right angles to the major axes of their parallactic ellipses - let their distances from us be such that the nearer one shall have a parallax of 1", and the farther no appreciable amount of it.

Also, let their apparent angular distance from each other be 5". It is evident that the variation alluded to will equal the angle subtended by a line of 1" in length, at a point 5" distant from its middle, that is, to $11^{\circ} 25'$ ". (49)

For closer components the angle could vary by an even greater amount: up to 53° for a pair only 1" apart, assuming the nearer had a parallax of 1". Angles as large as this would be very easy to detect; and as astronomers carrying out this project would be observing doubles, all the advantages pointed out by William Herschel in 1781 would still apply. As John Herschel wrote, to put his plan into practice, "a good telescope and a good micrometer [were] all the instruments required".

However, despite providing a lengthy mathematical investigation into how to determine the times of year at which it would be most appropriate to observe particular double stars and a table showing the results of these investigations for over 70 examples (50), Herschel had to excuse himself for not actually analysing any of his data. His reason was lack of time; but he was eager for other astronomers to exploit his new method and claimed he would expect no credit if anyone were to detect parallax in this way. However, none of his contemporaries took up the idea. To carry out his proposals as fully as he suggested would of course have been a very time consuming occupation: the data Herschel and South already possessed, although plentiful, was not in an appropriate form. Herschel's method required the observation of the double in question at the 6-monthly intervals calculated, over a period of several years. He and South did not have a sufficient number of observations at the right times of year for any of the stars Herschel had singled out. Precisely how vast an undertaking Herschel had suggested was appreciated when, over 15 years after the paper was published, the long-serving secretary of the Royal Astronomical Society, and President of the Royal Society

from 1854 to 1858, Lord Wrottesley, set out to work the project through. After almost 10 years of observing he presented his results to the Royal Society (51), pointing out the difficulties of comparing observations made at different seasons of the year, and explaining that many of the stars selected by Herschel from theoretical considerations lay too near the horizon to be observed accurately. Nevertheless, Wrottesley decided to observe some of Herschel's stars in the hope of detecting their parallaxes. By the 1840s of course, he had the advantage of knowing that acceptable values of parallax had been produced; in 1826 no one could know that, and consequently the prospect of putting Herschel's theory into practice would have been extremely daunting, and one with very little hope attached of producing reliable results.

Despite astronomers' disinclination in the late 1820s to seek the parallax of double stars, interest in the stars themselves - in the movements of the components relative to each other, in their colour and their apparent magnitude - persisted and grew. Struve, South and Herschel were still the astronomers mainly involved, but they had deliberately brought the subject to a wider audience. By the end of the 1820s it had become important to establish whether the motions of double stars were governed by the inverse square law: did Newtonian mechanics extend beyond the solar system? If so, how many observations of a particular double were needed to calculate reliable elements for its orbit? If not, what law did describe the motion? Such questions were asked by Felix Savary, Professor of Astronomy at the Ecole Polytechnique (52), and by Gauss's most influential assistant at Göttingen Johann Franz Encke (53). The pages of the Memoirs of the Royal Astronomical Society and the Monthly Notices of the Royal Astronomical Society for the early 1830s were littered with papers attacking such problems, and discussing the difficulties

of obtaining observations which were sufficiently accurate to be of use (54). In Königsberg, Bessel, swayed by the work of Struve and of South and Herschel, started to make serious observations of double stars and to analyse their motions. In volume 10 of his observations, published in 1825, he included a catalogue of doubles (55); then in 1833, after he had clearly devoted some time to the study of doubles (and in the meantime acquired an excellent measuring instrument from Fraunhofer; see below, section 6.3) he produced a detailed paper comparing Struve's, Herschel's and his own observations, and discussing the possible orbits of double stars (56). Later the same year he supplemented this paper with a further discussion of the orbits of specific stars, among them 61 Cygni (57).

By the early 1830s, therefore, the study of double stars was established as a substantial part of the work of several major European observatories, and its establishment was a significant factor in the emergence of sidereal astronomy as an enterprise in its own right. It was also important within the specific problem of the detection of stellar parallax: as we have seen William Herschel's initial aims in observing doubles were still remembered during the 1820s, if a little hazily. In 1812 Bessel had made a definite connection between the problem of parallax and the particular double star 61 Cygni, and this connection was extended and crystalised in the extensive work on doubles by Struve published in 1837: his Stellarum duplicium et multiplicium mensurae micrometricae It was in this that Struve was to publish a value of parallax for Vega, which was subsequently accepted as one of the first genuine measurements. That part of the book will be analysed below (see section 6.4); what must be discussed at this point is Struve's account of the selection of doubles for parallactic investigations.

It is well known that in the Mensurae micrometricae

Struve outlined three criteria which could indicate that a star was nearby: brightness, possession of a high proper motion and, for a binary star, a high ratio of the angular separation of the components to the period of revolution. The significance of this list has, however, been overestimated by historians. The provision of such a list has been viewed as a development which had to occur before parallax could be sought successfully (58), but this is not necessarily so. Struve's account is highly theoretical, particularly his discussion of the third criterion (which is in fact very similar to Bessel's 1812 analysis). Struve did conclude with lists of specific stars which might be suitable to study for parallactic variations, but the particular star he chose, Vega, was not the most likely candidate on any but the first criterion. His reasons for studying Vega will be considered below; the important point to note here is that although Struve clearly understood and appreciated the theoretical advantages of observing certain doubles, this did not greatly influence his practical attempt to detect parallax. His 1837 publication did not affect Bessel's choice of δ Cygni either: as pointed out above Bessel had been fascinated by δ Cygni for many years and always believed it must lie comparatively near the Earth, because of its high proper motion. The chapter in the Mensurae Micrometricae on parallax was largely ignored by astronomers at the time of its publication, and therefore the significance at that time of Struve's clarification of selection criteria was minimal.

On the other hand, taken as a whole, Struve's work on double stars was extremely important, particularly in the context of the development of interest in the stars as individuals. In fact Struve's, Herschel's and South's work was a major manifestation of this development and their endeavours did much to focus the attention of their contemporaries on the study of doubles. Given the establishment of double-star

astronomy as a respected part of the activities at major observatories, together with the continued refinement during the 1820s of positional astronomy, a reasonably clear picture emerges of how various factors came to dominate positional astronomy in the mid-1830s. The combination of these factors, and the presence of particular astronomers was sufficient to produce at least one believable value of parallax. To complete the picture, therefore, it is necessary next to consider the refinement of positional astronomy.

6.3 Improvements in precision

Having sketched the development of interest in double stars, it is necessary now to assess the technical improvements in instrumentation and data reduction which were taking place at about the same time. As far as the instruments are concerned, one name stands out above all the rest: that of Joseph Fraunhofer. His instruments were universally recognised by his contemporaries to be superb. Moreover, he has a specific importance within the history of attempts to measure parallax since both Struve and Bessel used Fraunhofer instruments for their respective measurements which led to the identification of the phenomenon.

Fraunhofer is perhaps most famous in instrument history for his devotion to the solution of problems of aberration - spherical and chromatic - in the optical systems of telescopes (59). However, although perfection of the optics was of course of fundamental importance in positional astronomy (any blurred image would introduce a number of errors into the observation of exact positions), this was not Fraunhofer's only contribution of specific significance to the history of attempts to measure parallax. The nature and importance of his non-optical contributions tend to be overlooked by historians, but they were certainly appreciated and

discussed by the astronomers who used his telescopes. In order to assess them and their significance for parallax work, we must consider the accounts of the Dorpat refractor and the Königsberg heliometer written by Struve and Bessel respectively.

The first of the two instruments to be erected and used was the $9\frac{1}{2}$ " Dorpat refractor, which was in operation by early 1825. Struve subsequently wrote a detailed description in the form of a monograph (60), and also sent accounts of the instrument in letters to von Zach and to Francis Baily, a founder member of the Astronomical Society of London (61). Struve outlined his initial reaction to the telescope by writing that he

"... stood astonished before this beautiful instrument, undetermined whether to admire most, the beauty and elegance of the workmanship in its most minute parts, the propriety of its construction, the ingenious mechanism for moving it, or the incomparable optical power of the telescope, and the precision with which objects [were] defined". (62)

The telescope was a "splendid masterpiece of art". As is abundantly clear, from the way in which the instrument was supported down to the minute detail of the scales, this telescope far surpassed anything else then in existence. The optics were first rate: Struve wrote that it would be difficult to find anything comparable in any existing telescope. But Struve did not emphasise the instrument's superior optical system over and above its other assets. His assessment of the optics was given after he had described the driving mechanism as "the principal feature of this instrument, and the greatest triumph for the artist" (63). By means of a simple system of weights and endless screws the telescope could rotate about a polar axis; as Struve explained the enormous advantage of this was that

"When the telescope [was] thus kept in motion, the star [would] remain quietly in the centre, even when magnified seven hundred times. At

the same time there [was] not the least shake or wavering of the tube, and it seemed as if [he] were observing an immoveable sky". (64)

Astronomers' new ability to track stars accurately was of paramount importance for the development of sidereal astronomy, and of particular relevance to the study of double stars. If more was to be learnt of the stars than just their positions it was essential to be able to examine them through the telescope for longer than the time taken to make a transit measurement. It can hardly have been accidental or coincidental that telescopes were for the first time driven effectively about a polar axis precisely at the time when astronomers were beginning in all earnest to explore the sidereal heavens. Specifically, when observing double stars two readings had to be taken while both components remained within the field of view: the distance (that is the angular separation) and the angle of position (that is the orientation of the pair with respect to the meridian). At high magnification the speed of the double through the field of view would be considerable, and this problem had been of great concern to William Herschel in his original survey of double stars (see above, section 3.4). With Fraunhofer's driving mechanism the problem was very neatly solved.

The advantage of this particular innovation was only implied in Struve's account of the Dorpat refractor, but it was stressed explicitly by Bessel when, in 1830, he wrote a description of the Fraunhofer instrument in Königsberg: the heliometer was completed after Fraunhofer's death and erected in 1829. After outlining the limitation on micrometrical measurement enforced by the diurnal motion, Bessel wrote that

" ... Fraunhofer has removed the causes of this difficulty by providing telescopes with clock-work, which communicates a constant motion equal to the diurnal motion, so that the stars do not change

their positions relatively to the wires of the micrometer, and, therefore, can be compared by means of it with the required absence of motion. How well this has succeeded, is shewn by the excellent measurements of distances and angles of position of double stars which Struve has obtained with his great telescope so furnished, and of which he has already published some proofs". (65)

The fact that Bessel included this passage in an account of a heliometer demonstrates that he was very well aware of the importance of Fraunhofer's new clock-work mechanisms. For, as he pointed out, the type of measurements made with a heliometer are unaffected by the diurnal motion: before the introduction of telescope drives the indifference of heliometers to diurnal motion was one of their main advantages over all other measuring instruments (66). This being so, why discuss clock-work drives at all? The answer is, few observatories possessed a good heliometer: they were difficult to construct optically, and at the main European observatories while positional astronomy continued to be the most important type of work there was little call for such an instrument (67). Fraunhofer's invention of a mechanism which made possible the accurate tracking of images and which could be used with any suitably mounted telescope was of great importance. All sorts of instruments could subsequently be used to measure small angular separations, with a suitable filar micrometer, or to study particular objects over a period of time. In his comments on the advantages of Fraunhofer's mechanisms, Bessel was keen to emphasise the former aspect; the whole point of his mentioning it derived from his wish to convince his readers that the accuracy of micrometrical measurements was significantly improved by using micrometers in conjunction with the clock-work drive mechanism.

In the light of the above remark - that heliometers were not commonly used at observatories - the question which must be answered is why did Bessel order a

heliometer from Fraunhofer in preference to any other instrument? This can be answered partially by appealing to Bessel's own account; he began his description thus:

"The problem of finding the relative position of two points on the surface of the sphere of the heavens is, in modern astronomy, commonly solved by the use of meridian instruments. These instruments afford, for the most part, a solution unrestricted by the greater or less distance of the points to be compared, and may always be employed when the power of the telescope is sufficient to render the object to be observed visible at the time of its culmination. Though such instruments possess means of solving the problem in all its generality, yet we should frequently sacrifice somewhat of the accuracy of the observation, if, whenever the relative position of two points is required, we were always to employ this most general method of observing". (68)

Bessel was therefore advocating the use of specialist instrumentation for specialist work. His particular speciality at this time was the measurement of the angle between two points on the celestial sphere. Micrometers were of course used to make such measurements, but these instruments

"... remain so imperfect, that astronomers, not being in a condition to make effectual use of them, have preferred grounding determinations on meridian observations, which a good micrometer would have given much more advantageously". (69)

One of the greatest problems with the micrometer was that before it could be used the two objects had to be simultaneously within the field of view of a single lens system; this greatly restricted its applicability. Also, the wires of the micrometer had to be illuminated, so it could not be used effectively for very faint objects (for example the measurement of comets when they first became visible through the telescope). Each of these problems was avoided by using a heliometer. Because of its double optical system, the angular separation of the objects to be measured could be as

large as one degree, and was independent of the magnification of the eyepiece; and since the principle of the instrument was based on the superposition of two light sources, no other illumination of the field of view was necessary. These advantages were each explained in detail by Bessel, but he was not explicit about which particular angles he wished to measure, nor, specifically, what had prompted him to order a heliometer from Fraunhofer. However, we know that Bessel ordered the instrument sometime in the middle of 1824, and from his published papers and his correspondence we have some idea of the projects upon which he was working at that time. Apart from his usual concern about the accuracy of observations and their reduction, which was obvious throughout his life, in the mid-1820s he was interested in the compilation of stellar charts and in planetary perturbation theory. Neither of these interests dictated the need for accurate measurements of small angles. However, it was in 1824 that he began his survey of double stars, which culminated in the catalogue of about 300 such objects, published late in 1825. That would obviously have involved the repeated use of micrometers and would undoubtedly have brought their failings to his attention. Also, when Bessel came to write his account of the new heliometer, one of the uses which he described in detail was the measurement of doubles (70). He did not use it only to measure doubles, but it is likely that this was one of its more important functions (71), and Bessel's wish to improve such observations may well have been a significant factor in his decision to order a heliometer from Fraunhofer.

The precise way in which Bessel used the instrument in the years 1837-8 to observe 61 Cygni will be discussed below; all that need be stressed at this point is that by 1830 both Bessel and Struve possessed instruments which were by far the best in existence, and that they had each shown a great deal of interest in the study of double stars. The final development to be considered,

before an analysis of their respective measurements of parallax is undertaken, is Bessel's improvement and refinement of the reduction of stellar positions outlined in the Fundamenta Astronomiae. His work in this area culminated in the publication, in 1830, of the Tabulae Regiomontanae; this exceptional book must now be briefly discussed.

As in the case of the Fundamenta Astronomiae, the Tabulae Regiomontanae is very much a complete piece of work, and it is difficult to assess the significance of individual sections. Moreover, unlike in the brief analysis of the Fundamenta given above in section 5.3, we cannot focus attention on a section dealing with parallax, because the phenomenon is not considered in Bessel's later book. This absence of any mention of parallax is very significant for the overall history of attempts to measure the effect; clearly when Bessel compiled the Tabulae Regiomontanae, he did not believe parallax would affect his observations sufficiently to warrant its inclusion. In 1830, therefore, Bessel presumably felt that positional astronomy had still not yet become precise enough to make possible the detection of a measureable parallax. Nevertheless, the Tabulae Regiomontanae played an important rôle in the history of attempts to detect parallax not least because it was Bessel's last major publication on positional astronomy before his identification of annual stellar parallax.

Although Bessel did not consider the problem of parallax in the Tabulae Regiomontanae, he did review his work on precession, proper motion (which had not been treated thoroughly in the Fundamenta), nutation and aberration. His treatment of precession and nutation was (and still is) exemplary: it is from the Tabulae Regiomontanae that the terms 'Besselian day numbers' and 'Besselian star constants' derive.⁷² To carry out the analysis of his data Bessel used the same basic method as he had for the reduction of Bradley's observations: he expressed the values of astronomical constants within limits

and calculated those limits by applying the method of least squares to his data; and from the same calculations he obtained the most probable position of the star in question at a particular epoch. The single, most important feature of the Tabulae Regiomontanae was not the method employed, but the sheer quantity of data to which Bessel applied it. As early in his career as 1809, when he had to decide which of Bradley's data to analyse for parallactic effects, he had chosen those for which he had the greatest number of observations. At that time he was of course limited to the number Bradley had recorded; however, once Bessel was director of his own observatory he could ensure that as many data as he needed were gathered, weather permitting. During the 10 years between the publication of the Fundamenta Astronomiae and the preparation of the Tabulae Regiomontanae he accumulated a tremendous wealth of material, and was absorbed particularly in the problem of fixing very accurately the positions of 36 'fundamental' stars. The purpose of establishing these positions was to make easier the task of charting the whole sidereal heavens. It was the British Astronomer Royal Nevil Maskelyne who, in the late eighteenth century, originally conceived of the idea of selecting certain stars and observing them closely and repeatedly to form a reference network against which all other celestial positions could be measured (73). The idea clearly appealed strongly to Bessel, and he devoted much of his time during the 1820s to the observation of 'fundamental' stars. In the Tabulae Regiomontanae the majority of the tables are of the positions of these stars reduced to ten-yearly epochs through the period 1750 to 1850. Again the most important feature of the whole project was Bessel's insistence that he started off with a sufficient number of observations to make his reductions meaningful. Because, by 1830, he had so many more observations at his disposal than he had had during his preparation of the Fundamenta Astronomiae, he was able to calculate

all the astronomical constants he needed more accurately than at any time before, and hence his reduced observations were the most reliable ever to have been produced. The Tabulae Regiomontanae became the standard reference book for astronomers throughout Europe.

Thus, by the early 1830s the state of positional astronomy was such that all the ingredients necessary for the successful identification of parallax were present. There was a small number of exceedingly skilful observers, who possessed excellent telescopes which could consistently provide data accurate to hundredths of a second of arc; the need for a vast number of observations was recognised and the means for analysing these data in such a way that they produced statistically significant results had been developed. Finally certain astronomers had clear ideas about which stars might be close to the Earth and, therefore, worth observing in the hope of detecting parallax. This statement is of course easy to make with hindsight; what remains to be done now is to explain why in the mid-1830s certain astronomers again turned their attention to the problem of parallax, and how and why this new interest resulted in the provision of acceptable values.

6.4 The three accepted measurements

In 1841 F. W. Bessel was awarded the gold medal of the Royal Astronomical Society for his outstanding achievement in having measured the distance from the Earth of the star 61 Cygni (74). The value he obtained was first published in two French journals late in 1838; early the following year a detailed account of Bessel's study of the star appeared in H. C. Schumacher's Jahrbuch for 1839, and the letter Bessel had sent to John Herschel was published in the Monthly notices of the Royal Astronomical Society and soon after in the

Philosophical Magazine (75). Across Europe Bessel was acclaimed for having finally overcome all the problems which had beset astronomers for the previous three centuries in their attempts to measure annual stellar parallax. Subsequently it transpired that F. G. W. Struve had in fact, in 1837, announced a value for the parallax of Vega which could well be genuine (76); and late in 1838 the Scottish astronomer Thomas Henderson wrote a paper which was read to the Royal Astronomical Society in January 1839, and which contained Henderson's calculated value of parallax for the bright southern star α Centauri (77). Thus in the space of less than two years, three separate values of parallax were announced to the astronomical community and were accepted, albeit with varying degrees of conviction.

Historians have paid more attention to this final chapter of the history of attempts to measure parallax than to any other part of that story (78). But much remains untold. The emphasis in the secondary literature is on the question of priority, and even this issue has not been satisfactorily settled, because no one has established what in this case is meant by priority (79). In fact the problem of priority is not very interesting historically and it will not be considered any further. It is far more important and revealing to explain how and why each value of parallax was obtained, and to analyse the interactions between the astronomers directly concerned and between those astronomers and their audience. It is very significant that the three announcements were made within so short a time, and that they were believed; both these aspects of the story demand an explanation. The question of acceptability will be discussed in the next section; before that the sequence of events leading to the separate publications will be outlined, and the contents of the relevant papers analysed.

As has been made clear, in 1830 Bessel did not consider that the parallax of any of the 36 'fundamental' stars tabulated in the Tabulae Regiomontanae could amount to a significant value. However, from his correspondence with Olbers, we know that he again considered the problem of parallax, if only briefly, sometime during 1830. In an exchange of letters between November 1830 and March 1831 the advantages of using a heliometer specifically for the measurement of parallax were mentioned by one and confirmed by the other (80). The idea was suggested by Olbers in his response to Bessel's description of the Königsberg heliometer, and the latter immediately perceived how appropriate the scheme was. It is perhaps significant that Bessel's reply to Olbers' suggestion followed immediately after Bessel had listed a number of observations of double stars made with the heliometer. However, although Bessel continued through the early 1830s with his observations of doubles, he did not attempt to measure parallax until September 1834; and, indeed, we only have his word, written over four years later, that he did review the problem in 1834 (81). According to Bessel's account he tried originally to compare the components of 61 Cygni with two faint stars (of the 11th magnitude) lying near 61 Cygni - one lying to the north of the double and the other preceding it in right ascension. This method of gathering the required data combined brilliantly the advantages of observing binaries and optical doubles. Bessel also enhanced his chances of detecting parallax by comparing 61 Cygni with two faint markers rather than the one generally suggested in the literature considering the possibility of using optical doubles to detect parallax. Unfortunately, it was not long before he

" ... perceived that the atmosphere was seldom sufficiently favourable to allow of the observation of stars so small; and, therefore, [he] resolved to select brighter ones, although somewhat more distant". (82)

After this initial disappointment, Bessel was away from Königsberg for some time, staying temporarily in Berlin to carry out experiments on the length of the seconds pendulum there (83). 1835 saw the return of Halley's comet and Bessel was very much preoccupied with that; then he spent much of 1836 on the geodetical problem of calculating the length of a degree across the land around Königsberg. Clearly then, although Bessel was once more interested in the question of parallax, it was still not a pressing problem for him. As he was quite prepared to admit, the immediate cause of his renewed concern in 1837, and his subsequent determined effort to measure the parallax of δ Cygni, was his reading that year of Struve's study of Vega. This is thus an appropriate moment to look closely at Struve's claim.

First, some explanation must be attempted of Struve's interest in the problem: why did he write chapter 14 of Mensurae Micrometricae? There are no specific clues in the chapter itself; Struve gave a selected history of previous interest in parallax, and a comparison of photometric estimates of stellar distances with those obtained from a consideration of dynamical parallaxes and of geometrical parallaxes. To answer the question of why he wrote about parallax, the Mensurae Micrometricae must be considered as a complete work. Struve had spent 12 years gathering his data, and after such an enormous undertaking he was determined to produce a memorable and important document. The Mensurae Micrometricae was clearly designed to be the definitive work on double stars. If the contents pages are studied it can be seen that every possible aspect of the study of doubles was discussed in detail in the book: the instruments best suited for the observations, the magnitude of doubles, their colours, proper motions, orbital motions and finally their use for parallax measurements. Struve included the chapter on parallax for the sake of completeness: he could not

have omitted it, particularly since the connection between parallax and doubles had, in the past, been very important.

The discussion of parallax was, as remarked previously, basically technical and mathematical rather than practical, although he did provide a list of stars which he believed might have detectable parallaxes. When he turned to his observations, however, he chose to study those of Vega, which only appeared in his first category of bright stars. The reason he gave for concentrating on this star was that

" α Lyrae, the most brilliant [star] in the northern sky, is very suitable for the determination of parallax, because its great distance from the equator means, that for those ... who live in the north, this star never sets, and that in every season of the year [they] are permitted to observe it at night at a great altitude". (84)

Plainly then, Struve was guided by the criteria of high transit altitude, of the visibility of the star throughout the year, and of brightness. The first of these can only have been governed by consideration of refraction: the criterion which dates back to the decision of Hooke and his contemporaries to observe γ Draconis. The wish to avoid the problem of refraction was engrained in the minds of all astronomers by the early nineteenth century, despite the many theoretical and empirical attempts to solve the problem. Nevertheless it is odd that Struve should have been worried about refraction in this particular project since he was observing an optical double and hence the difference in position between two close objects, in which case refraction is unimportant. The second point, that Vega is always visible, might indicate that Struve was concerned about the number of observations he could gather; but when he came to do his analysis, he used only 17, which from 12 years of observations is not many. We are left therefore with the conclusion that Struve chose to observe Vega because it is bright, a criterion which dates back specifically to Jacques Cassini's selection

of Sirius in the early eighteenth century and probably even earlier. In which case Struve's celebrated "solution" of the problem of selection criteria was not applied even to his own work (see above, section 6.2).

The result Struve obtained for the parallax of Vega - $0''.125$ with a probable error of $0''.055$, from 17 observations - was not triumphantly announced. It was tentatively suggested amid assertions that more observations of the star were needed before a more positive claim could be made (85). He gave a detailed mathematical analysis of the geometry involved and of his reduction of the data, but neither added conviction to his answer. The result was mentioned only briefly in the French synopsis of the Mensurae Micrometricae which was published in the same year as the original (86), and it was not mentioned at all in the advanced notice of the book which appeared in the Astronomische Nachrichten in mid-1837 (87). All of which suggests that Struve was far from convinced himself that he had genuinely detected parallax. His result was certainly passed over by most other astronomers at the time of its publication.

However, as Struve's descendent Otto Struve was at great pains to point out, the result was not ignored by Bessel (88). Its appearance caused him to devise a long-term project in which he intended to attack the problem of parallax rigorously. He reviewed his idea of observing the position of δ Cygni relative to two faint markers, but after his previous difficulties he chose this time to measure the distance and position angle between the mid-point of δ Cygni and two stars which were slightly brighter than the two used in 1834, but slightly further away (in angular terms) from the main star. In figure 2, if C_1 and C_2 are the components of δ Cygni and a and b the marker stars, the measurements made by Bessel were of the arc lengths $\underline{OA}$ and $\underline{OB}$, and of the angles θ_1 and θ_2 .

The programme of observations began in August 1837, and over the following 14 months Bessel took measurements involving at least one of the faint stars, and usually both, on over 100 occasions. Each individual observation was, he claimed, repeated at least 16 times in an evening and he made more observations when the atmosphere was, as he phrased it, "unusually unsteady" (89). As usual, it was to Olbers that he sent the first description of his project; he explained the details in a letter written in October 1837, and included a table of the first two months of observations (90). By the summer of 1838, less than a year after he began his study, Bessel was fairly sure that he had good evidence of parallax and wrote of this to Olbers in June of that year (91). However, he wished for further confirmation before he would publish and hence continued to make observations until early October. Then on 9 October he wrote to Olbers announcing positively that the parallax of δ Cygni was $0''.3136$ with a mean error of $0''.0202$. Within the following month he wrote also to Schumacher, John Herschel and Alexander von Humboldt, and each of these letters was published either late in 1838 or early in 1839 (see ref. 75).

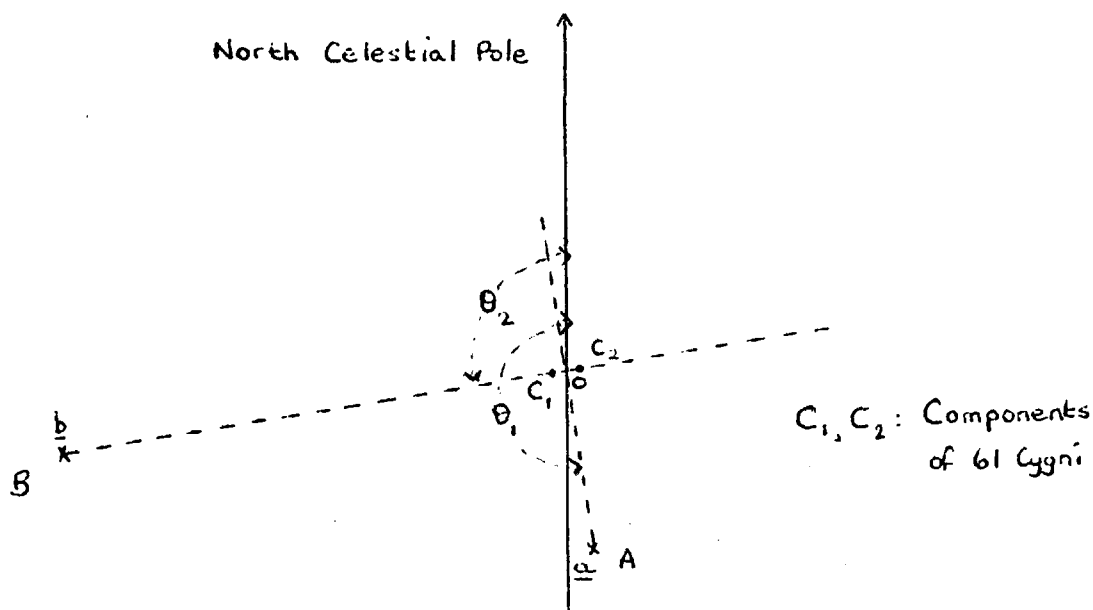


Figure 2 - Bessel's measurements of δ Cygni.

Bessel's most detailed account of the project was that in his letter to Schumacher: the account which was clearly intended for publication. The geometry involved and the technical details of his reductions were all explored in depth, and Bessel provided several pages of observations. They were all reduced to the epoch 1 January 1838: that is, Bessel calculated the values of $\underline{OA}$, $\underline{OB}$, Θ_1 and Θ_2 as they should have been on that date, by working out how the positions had been affected by precession and nutation, and then allowing for refraction and aberration. Because Bessel was happier with the accuracy of the heliometer's measurements of distance than those of position angle he chose to concentrate on the former. He equated each distance as follows:

$$\text{Distance for star } \underline{a} \text{ (}\underline{OA}\text{)} = \alpha + t\alpha' + \alpha\alpha''$$

$$\text{Distance for star } \underline{b} \text{ (}\underline{OB}\text{)} = \beta + t\beta' + \alpha\beta''$$

where α, β were the values of $\underline{OA}$ and $\underline{OB}$ for 1 January 1838 (92),

α', β' the errors in the differences between the proper motions of 61 Cygni and of $\underline{a}$ and $\underline{b}$ respectively,

α'', β'' the differences between the constants of parallax for 61 Cygni and for $\underline{a}$ and $\underline{b}$ respectively,

t the time (in days) between 1 January 1838 and the date of observation

a the coefficient of parallax: that is, the multiple of the parallactic constant which varies through the year according to the position of the Earth in its orbit.

Bessel's use of these particular functions in his analysis demonstrates his awareness of even the tiniest effects which could possibly alter his results. He did not take it for granted that the two faint stars were sufficiently far away to have no sensible parallax, and no sensible proper motion. Instead he allowed them parallax and proper motion (through his use of the terms $\alpha', \beta', \alpha''$ and β'') and proceeded to show that the errors in his reductions were minimised if $\underline{a}$ and $\underline{b}$ were

assumed to be very far away. The minute details of his use of the method of least squares need not be documented here (it involved the analysis of over 80 equations dealing with the measurements relative to star a, and over 90 with star b), but his conclusion was enormously important. Bessel explained that he had

" ... deduced a second result from the observations, which rests on the supposition that the parallaxes of a and b are insensible; or that ω and ϕ are equal ... and hence the most probable value of the annual parallax of 61 Cygni = $0^{\circ}3136$... and the mean error of the annual parallax of 61 Cygni = $\pm 0^{\circ}0202$ ". (93)

It was a result which brought him acclaim throughout the astronomical community.

Reaction to Bessel's account will be considered in detail below, but it is important here to note the names of the astronomers Bessel decided to inform of his work. Naturally he wrote first to Olbers, as he always did before making public the results of his researches. He also promptly sent a report of his results to the Berlin Academy of Sciences. The definitive account was prepared for Schumacher as it was he who would publish it to the astronomical world in the German states. Having written to Schumacher there is no evidence that Bessel wrote to any other astronomers in Germany until after his paper had appeared; when he did mention it in subsequent letters it was very much as a minor point (94). Also, there is no evidence that he wrote to inform Struve of his success, despite his willingness to acknowledge that it was Struve's result which had re-kindled his own interest. Although Bessel was not particularly concerned about informing either the astronomers in his immediate circle or Struve, he was clearly anxious to inform astronomers in other countries; in fact he wrote to John Herschel before writing to Schumacher. Bessel was worried that he might be misunderstood and he chose Herschel to write

to because he could then "write in [his] own language, and thus secure [his] meaning from indistinctness" (95). Presumably for the same reason, it was to Humboldt that he wrote his account for the French astronomical community. Moreover, both John Herschel and Humboldt were known personally to Bessel and perhaps he hoped for a sympathetic reading of his work from them. Whether Bessel felt that his results might not be believed or that his analysis would not be understood by astronomers in Britain and France is unclear. Perhaps he was afraid of both. In comparison with the English and French accounts the original German text is very detailed and in places difficult to follow. The overall impression of Bessel's position is that by the end of his project he was very sure of his claim; he was prepared to explain his work in great detail to those few he believed would understand it (his German colleagues) and merely to inform other astronomers of the results together with the more important details. It is possible that he was surprised at the enthusiasm with which his work was greeted outside his own circle of astronomers.

However, before the aftermath of Bessel's announcement is reviewed, some account must be given of the third claim to have detected parallax, made by Thomas Henderson at the end of 1838. By then Henderson was Astronomer Royal for Scotland, but his work on α Centauri had started a number of years earlier when, from 1832 to 1833, he was director of the observatory at the Cape of Good Hope. While at the Cape, when he was not simply deploring every aspect of his existence there (96), he gathered as a matter of course several observations of the declination of the bright star α Centauri. According to the letter he sent to the Royal Astronomical Society at the end of December 1838, on his return to Edinburgh he learned of the high proper motion of α Centauri - about 3.6" per year in declination - and consequently he decided to analyse his data in the hope of detecting its parallax (97). Because he had not known

of the high proper motion while he was at the Cape, he had not made observations particularly aimed at the detection of parallax, and had to rely on assumptions about the fundamental stars relative to which he had measured the position of α Centauri. However, the reduced observations apparently exhibited a "sensible" parallax; but Henderson was reluctant to publish before he could confirm his results with observations of the star's right ascension. Once confirmation was at hand, he wrote to the Royal Astronomical Society outlining his reductions both of declination and of right ascension.

In his account Henderson gave certain details of his calculations; for example, the values of the constants he used, and tables of reduced observations from the period April 1832 to May 1833. But, for no obvious reason, he appears to have changed the value he assumed for the constant of aberration. He gave no account of his precise application of the method of least squares, and the results he obtained were mixed:

	α_1 Centauri	α_2 Centauri
parallax in right ascension	$0^{\circ}92 \pm 0^{\circ}35$	$0^{\circ}48 \pm 0^{\circ}34$
parallax in declination (direct) (98)	$1^{\circ}42 \pm 0^{\circ}19$	$1^{\circ}05 \pm 0^{\circ}18$
parallax in declination (by reflection)	$1^{\circ}96 \pm 0^{\circ}47$	$1^{\circ}21 \pm 0^{\circ}64$
weighted mean	$1^{\circ}38 \pm 0^{\circ}16$	$0^{\circ}94 \pm 0^{\circ}16$

The most notable anomaly in them was the difference between the results obtained for α_1 Centauri and those for α_2 Centauri. α Centauri was of course assumed to be a binary star because the two components share a common proper motion, but Henderson was not daunted by the discrepancy of his results. He completed his paper by writing that

"If we suppose that the two stars are at the same distance, then the parallax = $+ 1^{\circ}16$, with a probable error of $0^{\circ}11$ ".

and hence,

"It therefore appears probable, that these stars have a sensible parallax of about one second of space". (99)

Like Struve, Henderson was keen to stress that his observations were not sufficiently adequate to be incontrovertible, and he urged his successor at the Cape, Mr Maclear, to continue observing α Centauri and hence to settle the matter. His results as they stood were not at all convincing.

Fitting Henderson's measurement of parallax into the picture we already have of the successful identification of the phenomenon is not easy. He was not explicit about dates or about what he had read. In his opening paragraph he referred to the brightness and high proper motion of α Centauri which made it a suitable object to study, and it has been suggested that he was prompted to carry out his analysis by reading Struve's Mensurae Micrometricae (100). But although this is possible, it is equally possible that he had heard of Bessel's work or, even more likely, of a paper by the English Astronomer Royal George Airy, who in November 1837 announced to the Royal Astronomical Society that he could not detect any trace of parallax for Vega (101). However, this is mere speculation, and perhaps of little relevance. It is fair to say that Henderson was lucky to have observations of one of the solar system's nearest neighbours; if α Centauri were not so close, the accuracy of Henderson's observations would not have been good enough to show any sign of parallax. But Henderson does deserve some credit for recognising that the star might be near and for persisting with his calculations despite the relatively poor quality of his data. His results were not very convincing, and the important aspect of Henderson's work historically is the way in which it was received, particularly in Britain and in the light of Bessel's announcement.

6.5 Why were they believed?

This is not a trivial question. For over 160 years, even if only sporadically, astronomers had supplied their contemporaries with values of annual stellar parallax which had all, sooner or later, been rejected. As we have seen the number of claims increased dramatically during the early years of the nineteenth century, but they were still not accepted. All that happened was that the upper limit which would be accepted for parallax decreased and the corresponding lower limit for the distance of the nearest stars increased. But when Bessel wrote in 1838 on the parallax of 61 Cygni there were no dissenters. Of course with hindsight it is easy to say that Bessel's value was "right", or that it agrees quite closely with modern values obtained photographically (102). But by which criteria did Bessel's contemporaries judge his work? Furthermore, did the reaction to his account influence astronomers' assessment of the other two claims by Struve and Henderson, and if so, how?

In an attempt to answer such questions it is revealing to consider how astronomers' reaction, in the first instance, to Bessel's work varied from country to country. In Germany there was little public celebration. Bessel was certainly believed; it was as if his colleagues always expected him to succeed eventually in his attempts to detect parallax: if anyone were capable both of making the delicate observations and of having the patience to analyse them rigorously, it was Bessel. From the private correspondence between Bessel and his contemporaries it is clear that they were all delighted with his result, but again there is no evidence of their ever registering any surprise (103). In fact, only few of Bessel's contemporaries could appreciate fully the monumental task he had undertaken, or the elegance of his results. But Bessel wrote specifically for those few who could and he was content to know that they had.

The situation was different in France. Bessel's letter to Humboldt was first published in the Comptes rendus (104), and it appeared without any editorial comment. The letter was followed by a short letter by Arago, which had first appeared four years earlier in the report of the Bureau de Longitude for 1834 (105). In this Arago noted the results of observations of 61 Cygni made between the months of August and November 1812, made by himself and M. Mathieu, one of his assistants at the Observatoire (106); from observations of zenith distance they concluded that the parallax could not amount to more than $\frac{1}{2}''$ (107). Again there was no editorial comment concerning Arago's paper, but the juxtaposition of the two argues that the French wished to have their work taken into account. This was even more evident in the second French publication concerning Bessel's result, written by A. Gautier, director of the observatory at Genève (108). Gautier began with a description of the work of Arago and Mathieu, then introduced Bessel's paper as though its main importance was that it confirmed the Frenchmen's earlier measurements (109). He then gave a synopsis of the account in the Comptes rendus and concluded by emphasising that although much remained to be done in the enquiry into the nature of 61 Cygni, he and his colleagues should thank the Creator for allowing them to know as much as they did know about the stars. But he had little to say in praise of Bessel. There does not appear to have been any disagreement with Bessel's conclusions but there was no enthusiasm over them either.

Reaction in England to Bessel's account was quite the opposite; in 1841, specifically for his work on the parallax of 61 Cygni, he was awarded the highest honour of the astronomical community in Britain: the gold medal of the Royal Astronomical Society. During 1840 Robert Main, Airy's chief assistant at Greenwich, studied thoroughly the various claims by his contemporaries to have measured parallax and his conclusions were published

in the Memoirs of the Royal Astronomical Society for 1842 (110). Then, in February 1841 John Herschel announced the decision of the council of the Royal Astronomical Society to award their gold medal to Bessel (111). Herschel's address and Main's paper provide a reasonably clear picture of British reaction to all three claims to have detected parallax, but especially of that to Bessel's.

In particular, John Herschel's address provides the most obvious answer to the question of why Bessel was believed. Herschel was convinced primarily because the observations, when reduced, matched the theory so well. In other words, over a cycle of more than a year - although Herschel in fact thought this might not have been long enough - the distances between the mid-point of δ Cygni and the two stars a and b varied in ways which were accurately predicted by the theory of parallax. "And", wrote Herschel,

"the rules of philosophising will not justify us in referring the observed effect to an unknown and, so far as we can see, an inconceivable cause, when this is at hand, ready to account for the whole effect". (112)

Such a view was endorsed by Main, who plotted the values of distance given by Bessel against time. The resulting graph (see figure 3) was a particularly impressive demonstration of the close fit between theory and observation: maximum and minimum displacements were reached at precisely the times of year predicted by parallax theory.

Close agreement between observation and theory was of course an excellent reason for believing Bessel, but if Bessel could be believed on these grounds why could other, earlier claims not be? In particular, why were the results of the Irish Astronomer Royal John Brinkley, published at regular intervals during the 1810s and 1820s, dismissed as unbelievable? The main answer given by astronomers at the time was that the values Brinkley suggested were too big: this was one of the

only reasons they could give, for Brinkley always explained carefully why his observations could not be the results of any known phenomenon except parallax. Perhaps, therefore, the acceptance of Bessel's value had something to do with its being of the right, or expected order of magnitude. As we have seen, Bessel himself fully expected the parallax of 61 Cygni to amount to about $\frac{1}{2}$ ", and the agreement between the answer obtained from a consideration of the dynamical parallax and the measured value was commented on favourably by Olbers (113).

However, the fact that the angle Bessel had measured was of an acceptable size is not on its own sufficient to explain the general reaction to Bessel's claim. After all, it is only with hindsight that we can say that the largest known parallaxes are a small number of tenths of a second. Astronomers in the late 1830s had no means of establishing this. Even if they considered themselves to be justified in rejecting the work of Brinkley and others (including Piazzzi and Calandrelli) because their results were too big, they had no grounds for judging how small parallax might be.

The only other reason ever given for rejecting Brinkley's results was that provided by John Pond: that he had been unable to obtain similar results from observations made at Greenwich. It has been shown above (see section 4.3) that Pond's observations were dubious and that his understanding of the geometry of parallax was poor. Nevertheless his opinion may have influenced various of his contemporaries; he was after all Astronomer Royal and there is evidence, for example, that the President of the Royal Society, Sir Joseph Banks, believed him (see chapter 4, ref. 83). Yet the issue of the repeatability of Bessel's project was never raised; how could it have been? Very few astronomers possessed the skill, the time or the instrument, and certainly not all three, to carry out anything comparable. And, although Bessel wrote that he intended to confirm his results with further

observations, no one waited for confirmation before acclaiming his achievements. (Confirmation was forthcoming and Bessel wrote of it to Francis Baily, Secretary of the Royal Astronomical Society, in November 1839 (114)).

The question of repeatability becomes a little more complicated when the claims of Struve and Henderson are reconsidered. Both had stressed the need for their particular endeavours to be confirmed with new observations. However, when Struve published the results of his later study of Vega, he obtained the value $0''.2613$: that was, more than twice his original value (115). This certainly cast some doubts over the validity of his first claims; but Struve's work continued to be included among the first measurements, generally with a qualifying clause attached. And, interestingly enough, the fact that Airy had failed to detect any trace of parallax for Vega from observations made at precisely the same time as Struve's made no difference whatever to anyone's reaction to the claims from Dorpat. Turning to the confirmation of Henderson's announcement of parallax, when Maclear analysed his observations of α Centauri, taken over the year following Henderson's claim, he obtained consistently smaller values for its parallax than those originally calculated (116). Maclear's answers for the two components were also in closer agreement with each other than were Henderson's, all of which renders the original values open to question. But like Struve's claim, Henderson's was still accepted, if with a little caution.

Thus the repeatability of Bessel's work was not a necessary criterion of acceptance. Part of the credibility of his account must have derived from his reputation: anything he wrote would have been regarded as worthy of the attention of all astronomers. By 1838 he had behind him his two monumental publications on the reduction of

data: the Fundamenta Astronomiae and the Tabulae Regiomontanae; he also had many important papers to his credit and several volumes of observations. He was quite obviously widely revered by all in the astronomical community, and was known to be the authority on all aspects of positional astronomy. Since the identification of annual stellar parallax was very much a problem within positional astronomy, Bessel's word on the subject was unlikely to be challenged.

In addition to his own reputation, that of the Fraunhofer heliometer was also beyond doubt. This raises again the vexing question of the role of the instrumentation in the history of attempts to detect parallax. Was the successful detection of parallax merely a reflection of the standard of the instruments used? This is certainly implied in much of the secondary literature concerning parallax. However, although it is naively obvious that unless the instruments are sufficiently accurate, parallax cannot be detected, the converse is not true. That is, merely because the instruments are technically accurate enough to measure parallactic angles, this does not automatically mean that the evidence will be forthcoming. This is borne out clearly by the history of attempts to measure parallax. By about 1820 the best instruments in existence could measure angles consistently to $0''.1$; if the detection of parallax were simply a matter of observing a difference of about $0''.5$ to $1''.0$ in the position of a star at either end of a specified six-month period, why did the phenomenon remain elusive for a further 18 years? The obvious answer is that sufficiently accurate instruments by themselves are not enough. The astronomer has to be prepared to devote himself to a long-term programme of observing, he has to have a clear idea about which stars might be worth studying and finally he has to be able to reduce the data very accurately. It was because he fulfilled all

these requirements, and especially the last, that Bessel was so important. He appreciated that before a convincing result could be produced he needed a lot of data (117): hence the repetition of his observations so many times each clear evening and the continuation of his project for well over a year. Secondly, he made a positive decision, for clearly expressed reasons, to concentrate his effort on the study of one star: δ Cygni. Then, finally, and most importantly, he had developed a sophisticated method of analysis which allowed him to produce reliable and convincing results.

We are now in a position to reconsider John Herschel's assessment of Bessel's measurements in terms of his "rules of philosophising". Bessel's endeavours were not acceptable simply because he possessed observations which could, and therefore should, be explained by parallax. The "observations" themselves were remarkable for two reasons. Firstly, they were skilfully planned and gathered (many of Bessel's contemporaries commented on his skill as an observer); and, secondly, the observations were analysed so thoroughly.

With this in mind, the role of Struve and Henderson in the successful identification of parallax can be reassessed. Whenever their claims to have measured parallax are evaluated - from Main's paper onwards - astronomers and historians have been reluctant to say much about the significance or relevance of their work. They are always included in accounts of the final successful measurements of parallax, each with some qualifying statement. Of Struve it is generally, and justifiably, said that the fact that his second value for the parallax of Vega was roughly twice his first value cast grave doubts on the validity of the claim. Henderson is usually said to have been lucky to have possessed observations of the second nearest star, which when reduced yielded results that were fairly,

but not very, close to the accepted modern value. This also has some truth in it, although Henderson deserves credit for recognising that α Centauri was likely to be nearby. The question which actually emerges from a detailed study of each of their claims is whether or not either of them would ever have been remembered as "successful measurements" at all if it had not been for Bessel's far superior set of results for 61 Cygni.

The fact remains of course that Bessel's result had been published and accepted, and for the first time ever astronomers knew, and were convinced that they knew, the distance to at least one star. Having accepted that, it would have been fairly easy to accept other values of parallax, particularly if they turned out to be of a similar size to the result for 61 Cygni. During the 1840s C. A. F. Peters, one of Struve's young assistants at the new observatory at Pulkowa, set out to measure the parallax of a number of stars and in 1847 several of his results were announced by Struve (118). They all appear to have been accepted as genuine, although the results Peters obtained do not all match up with the values we accept now (119). This argues that to some extent astronomers wanted to accept that parallax had been measured; in the case of Bessel's work he made it easy for his colleagues to indulge their wishes, and after the barrier imposed by the problem of parallax had been overcome once, and so convincingly, it became increasingly easy to accept other values of parallax.

The successful identification of parallax occurred at a time when sidereal astronomy was truly becoming a very important branch of astronomy - although it must be remembered that one of the greatest controversies of the late 1840s concerned the search for Neptune - and the establishment of certain stellar distances was important for that. All the suggestions that Bessel

had made in 1812 about 61 Cygni could be closely investigated for that star and others. Astronomers who for years had proclaimed the precise nature of the study of astronomy at last had an example of the one measurement that had daunted them for so long. For a moment at least they had something to celebrate. Their feelings in Britain were best expressed by Herschel towards the end of his 1841 address to the Royal Astronomical Society, and it is worth quoting him at length:

"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 parts. 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. Let us rather accept the joyful omens of the time, and trust that, as the barrier has begun to yield, it will speedily be effectually prostrated. Such results are among the fairest flowers of civilisation ... " (120)

Chapter Seven

CONCLUSIONS

We are now in a position to reappraise existing accounts of attempts to measure annual stellar parallax. Clearly, the history of such attempts involves more than the compilation of a list of all publications ever produced on the subject. As should be apparent from the evidence given above, it is no longer sufficient to write this history in terms of the instrumentation; success in the detection of parallax did not rest solely on the development of sufficiently accurate instruments, nor were the problems encountered by astronomers on their way to ultimate success simply practical. Equally, the history of attempts to measure parallax should not be considered in complete isolation from developments in other branches of astronomy. Investigations into stellar parallax had their place within the larger study of astronomy, and one of the main aims of this thesis has been to explain what that place was and how it changed during the period 1660 to 1840.

The concept of change is crucial to our understanding of the history of attempted parallax measurements. All aspects of the study of parallax underwent changes of varying degree: the number of astronomers concerned, their reasons for considering the problems, the associated difficulties they perceived, the solutions they produced, the audience they wrote for, and, finally, what constituted acceptable evidence of parallax. The

fact that the nature of the problem of parallax detection changed with time must be stressed. It is not actually recognised in the secondary literature, but it is perfectly clear that Bessel's paper of 1838 was a far cry from Hooke's Cutlerian lecture, even though on the surface they were both about the same thing: the measurement of annual stellar parallax. Historians' failure either to explain or even to comment on this reflects the restricted way in which the history of attempts to measure parallax has always been written. As an enterprise, the detection of parallax has been isolated from other astronomical enquiries being carried out at the same time and, more often than not, by the same people. From the nineteenth century onwards the history of astronomy has been compartmentalised, and although such an approach has provided historians with a wealth of interesting, even essential, information, its interpretative power is limited. As a result, the chronology of attempts to detect parallax has been established for some time, but this chronology has never been fitted into any historical context.

The most effective way of moving away from, or beyond, simple chronology is to study the varying approaches of astronomers to the subject of parallax detection and to investigate how and why they changed. If we consider first astronomers' reasons for seeking the phenomenon, it is obvious that although they were always interested in obtaining stellar distances, this was not always their sole motivation. In the 1660s both Hooke and Wren felt the need for further evidence in support of the Copernican theory, not to convince themselves so much as to be used against "anti-Copernicans". They each wished to prove that the Earth moves and both perceived a measurement of parallax as a means of doing this. Hooke summarised his attempt as an endeavour

"to furnish the Learned with an experimentum crucis to determine between the Tychonick and the Copernican Hypotheses". (1)

Hooke's arguments were aimed primarily at Riccioli, whose Almagestum novum was the most detailed and comprehensive tract in favour of Tycho's system as against that of Copernicus to be published in the seventeenth century. Absence of parallax was in fact only one argument among 77 cited by Riccioli, and it is unlikely that a single measurement of parallax would have influenced many people's attitude to heliocentricity. The fact that one star among thousands exhibited an annual motion which could be explained by parallax would hardly be regarded as decisive by any anti-Copernicans. Nevertheless, the argument was an attractive one to Copernicans and the possibility of 'proving' the truth of the Copernican hypothesis by measuring stellar parallax was reiterated by Long and Maskelyne well into the eighteenth century. Also, as indicated in Chapter Three, it is far from certain that Manfredi accepted the Sun-centred universe without hesitation; his interest in possible annual motions of the stars seems to have derived from a wish to be convinced about heliocentricity one way or the other rather than any conviction (2).

By the early nineteenth century, however, we have evidence that astronomers definitely regarded the 'proof' of heliocentricity by a measurement of parallax as an outdated issue. Writing in 1817 John Pond declared that

"The question of parallax in a theoretical point of view, is scarcely of any importance, it is in fact of mere curiosity. The motion of the Earth has long since ceased to be a subject of controversy, and could a difference of opinion still be supposed to exist, the advocates of the Copernican system would derive but slender support from the discussion of such small variations as form the subject of this paper". (3)

By this stage there was a different motivation behind the wishes of astronomers to detect the elusive phenomenon.

Pond gave two reasons; the first was that

" ... with reference to the state of practical astronomy the case is very different; in the future history of this branch of the science, that period of time will always acquire a certain degree of celebrity, in which astronomical instruments shall have been brought to such a degree of perfection, as to exhibit distinctly the effects of parallax in the fixed stars, and to distinguish these from the variety of complicated oscillations to which they are perpetually subject". (4)

and the second:

" ... it is surely a subject of rational curiosity to ascertain whether the distance of the nearest fixed star can be numerically expressed from satisfactory data, or if it be really so immeasurably great, as to exceed all human powers either to conceive or determine". (5)

In other words, a measurement of parallax would illustrate how the practice of astronomy had attained a state of great precision, and it would also establish a finite minimum value for the distance of the stars.

Thus, reasons for measuring parallax were not as straightforward as has been implied in the secondary literature. It is also important to note that the underlying motives for wishing to know stellar distances changed. In the late seventeenth century it is apparent that sheer curiosity was the prime incentive. This should not surprise us: astronomers have always wanted to know how far away celestial objects lie. But no astronomer of this early period was specifically interested in the intrinsic properties of the stars; such interest was not really expressed before the work of Jacques Cassini and Edmond Halley, and their work on the fixed stars was certainly not typical of their times. Astronomers of the early eighteenth century were concerned with the solar system almost exclusively; the 'fixed' stars were still very much fixed and hence uninteresting. In the Principia, Newton was concerned

only that the stars were sufficiently far away not to interfere with his description of the solar system: he referred to stellar parallax only once. As Corollary ii to Proposition xiv of Book iii of the 1726 edition he wrote

"... since these stars are liable to no sensible parallax from the annual motion of the Earth, they can have no force, because of their immense distance, to produce any sensible effect in our system". (6)

Thus it was not important to Newton that stellar distances be numerically evaluated; rather he needed the stars to be at an indeterminable distance.

By the final third of the eighteenth century there were signs of a new interest in the realm of the fixed stars, emerging specifically from the work of John Michell and William Herschel. The concern of both these men about stellar distances derived from their ideas and thoughts on the sidereal heavens and, as mentioned previously (see section 3.3), the fact that they were primarily concerned with the stars rather than with the solar system differentiates them from all their contemporaries. This also distinguishes them from Halley and Jacques Cassini who each paid relatively little attention to the fixed stars in comparison with their other astronomical enquiries. Michell and Herschel wished to be able to measure stellar distances because of what that could tell them about their main interest: the nature of the universe beyond the solar system.

It was for similar reasons that Bessel wanted to measure the distance from the Earth of the double star 61 Cygni, although he had the added incentive of wishing to try to extend Newtonian dynamics beyond the immediate neighbourhood of the Sun. With increased interest in double stars and in the proper motions of stars, natural curiosity about their distance gave way to a specific need to know them; until such distances could be

ascertained, it was impossible to build any meaningful model of the stellar heavens. The model based on uniformity was definitely unacceptable by the early nineteenth century, and astronomers were left without an alternative. Moreover, by this time certain astronomers were specifically interested in the hitherto unexplored universe beyond the solar system, and fundamental to any synthesised picture was a knowledge of distance.

Hence it is clear that astronomers' reasons for measuring parallax and for calculating stellar distances became more complex as time passed. The same is true of their perception of the problems involved and of what constituted adequate solutions. For Hooke the major problems were practical: how to eliminate refraction considerations and, once that problem was solved, how to set up his telescope in order to observe the star he selected. He perceived no problems once he possessed the data. Contrastingly, after the discovery of aberration it became obvious that the apparent annual motions of the stars were more complicated than had previously been assumed. Bradley's discovery also introduced a new concept of the best time of year at which to observe the chosen star. As Clairaut pointed out, since aberration and parallax are 90° out of phase, in any given coordinate the effect of one will be maximum when that of the other is minimum, so that parallax should be sought at the times of its maximum manifestation only. During the eighteenth century the difficulties of allowing for the apparent motions of the stars increased rather than decreased: as a result of Bradley's further discovery of nutation and the establishment of various values of proper motions. Arguments developed over whether precession was a constant effect, and also over how to allow most effectively for refraction. We have seen that by the 1810s, despite the claims of Pond and

Brinkley as to the precise nature of positional astronomy, there was no consensus over the values of astronomical constants or over the best means of reducing observational data. Of associated importance to the problems of reduction was astronomers' increasing awareness of how small the effect of parallax must be. In 1717 Jacques Cassini felt justified in claiming that Sirius was a suitable star to study because precession affects its position by only $2\frac{1}{2}''$ every year, against which parallax should be easily identifiable. By the early nineteenth century no astronomer could have made such a claim: $2\frac{1}{2}''$ would have marked the uppermost limit for parallax in the minds of virtually every astronomer.

As the problems of reduction increased, so did the difficulties of convincing fellow astronomers that any observations truly indicated the existence of parallax. All through the seventeenth and eighteenth centuries, in order to look for parallactic effects astronomers were generally satisfied with selecting a particular star and then studying its position to see if that had changed at specified times of year. [The one exception to this was Jacques Cassini who seems to have observed Sirius as often as possible, and who described an oscillation of the transit point about a cross-wire, suggesting he was interested in a continual annual movement]. By the early years of the nineteenth century, however, particularly with Bessel's work, the need for evidence from the whole cycle was recognised. Lindenau and Olbers posed the mathematical problems of how to describe the ways in which the position of a star varied as a result of parallax throughout the year, and Bessel met their demands. Associated with the problem of continued variation was that of the need for as much usable data as possible. Again it was Bessel who was mainly responsible for demonstrating the need for a

sufficiently large number of data to produce reliable results. With Bessel's mathematical description of parallax, and his application of the method of least squares for reducing vast quantities of data, astronomers could use observations from any time of year as evidence of a parallactic motion. After his writings on such subjects - principally the Fundamenta Astronomiae and the Tabulae Regiomontanae - any claims to have measured parallax which were not supported by a large number of observations, taken at all times of year, were unlikely to be greeted with much enthusiasm by astronomers.

The other theoretical problem associated with the detection of parallax which underwent a number of changes between Hooke's work and Bessel's was that of selection criteria. We have seen how these criteria changed, from high transit altitude and brightness in the late seventeenth and early eighteenth centuries, to close pairs of stars of different brightness in the late eighteenth then finally to specific types of binary and, most importantly, stars of high proper motion in the nineteenth century. What we must explain now is why they changed. The simple first answer is that as astronomers devoted more time to a study of the sidereal heavens they became aware of the immense variety among the stars and hence of the number of possible ways of judging nearness. After Michell's arguments, Herschel's observations of the mutual motions of double stars, and the establishment of the values of a number of proper motions, astronomers had real doubts about the reliability of taking brightness as an indication of nearness. Gradually high proper motion began to take precedence: it was certainly Bessel's main reason for his continued study of 61 Cygni. High transit altitude was dropped as a necessary criterion for two reasons. Firstly atmospheric refraction was studied in detail both empirically and theoretically and it became easier for astronomers

to allow for it than it was for Hooke, Wren or even James Bradley; secondly, and more significantly, as astronomers paid more attention to close double stars they realised, particularly after William Herschel's 1781 paper, that if relative parallax were sought refraction effects would cancel out of the calculations. As explained in Chapter Six, the study of double stars was of great importance to the development of stellar astronomy in general and to the particular problem of annual parallax, principally because it increased astronomers' awareness of the variety of objects outside the solar system and it caused them to think of these objects as possible candidates for parallactic investigations. It was because, by the late 1820s, certain astronomers were interested in the general properties and behaviour of double stars that they thought of using them to solve the long-standing problem of parallax; it was not the case that doubles were studied because of their implications in that problem.

Finally, we must consider changes among the audience for whom particular individuals were writing, as this illustrates more clearly than anything else how much the problem of the detection of parallax, and the solution to that problem, changed over the 170 years between Hooke and Bessel. We have Hooke's word that he was addressing the 'learned'; he presented his observations to the Royal Society in the first instance, but then chose to make his 'experimentum crucis' the subject of one of his Cutlerian lectures. He clearly felt that any sufficiently well educated man was capable of understanding his argument, as indeed he would have been. Hooke's experiment involved no conceptual problems and no very complicated mathematics; the mathematics it did contain was a geometrical explanation of parallax which the 'learned' should certainly have been able to follow. Furthermore, an

understanding of the geometry of the problem was not crucial to Hooke's argument. The case was very different for Bessel. A mere glance at his account of his detailed study of 61 Cygni reveals that it must have been written with a specific readership in mind. Although the underlying concept of parallax was no more complicated than it had been for Hooke's audience, the problems surrounding the concept were very technical and could only be understood by individuals with a high standard of mathematical training. The paper was published in a technical astronomical journal which would automatically limit the type of reader. Furthermore, when Bessel wrote of his results to scientists outside Germany he was careful in his choice of whom to inform, because he feared he might be misunderstood. Part of his fear might with justice have derived from the language problem, but the difficulty of the mathematics involved must neither be overlooked nor underestimated. Bessel wrote for an elite even among astronomers.

The fact that Bessel directed his paper on the parallax of 61 Cygni towards a particular small group of astronomers, whereas Hooke had been interested in convincing a much larger and vaguer set of people whom he called 'learned' is of course one facet of increased specialisation in astronomy. But that alone is not enough to explain the enormous differences between the undertakings of Hooke and Bessel in connection with the question of annual stellar parallax. We have seen how so many aspects of the quest for parallax changed with time; we must now attempt an explanation of that change. To do this it will be necessary to examine the wider questions of the ways in which astronomy changed, and finally to assess the significance of the search for parallax within the context of the changing study of astronomy.

As has been stressed several times already, the

fixed stars were not regarded as objects worthy of particular enquiry when Hooke, Wren, James Gregory, Wallis and J. D. Cassini were investigating the possibility of detecting parallax. Astronomers were then interested in the movements and appearance of bodies within the solar system, and that system provided them with a wealth of fascinating material to consider. The 1670s brought the foundation of the Royal Observatory at Greenwich, and the Astronomer Royal was committed to gathering observations which could be used to solve the longitude problem for navigators; in other words to the compilation of accurate tables showing the movements of solar system objects, particularly the Moon. During the same decade Ole Roemer suggested a finite speed for light, reaching his conclusion from a consideration of observations and tables of the movement of satellites of Jupiter. In Paris, Cassini's main preoccupation was with his major geographic and geodetic surveys; such work did involve observations of one star, the Pole Star, but as a means of establishing the constant of precession and the values of terrestrial latitude, not because the Pole Star might be interesting in itself.

After the publication of the Principia in 1687 astronomers had an even greater incentive for investigating the motions of solar system objects, to check how closely their positions as predicted by the theory of gravitation matched the observations. Throughout the eighteenth century the situation, certainly at the main European observatories, hardly changed. Systematic observations of the daily positions of celestial bodies were gathered and catalogued. Obviously stellar positions were recorded as well as the coordinates of bodies within the solar system, but it was the latter which were of first importance. The former were in fact collected only to simplify the process of identifying changes in the latter. Perhaps the most useful

application of this form of positional astronomy was in the tracking of comets; indeed the period 1600 to 1800 has justly been described as "the age of the comet" (7). There was certainly very little interest in the fixed stars (other than in the use of their positions as markers) at all during the eighteenth century. 'Nebulous' stars were occasionally noted as curiosities; but the most comprehensive list of these curious objects - prior to the pioneering work by William Herschel - that produced in 1771 by the French astronomer Charles Messier, was compiled so that they would not in future be confused with comets, which formed the nucleus of Messier's interests. It is also true that two individuals, John Goodricke and Nathaniel Pigott, devoted some of their time to the study of variations in the brightnesses of stars, and speculated over possible causes of the observed variations. But their work, though interesting, could hardly be described as a major component of eighteenth century astronomy.

Perhaps the most significant 'stellar' study to be undertaken during the eighteenth century was the accumulation of values of proper motion; this was certainly the most important work related to, but not dealing directly with, the question of annual parallax. But again this study was not carried out widely, nor was it undertaken specifically because astronomers were interested in the stars. In fact, before the important work of Tobias Mayer we only have evidence of the compilation of a few values by Halley, Le Monnier and Jacques Cassini and nothing to suggest what conclusions, if any, they drew from them. Later in the century we know that Michell, Herschel and Prévost perceived that proper motions could be a reflection of the real motion of the solar system among the stars, but again, the work of these astronomers on this subject was atypical of eighteenth century astronomy and discussed little by

their contemporaries.

It is therefore fair to say that during the eighteenth century study of the sidereal heavens, where it existed, was sporadic and disconnected. Astronomers were still very much concerned with the solar system: their books and articles discussed at length such things as the appearance of the planets, the presence of Sun spots, phases of Mercury, Venus and the Moon, and their observations of comets. Moreover, although the name of William Herschel is connected, on excellent grounds, with the ultimate establishment of the study of stellar astronomy, his original fame as we very well know was for the discovery of Uranus. The immediate impact of his discovery was that astronomers devoted themselves to an even greater extent to the solar system: the new planet had to be investigated thoroughly and its orbit calculated precisely.

However, the discovery of Uranus did have one significant ramification in the development of a new kind of astronomy which came to fruition in the nineteenth century in the German states. In the early 1770s J. E. Bode and J. D. Titius (a professor at Wittenberg) formulated independently the so-called 'Bode-Titius' law of planetary distances (8). After the discovery of Uranus, the distance of which complied with the law to a certain extent, astronomers began to think again of the 'missing' planet between the orbits of Mars and Jupiter. As a result, certain German astronomers - von Zach, Bode, Olbers, Schröter and Gauss among others - decided to organise a combined effort to find the missing planet. It was the organisation that was important in the long-term: a sense of cooperation came to prevail among astronomers in Germany and although the first asteroid, Ceres, was discovered by Piazzi, the subsequent study of Ceres and the discoveries of the three next asteroids all

resulted from the combined efforts of this self-formed group of astronomers. Even more important was the establishment of lines of contact between different astronomers and from one astronomical centre to another. During the early years of the nineteenth century these men kept in touch by frequent letters and freely exchanged observations and ideas on how best to reduce them; of particular significance was Gauss's work on the orbits of comets and asteroids.

It was into this circle of astronomers that Bessel was introduced; he, Schumacher and, for a few years, Lindenau joined the circle and maintained mutual contact, thus increasing the size of the group. They were all very much involved in the problems of precision in positional astronomy and were all convinced that between them they could make the subject more mathematically rigorous and precise than it had ever been. In 1821, Olbers could address Bessel with the laudatory declaration

"You, dear Bessel, and our Gauss are bringing about an explicit revolution, and marking a veritable epoch in observational astronomy", (9)

referring to his colleagues' application of mathematics to observational astronomy, especially the formulation of celestial orbits. But before any astronomer could successfully compute an orbit, he had to be sure that his data were properly reduced to allow for all errors; as has been shown at great length in Chapter Five, the most important contributions to the theory of errors in positional astronomy were made during the 1810s and 1820s by Bessel. By 1830, positional astronomy was an exceedingly complex and highly mathematical discipline, the full details of which could be grasped by only a few. It had attained this state as a result of the deliberate policy on the part of certain astronomers, particularly those in Germany, to 'perfect' astronomy.

This newly perfected positional astronomy dominated the work at major observatories during the middle decades

of the nineteenth century, and it was applicable equally to observations of the solar system and to those of the stars. By the 1830s, as we have seen, many astronomers were genuinely as interested in the stars as they were in the solar system, and their studies of the stars were mainly directed towards attempts to establish their positions very precisely: this meant determining their proper motions accurately and making detailed studies of the orbits of double and multiple systems. Such projects were carried out zealously at all main observatories. The type of sidereal astronomy which is associated with the name of William Herschel still did not form any part of the programme of research at national observatories well into the nineteenth century. It is clear, nevertheless, that a particular variety of stellar astronomy, which depended on astronomers' being interested in the stars as individuals not merely as background markers, did prevail. It is within the context of the development of this type of astronomy that the history of attempts to measure parallax must be viewed.

In fact, if we now reconsider this history, two significant points can be made. Firstly we can see how the changing attitudes of astronomers to the problem of parallax illustrate the way in which particular studies in stellar astronomy emerged; secondly, and more importantly, it is clear that successful identification of annual stellar parallax was achieved as a result of the maturation of those same studies. On the first point we need only to remind ourselves that before 1800 astronomers turned to the problem of parallax only occasionally, and each time their endeavours failed they merely occupied their time with other astronomical projects, almost always related to the solar system. On the other hand during the early years of the nineteenth century, despite continuing failure, we find astronomers returning to the problem of parallax detection as though they refused to admit

ultimate defeat. Their interest lasted, and was paralleled by concern in other aspects of the motions of the stars. They paid greater attention than ever before to the problems surrounding parallax - those explained in Chapter Four - and persisted until they were solved.

The second point is also quite clear if we remember the conditions listed in Chapter Six (see sections 6.2 and 6.3) as prevailing in astronomy in the mid-1830s and relevant to the problem of parallax. At that time particular astronomers - Struve and, especially, Bessel - were interested in the study of double stars, they possessed excellent instruments, and were eager to devote sufficient time to the analysis of carefully chosen stars. These conditions resulted from the general wish of astronomers to perfect their subject by applying the best available mathematical techniques to it. The final outcome for the history of attempts to measure parallax was Bessel's paper on the parallax of 61 Cygni. Both Hooke and Bessel had set out to answer the same question: what is the annual parallax of a particular star? What was so very different was the astronomical context within which each asked the question. The ways in which that context changed with time, as outlined in this thesis, have provided a means for understanding the whole history of attempts to measure annual stellar parallax.

List of abbreviations

Gauss - Bessel Correspondence: C. F. Gauss, Werke:
Briefwechsel mit F. W. Bessel;
edited by R. Englemann
(Leipzig, 1880)

Huygens, Oeuvres: Oeuvres Complètes de Christian Huygens
(The Hague, 1888 - 1950)

J. H. A.: Journal for the History of Astronomy

Mon Corr: Monatliche Correspondenz

Olbers - Bessel Correspondence: Briefwechsel zwischen Olbers
und Bessel;
edited by A. Erman
(Leipzig, 1852)

Oldenburg: The correspondence of Henry Oldenburg;
edited by A. R. and M. B. Hall
(Wisconsin, London, 1965 -)

Phil Trans: Philosophical Transactions of the Royal Society
of London

Notes to Chapter One: Introduction

1. S. Mitton (ed.) The Cambridge Encyclopaedia of Astronomy (London, 1977) 23.
2. For a discussion of modern problems related to the measurement of parallax see D. E. Blackwell, "The stars as suns", in G. T. Bath (ed.) The state of the Universe (Oxford, 1980) 63 - 92, 88 - 90.
3. See Rigaud MS 37 f. 147 at the Bodleian Library Oxford; I am grateful to Simon Schaffer for drawing my attention to this manuscript.
4. The details of Rigaud's dating of the lecture may be found in ibid., f. 158.
5. G. R. Fockens, "Exponantur observandi et computandi rationes, quibus astronomi stellarum fixarum parallaxin annuam definire conati sunt", Annales of the Academia Lugduno Batavia (Leyden, 1834).
6. F. G. W. Struve, Études d'astronomie stellaire (St. Petersburg, 1846) 94 - 108.
7. C. A. F. Peters, "Recherches sur la parallaxe des étoiles fixes", Mémoires de l'Académie impériale des Sciences de St. Petersburg, 6 série, v (St. Petersburg, 1853) 1 - 180.
8. See R. Grant, A history of physical astronomy (London, 1852) 547 - 51; A. Clerke, History of astronomy during the nineteenth century (London, 1885) 18 - 9, 38 - 45; A. Berry, A short history of astronomy (London, 1898) 161, 258 - 60, 341, 360 - 2; P. Doig, A concise history of astronomy (London, 1950) 147 - 8, 185; A. Pannekoek, A history of astronomy (London, 1961) 316, 335 - 339 - 44.
9. M. A. Hoskin, "Sidereal astronomy in adolescence", Avant, avec après Copernic (Paris, 1975) 315 - 42.
10. J. D. Fernie, "The historical search for stellar parallax", Journal of the Royal Astronomical Society of Canada, lxix (1975) 153 - 61, 222 - 39.
11. M. A. Hoskin, "Stellar distances: Galileo's method and its subsequent history", Indian journal of History of Science, i (1966) 22 - 9.
12. O. Struve, "The first stellar parallax determination", H. M. Evans (ed.) Men and moments in the history of science (Seattle, 1959) 177 - 206; N. S. Hetherington, "The first measurements of stellar parallax", Annals of Science, xxviii (1972) 319 - 25.
13. J. Bradley, "An account of a new discovered motion of the fix'd stars", Phil Trans., xxxv (1728/9) 637 - 61.
14. Hoskin, "Sidereal astronomy" (ref. 9) 321.

Notes to Chapter Two: Early Ideas 1660 - 1720

1. N. Copernicus, De Revolutionibus Orbium Coelestium (Nuremburg, 1543).
2. Ibid., 5; from the translation by E. Rosen in Nicolas Copernicus complete works (London, Basingstoke, 1978) 14.
3. T. Brahe, De mundi aetherei recentioribus phaenomenis (Uraniborg, 1588) 187; reprinted in J. L. E. Dreyer (ed.) Tychonis Brahe Dani Opera Omnia, iv (Copenhagen, 1929) 156; from the translation by M. Boas and A. R. Hall in "Tycho Brahe's system of the world", Occasional notes of the Royal Astronomical Society, iii (1959) 258.
4. In fact Tycho's estimates of apparent stellar diameters were less than the estimates given by any of his contemporaries. However, an apparent diameter of 1' combined with an absence of parallax meant that the actual diameters of the stars must be at least as large as the orbit of Saturn, which Tycho could not accept. See J. L. E. Dreyer, Tycho Brahe. A picture of scientific life and work in the sixteenth century (reprint edition, New York, 1963) 191.
5. T. Brahe to J. Kepler, 9 December 1599; printed in M. Caspar (ed.) Johannes Kepler Gesammelte Werke, xiv (Munich, 1949) 89 - 98, 95.
6. T. Digges, Aleu seu Scalae Mathematicae (London, 1573).
7. J. Dee, Parallatice commentationis praxeosque nucleus quidam (London, 1573). For a discussion of this work and that of Digges, see F. R. Johnson, Astronomical thought in Renaissance England (Baltimore, 1937) 155 - 60.
8. G. Galilei, Dialogo di Massimi Sistemi del Mondo Tolemaico, e Copernicano (Florence, 1632); S. Drake translation: Dialogue concerning the two Chief World Systems (Second edition, Los Angeles, 1967) 358 - 62.
9. Ibid., 387 - 9.
10. For a brief account of the subsequent use of Galileo's ideas see M. A. Hoskin, "Stellar distances: Galileo's method and its subsequent history", Indian Journal of History of Science, i (1966) 22 - 9.
11. Galilei, Dialogue (ref. 8) 382 - 3.
12. In J. C. Houzeau Vade - Mecum (Brussels, 1882) 881, there is a list of values allegedly assigned to stellar parallax by several astronomers working in the period c. 1600 - 50. In fact a number of these values were calculated by Houzeau from estimates of the distances of the fixed stars given by the astronomers concerned (Magini, Scheiner, Kepler, Longomontanus, Bouillaud, among others). There is no evidence that any of them tried to measure the phenomenon in the way Tycho had.
13. Perhaps the most important, and certainly the most often-cited in subsequent accounts of parallax, was Giambattista Riccioli, who failed in his attempt to measure the parallax of Sirius. See his Almagestum Novum astronomium veterem novamque complectens, ii (Bologna, 1653) 452.

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14. R. Hooke, An attempt to prove the movement of the Earth from observations (London, 1674); reprinted in R. T. Gunther, Early science in Oxford, viii (Oxford, 1931) 1 - 28.
15. See R. Grant, A history of physical astronomy (London, 1852) 548; J. D. Frenie, "A historical search for stellar parallax", part 2, The Journal of the Royal Astronomical Society of Canada, lxi (1975) 222 - 39, 222; M. A. Hoskin, "Sidereal astronomy in adolescence", Avant, avec. après Copernic (Paris, 1975) 315 - 42, 318.
16. J. A. Bennett, "Christopher Wren; astronomy, architecture and the mathematical sciences", J. H. A., vi (1975) 149 - 84, 166.
17. S. Wren, Parentalia; or memoirs of the family of the Wrens (London, 1750) 198. "To find whether the Earth moves" is listed under the heading "A catalogue of New Theories, Inventions, Experiments, and Mechanick Improvements, exhibited by Mr. Wren ..."
18. Sir R. Moray to Christian Huygens, 29 November 1663; printed in Huygens, Oeuvres, iv, 443 - 5, 444 - 5: "... Monsieur Niele ... me dit il y a 10 Ans que Monsieur Wren et luy ont parlé de cette affaire ... ils se proposoyent de placer un verre objectif sur quelque grande hauteur et de le fixer là pour observer et comparer les altitudes meridionales de quelque estoile afin de tascher de decouvrir sil y a quelque paralaxe &."
19. For a detailed analysis of Hooke's collaboration with Wren in scientific work see J. A. Bennett, Studies in the life and work of Sir Christopher Wren (Ph. D. Thesis, Cambridge, 1974).
20. J. Ward, Lives of the Professors of Gresham College (London, 1740) 104.
21. Ibid.
22. J. Gregory to H. Oldenburg, 8 June 1675; printed in T. Birch, The History of the Royal Society, iii (London, 1757) 225 - 6, and subsequently in Oldenburg, xi, 336 - 9. The first half of the letter, concerning parallax, was also published in S. J. Rigaud, Correspondence of scientific men of the seventeenth century, ii (Oxford 1841) 262 - 3, and subsequently in H. W. Turnbull (ed.) James Gregory tercentenary memorial volume (London, 1939) 306 - 7. The mathematical contents of the letter were sent on by Oldenburg to Huygens; see Huygens, Oeuvres, vii, 475 - 6 and Oldenburg, xi, 375 - 6.
23. Gregory tried to obtain stellar distances photometrically as well as by direct measurement. See his Geometriae pars universalis (Padua, 1668) 147.
24. Turnbull, Gregory (ref. 22) 275.
25. Hooke, Attempt (ref. 14) 7 - 11.
26. See J. A. Bennett, "Robert Hooke as mechanic and natural philosopher", Notes and records of the Royal Society of London, xxxv (1980) 33 - 43.
27. Hooke, Attempt (ref. 14) 26. By parallax Hooke meant the angle subtended by the diameter of the Earth's orbit, rather than that subtended by the radius, which is the definition currently used.

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28. See the following letters: Oldenburg to Huygens, 30 March 1674; printed in Huygens, Oeuvres, vii, 380, and in Oldenburg, x, 547 - 8; Oldenburg to J. D. Cassini, 30 March 1674; printed in Oldenburg, x, 548 - 50; Oldenburg to J. Hevelius, 30 March 1674; ibid., 550 - 1. Hooke's lecture was also mentioned in the following letters: Wallis to Oldenburg, 7 April 1674; ibid., 564 - 6; Oldenburg to Bouillaud, 5 October 1674; ibid., xi, 96 - 7.
29. Huygens to Oldenburg, 5 May 1674; ibid., 1 - 4, 3 (translation by A. R. Hall and M. B. Hall). (Original French also in Huygens, Oeuvres, vii, 382 - 3.)
30. J. D. Cassini to Oldenburg, 18 June 1674; ibid., 33 - 5, 34 (translation by Hall and Hall).
31. Ibid.
32. Ibid., 34 - 5.
33. D. W. Howse, Greenwich Observatory: Its buildings and instruments (London, 1975) 59 - 60.
34. W. Ball to Huygens, 10 March 1674; printed in Huygens, Oeuvres, vii, 428 - 9.
35. J. Picard, Voyage d'Uranibourg (Paris, 1680) 17 - 22; reprinted in A. Auzout, Lettres du M. Auzout sur les grandes lunettes (Paris, 1736).
36. J. D. Cassini, "S'il est arrivé du changement dans la hauteur du Pole, ou dans le cours du Soleil?", Mémoires de l'Académie royale des Sciences depuis 1690 jusqu'à 1699, x (Paris, 1730) 360 - 75; from a letter written to the Académie on 31 July 1693.
37. Ibid., 372: " ... il n'en restera que très-peu que l'on puisse attribuer à un changement réel arrivé dans la situation du Ciel à l'égard de la terre, ou dans le mouvement des Astres; et il est bien plus raisonnable d'attribuer cette différence à ces causes accidentelles ... "
38. J. Wallis "On the parallax of the fixed stars in reference to the Earth's annual orbit", Phil Trans, xvii (1693) 844 - 9.
39. Ibid., 847.
40. Ibid., 846. For circumpolar stars, Wallis was referring to the points in their diurnal rotation 90° (and therefore 6 hours) away from their points of superior and inferior culmination.
41. By the 1690s John Wallis was in his 70s, and unlikely to spend his evenings at a telescope! However, we know from his letter to Oldenburg in April 1674 (see ref. 28) that he had first thought of his idea 26 years earlier. But there is no evidence to suggest that he ever put his thoughts into practice.
42. Wallis, "On parallax" (ref. 38) 848.
43. F. Roberts, "Concerning the distance of the fixed stars", Phil Trans, xviii (1694) 101 - 2.
44. For a bibliographical note on Francis Roberts see Oldenburg, xi, 117: "Francis Robartes (c. 1650 - 1718) a younger son of John Robartes, Earl of Radnor, whose chief interest was in music and its mathematics."

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45. J. Flamsteed to J. Wallis, 20 December 1698. Flamsteed wrote in English, but this letter appears not to have survived. Wallis translated the letter into Latin and published it in his Opera Mathematica, iii (Oxford, 1699) 701 - 8. This letter is discussed in the context of contemporary reaction to it in Appendix 1.
46. I. Newton to J. Flamsteed, 26 January 1695; printed in F. Baily, An account of the Rev'd John Flamsteed (London, 1835) 148 - 50; also printed in J. F. Scott (ed.) The Correspondence of Isaac Newton, iv (Cambridge, 1967) 73 - 5. Some controversy arose over whether Flamsteed should mention Newton's theory in print, and resulted in Wallis's omitting any mention of Newton in the published version of Flamsteed's claim. See Baily, 166 - 9; also E. G. Forbes, Greenwich Observatory: its origins and early history (London, 1975) 70 - 2.
47. J. Cassini, "Réflexions sur une lettre de M. Flamsteed à M. Wallis touchant la parallaxe annuelle de l'étoile polaire", Mémoires de l'Académie royale des Sciences pour 1699 (Paris, 1702) 177 - 83.
48. Flamsteed to Wren, 19 November 1702; printed in Wren, Parentalia (ref. 17) 247 - 53.
49. Ibid., 252.
50. J. Flamsteed, Historia Coelestis Britannica (London, 1725).
51. Many of the letters which were written in the course of this dispute are published in Baily, Account (ref. 46) 280 - 335.
52. J. Cassini, "Réflexions" (ref. 47) 183.
53. As well as Jacques Cassini, the Danish astronomer Ole Roemer said that he suspected Flamsteed's theory must be wrong, in an unpublished letter which Roemer wrote to Flamsteed in 1701. The letter was later printed in P. N. Horrebow, Operum mathematico-physicorum tomus tertius (Copenhagen, 1741) 80 - 1. For a discussion of Roemer's contribution to the development of the problems of parallax see below, section 2.4.
54. This is reported in J. Pound, "A rectification of the motions of the five satellites of Saturn", Phil Trans, xxx (1718) 768 - 74, 768 - 9.
55. Ward, Lives (ref. 20) 105.
56. Horrebow's very moving account of the fire and his own desperate attempts to save as much as he could may be found in his Basis Astronomiae (Copenhagen, 1735), part of which is translated into English in Grant, History (ref. 15) 466 - 7.
57. The extract was eventually published by Horrebow in ibid., 62: "Terra motu, seu parallaxis orbis annui ex observationibus Sirii et Lyrae, annis 1692 - 93".
58. Like so many of Roemer's innovative ideas this one - whereby the most significant instrumental errors were the clock errors, which Roemer knew he could estimate more accurately than any others - was never taken up by his contemporaries or immediate successors. For this reason, although his work is interesting, Roemer's significance in the history of astronomy is perhaps not very great. For Bessel's paper on parallax in right ascension see below (Chapter Five).

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59. With the analysis that Roemer made he could only hope to show that parallax existed; he could not obtain an absolute measurement of the phenomenon for either star. All he could obtain was the sum of the parallaxes in right ascension for the two. If he were to assume that they were each at similar distances from the Earth, then he could calculate that distance, but his answer would not be anything more than an estimate.
60. Horrebow, Basis (ref. 56) 68.
61. That Roemer was always anxious about the accuracy of his instruments is clear, and he invented a number of new devices for improving telescopic mounts and new methods for dividing and reading scales. See H. C. King, The history of the telescope (London, 1955) passim.; and A. Chapman, Dividing the circle (D. Phil. Thesis, Oxford, 1978) 135 - 41. Roemer was sufficiently interested in the accuracy of his observations to derive a theory of instrumental errors for his meridian circle, although the evidence for this was not published until 1910 when a selection of miscellaneous Roemer manuscripts, mostly dating from the final years of his life, were published to commemorate the 200th anniversary of his death. See T. Eibe and K. Mayer (eds.) Ole Roemer's Adversaria (Copenhagen, 1910) 76 - 9; see also A. V. Nielsen, "Ole Roemer and his meridian circle", Vistas in astronomy, x (1968) 105 - 12; and N. K. Jorgensen and K. M. Moesgaard, "Peder Horrebow's formulation of the so-called Mayer formula", Centaurus, xviii (1972) 276 - 9.
62. K. M. Pedersen, "Roemer, Flamsteed and the search for a stellar parallax", Vistas in astronomy, xx (1976) 165 - 9, 166.
63. K. M. Pedersen, "Vie et oeuvre de Roemer", in R. Taton (ed.) Roemer et la vitesse de la lumière (Vrin, 1978) 113 - 28.
64. Fahrenheit to Boerhaave, 1729; printed in W. E. Knowles Middleton, A history of the thermometer (Baltimore, 1966) 71.
65. Roemer to Flamsteed (ref. 53); Professor E. G. Forbes kindly allowed me to see his translation of this letter.
66. O. Roemer, "Démonstration touchant le mouvement de la lumière" Journal des Savants (December, 1676) 233; English translation in Phil Trans, xii (1677) 893 - 4.
67. Roemer, Adversaria (ref. 61) 50; see also G. Van Biesbroeck and A. Tiberghien, "Études sur les notes astronomiques contenues dans les Adversaria d'Ole Roemer", Oversigt over det Kongelige Danske Videnskabernes selskabs Forhandlinger (Copenhagen, 1913) (21) - (25), 213 - 324, 312 - 3.
68. J. Cassini, "De la grandeur des étoiles fixes et de leur distance à la terre", Mémoires de l'Académie royale des Sciences pour 1717 (Paris, 1719) 256 - 68.
69. Such an assumption was made by Newton when he attempted to explain why there are as many stars of each magnitude as are observed. See A. R. Hall and M. B. Hall, The unpublished scientific papers of Isaac Newton (Cambridge, 1962) 376; see also M. A. Hoskin, "Newton, Providence and the fixed stars", J. H. A., viii (1977) 77 - 101.
70. The solstitial colure is the great circle passing through the two solstitial points (the points reached by the Sun on the eclip-

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- tic at mid-summer and mid-winter) and perpendicular to the ecliptic at these points. It is the only great circle along which a star's celestial latitude and declination are related linearly. (See W. M. Smart, A text-book in spherical astronomy, 6th. edition (Cambridge, 1977) ch. 1.) The advantage of this lies in the calculations for finding the times of year at which the effect of parallax in a given coordinate is maximum or minimum. The simplest variation with time occurs in celestial latitude, but the coordinate generally measured was declination (or altitude at transit), so selecting stars for which the two coordinates are linearly related simplifies the calculations appreciably.
71. When giving this figure Cassini quotes 2" 25'" (ref. 68, 263), obviously a theoretical calculation rather than an observation!
 72. See M. E. W. Williams, "Flamsteed's alleged measurement of annual parallax for the Pole Star", J. H. A., x (1979) 102 - 16, (Appendix 1) 112 - 3.
 73. Jacques Cassini's later contribution is discussed in the two nineteenth century surveys of attempts to measure parallax: G. R. Fockens, "Exponantur observandi et computandi rationes, quibus astronomi stellarum fixarum parallaxin annuam definire conati sunt", Annals of the Academia Lugduno Batavia (Leyden, 1834); C. A. F. Peters, "Recherches sur la parallaxe des étoiles fixes", Mémoires de l'Académie impériale des Sciences de St. Petersburg, 6 série, v (St. Petersburg, 1853) 1 - 180. It is, however, hardly mentioned in more recent secondary sources.
 74. E. Halley, "Some remarks on a late essay of Mr. Cassini, wherein he proposes to find, by observation, the Parallax and Magnitude of Sirius", Phil Trans, xxxi (1720/1) 1 - 5.
 75. Ibid., 3.
 76. See for example Grant, History (ref. 15) 336 - 9; Hoskin, "Sideral astronomy" (ref. 15) 318; A. Pannekoek, A history of astronomy (London, 1961) 289 - 90.
 77. J. Bradley, "An account of a new discovered motion of the fix'd stars", Phil Trans, xxxv (1728/9) 637 - 61, 645. The paper was reprinted in S. P. Rigaud (ed.) Miscellaneous works and correspondence of the Rev'd James Bradley (Oxford, 1832) 1 - 16.
 78. Ibid., 638.
 79. See Flamsteed's letter to Wallis (ref. 45) 703, compared with ibid., 661.
 80. Wallis to Oldenburg (ref. 28). Hooke disputed Wallis's claim to have thought through the problems in a similar way to Hooke's own as long before as the 1650s. He pointed out that Wallis had said nothing of this until the publication of Hooke's Attempt. See R. W. Robinson and W. Adams (eds.) The diary of Robert Hooke (1672 - 80) (London, 1935) 97: entry for 16 April 1674.
 81. Such a claim was made by David Gregory (the nephew of James Gregory) in his Astronomiae physicae et geometriae elementa (Oxford, 1702) 276.
 82. I. Newton, Philosophiae naturalis Principia mathematica (London, 1687).

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83. See Bailly, Account (ref. 46) 147 - 60. Also, on Newton's lunar theory see R. S. Westfall, Never at rest: A biography of Isaac Newton (Cambridge, 1980) 432 - 60, 538 - 48, 583 - 6, 655 - 63, 736 - 50.

Notes to Chapter Three: James Bradley and the eighteenth century 'gap'

1. J. Bradley, "An account of a new discovered motion of the fixed stars", Phil Trans, xxxv (1728/9) 637 - 61; reprinted in S. P. Rigaud (ed.) Miscellaneous works and correspondence of the Rev'd James Bradley (London, 1832) 1 - 16.
2. Ibid., 661.
3. M. A. Hoskin, "Sidereal astronomy in adolescence", Avant, avec, après Copernic (Paris, 1975) 315 - 42, 318 - 9.
4. J. D. Fernie, "The historical search for stellar parallax", Journal of the Royal Astronomical Society of Canada, lxi (1975) 153 - 61 and 222 - 39. It should be noted that neither Fernie nor Hoskin denies that there were attempts to measure parallax in this period: William Herschel's work is far too well known for that. But Hoskin appears to regard Herschel's 1782 paper very much as an exception, and as Herschel did not try to measure absolute parallax his paper was exempted from Fernie's generalisation.
5. G. R. Fockens "Exponantur observandi et computandi rationes, quibus astronomi stellarum fixarum parallaxin annuam definire conati sunt", Annales of the Academia Lugduno Batavia for the year 1834 (Leyden, 1834).
6. C. A. F. Peters, "Recherches sur la parallaxe des étoiles fixes", Mémoires de l'Académie impériale des Sciences de St. Petersbourg, 6 série, v (St. Petersburg, 1853) 1 - 180.
7. E. Manfredi, De annuis inerrantium stellarum aberrationibus (Bologna, 1729); and "De novissimis circa fixorum siderum errores observationibus", De Bononiensi Scientiarum et Artium instituto atque academiae commentarii (Bologna, 1731) 277 - 84.
8. R. Long, Astronomy in Five Books (published in two volumes) vol. i (Cambridge, 1742) chapters 17 and 18.
9. J. Lalande, Astronomie, iii (Paris, 1764); 3rd. edition (Paris, 1792) 75 - 86.
10. N. Maskelyne "A proposal for discovering the annual parallax of Sirius", Phil Trans, li (1760) 889 - 95.
11. W. Herschel "On the parallax of the fixed stars", ibid., lxxii (1782) 82 - 111; reprinted in J. L. E. Dreyer (ed.) The scientific papers of Sir William Herschel, i (London, 1912) 39 - 57.
12. A. C. Clairaut, "Sur la manière la plus simple d'examiner si les étoiles fixes ont une parallaxe, et de la déterminer exactement", Histoire de l'Académie royale des Sciences pour 1739 (Paris, 1741) 358 - 69.
13. J. Michell, "An enquiry into the probable parallax, and magnitude of the fixed stars ...", Phil Trans, lvi (1767) 234 - 64.
14. P. Horrebow, Basis astronomiae (Copenhagen, 1735).
15. P. Horrebow, Copernicus triumphans (Copenhagen, 1727).

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16. See S. P. Rigaud, "Memoirs of Bradley", Bradley works (ref. 1) i - cviii, xxxviii.
 17. E. Manfredi, Ephemerides motuum coelestium (Bologna, 1715).
 18. Before the publication of Manfredi's book the best available theories of parallax were in the two papers by Jacques Cassini written in 1699 and 1717 (See Chapter Two) ; these were far from adequate, and the De ... Aberrationibus was a vast improvement. Manfredi discussed how parallax was manifested in various coordinate systems and gave copious diagrams to illustrate the text.
 19. It is not even clear from Manfredi's work that he was a confirmed Copernican: his initial investigation of the apparent motions of the stars seems to have been made in an attempt to convince himself one way or the other about the Copernican system. A close analysis of Manfredi's publications might reveal more certainly than this his views on heliocentricity, but that would be too large an undertaking to include in this history of attempts to measure annual stellar parallax.
 20. Subsequent analysis by C. A. F. Peters showed that the effect Roemer had detected was the result of a systematic clock error; see Peters (ref. 6) section 10.
 21. These letters were first printed in P. Horrebow, Operum mathematico-physicorum, iii (Copenhagen, 1741) and subsequently in Fockens (ref. 5) 186 - 8.
 22. It was of course Roemer who had first postulated a finite speed for light in his "Demonstration touchant le mouvement de la lumière", Journal des savants (Paris, 7 December 1676) 233 - 6; English translation in Phil Trans, xii (1677) 893 - 4.
 23. J. Delisle to P. Horrebow, 20 July 1728; printed in Horrebow, Basis (ref. 14) 76 - 80.
 24. Ibid. 77 - 8: "Dez les commencements de l'Académie des Sciences de Paris, Mrs. Picard, de la Hire & Cassini se sont à la verité appliquez à observer les differences de passage des etoiles fixes & des Planetes entre elles avec les horologes à pendule fort exact; Mr. Flamsted a fait la meme chose en Angleterre. Mais comme ces astronomes n'avoient en vüe dans leur observations, que la theorie des Planetes, & le catalogue des etoiles fixes sans penser à determiner la parallaxe de l'orbe annuel, il n'a guere d'esperer, malgré le grand nombre de leurs observations, d'en trouver beaucoup de faites dans leur circonstances, que demande la methode de Mr. Roemer ... "
- There is further evidence that Delisle had investigated previous attempts to measure parallax; among the archives attributed to Delisle at the Observatoire de Paris is a folder entitled "Parallaxe annuelle des étoiles fixes suivant Mms Flamsteed, Cassini, Roemer, Horrebow et De l'Isle", Observatoire archives, box A. 2. 6. It contains hand written copies of papers written by the astronomers named.

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25. Clairaut, "Sur la manière" (ref. 12) 359: "Quant à la parallaxe, il semble qu'on pourroit conclurre d'après les observations de M. Bradley, qu'il n'y en a point de sensible, puisque cet Astronome n'a pas trouvé 1" quand il a eu fait le retranchement nécessaire pour l'Aberration; cependant comme les étoiles qu'il y a observées, sont peu en nombre, puisque l'instrument dont il s'est servi, n'avoit qu'un limbe de 12 degrés, il me semble qu'on n'est pas en droit de conclurre qu'aucune étoile n'a de parallaxe, comme on peut affirmer que toutes les Etoiles sont sujettes à l'aberration de la lumière."
26. The first two books were published as volume i in 1742; book 3 appeared on its own in 1764 and was republished together with books 4 and 5 as volume ii in 1785.
27. Long, vol. i (ref. 8) 297.
28. D. Gregory, Astronomiae physicae et geometricae elementa (Oxford, 1702) book iii, proposition 54.
29. Long, vol. i (ref. 8) 321 - 2. The other two reasons offered as explanations by Long were a) that one or both stars actually changed their positions in space, which he thought very unlikely or b) one star had one side brighter than the other and as it revolved it showed alternately its bright and dark sides.
30. Ibid., 322.
31. Ibid.
32. See for example Maupertuis (ref. 83), and the vast number of publications by Cassinis II, III and IV for accounts of the large scale surveys undertaken by astronomers at the Observatoire.
33. See E. G. Forbes, Greenwich observatory: its origins and early history (London, 1975) chapter 3.
34. N. L. de Lacaille, Fundamenta Astronomiae (Paris, 1758).
35. Maskelyne. "A proposal" (ref. 10) 891.
36. Peters, "Recherches" (ref. 6) 26: "Il est donc à présumer, que la parallaxe indiquée par Maskelyne doit être attribuée en grande partie, à l'imperfection des observations de Sirius, d'ailleurs peu nombreuses."
37. Forbes, Greenwich observatory (ref. 33) 120; see also H. Woolf, The transits of Venus (Princeton, 1959) 132.
38. Maskelyne, "A proposal" (ref. 10) 889.
39. Ibid., 890.
40. Lalande, Astronomie (ref. 9) 75: "Il est démontré actuellement que la parallaxe est insensible et comme nulle dans les étoiles fixes."
41. Ibid., 82 - 3: " ... mais ces observations de Sirius ne vont que l'été 1752 à l'hiver suivant; et il peut y avoir eu quelque cause locale qui ait produit dans ces observations des différences de 8".
42. Peters, "Recherches" (ref. 6) 27.

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43. This alternative method of measuring stellar distances is discussed by M. A. Hoskin, in "The English background to the cosmology of Wright and Herschel", in W. Yourgrau and A. D. Breck (eds.) Cosmology, History, and Theology (New York, 1977) 219 - 31. J. D. Fernie also gives an account of the main attempts, but his article is misleading because he includes photometric attempts to measure distance among attempts to measure parallax. In fact parallax was regarded by the astronomers concerned as an alternative way of obtaining distances; indeed, as with the work of Robert Hooke, parallax was not even always sought with the wish to measure distance as the main incentive.
44. Michell, "An enquiry" (ref. 13) 244.
45. Ibid., 249. See also Appendix 2.
46. See M. A. Hoskin, William Herschel and the construction of the heavens (London, 1963) 32 - 5; and Hoskin, "Sidereal astronomy" (ref. 3) 327.
47. Michell, "An enquiry" (ref. 13) 254.
48. For an account of eighteenth century ideas concerning the possible configuration of the local stellar system see S. L. Jaki, The Milky Way (Newton Abbott, 1973) chapters 5, 6, and 7, in which the theories of Thomas Wright, J. H. Lambert and Emmanuel Kant are discussed. Although the works of these men are obviously of great significance to the development of cosmology, they were all outside mainstream positional astronomy.
49. The most important earlier papers in which the stars were considered as interesting objects in their own right were written by by Halley during the 1710s and 1720s: "Of the infinity of the sphere of the fixed stars", Phil Trans, xxxi (1720/1) 22 - 4; "Of the number, order and light of the fix'd stars", ibid., 24 - 6; "An account of several nebulae or lucid spots like clouds", ibid., xxix (1715/6) 390 - 2. However, Halley's work in astronomy as a whole could not be described as primarily concerned with the distribution and structure of the stars.
50. See M. A. Hoskin, "Goodricke, Pigott and variable stars", J. H. A., x (1979) 23 - 41.
51. For a recent account of Herschel's deliberate attempt to start and develop a new type of astronomy see S. Schaffer "Uranus and the establishment of Herschel's astronomy", ibid., xii (1981) 11 - 26.
52. See Hoskin, Construction (ref. 46).
53. See Forbes, Greenwich observatory (ref. 33) chapter 4: The rise of positional astronomy.
54. E. Halley, "Considerations on the change of the latitude of some of the principal fixed stars", Phil Trans, xxx (1717/9) 736 - 8.
55. In 1737 Jacques Cassini wrote a paper on the possible proper motion in celestial longitude of two stars: "Du mouvement apparent des étoiles fixes en longitude", Histoire de l'Académie royale des Sciences. Année 1737 (Paris, 1740); the year following the publication of this paper, Pierre Le Monnier discussed proper

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- motions in the introduction to his Histoire céleste (Paris, 1741).
56. For an account of Mayer's work on proper motion see E. G. Forbes, Tobias Mayer's 'Opera inedita' (London, 1971) 108 - 12; see also E. G. Forbes, "Tobias Mayer's contributions to observational astronomy", J. H. A., xi (1980) 28 - 49.
 57. It was of course during his survey of the skies in search of double stars that Herschel made the discovery of Uranus - the discovery which established his fame and led to royal patronage. See Schaffer, "Herschel and Uranus" (ref. 51) 12.
 58. From Herschel's journal books (at the archives of the Royal Astronomical Society) we can see that during these years Herschel kept in mind quite assiduously the idea of finding suitable candidates for parallax, commenting frequently on large apparent diameters indicating nearness. See R. A. S. Herschel Archive: Herschel W., 2.1.1 - 2.1.3.
 59. Herschel, "On parallax" (ref. 11) 41.
 60. Ibid., 44 - 5.
 61. W. Herschel, "Catalogue of double stars", Phil Trans, lxxii (1782) 112 - 63.
 62. W. Herschel, "Catalogue of double stars", ibid., lxxv (1785) 40 - 649.
 63. These letters are printed in C. A. Lubbock, The Herschel Chronicle (Cambridge, 1933) 98 - 102. William Watson was the son of Sir William Watson, the well known naturalist and physician to the King. The son was already a Fellow of the Royal Society when he and Herschel met in the late 1770s (probably through the Bath Philosophical Society) and was sometimes responsible for transmitting Herschel's work to the Society while Herschel remained in Bath.
 64. Lubbock, Chronicle (ref. 63) 101.
 65. Ibid., 109 - 10; the letter is only partly reproduced here.
 66. For a discussion of why Herschel may have done this see Hoskin, Construction (ref. 45) 38 - 40.
 67. W. Herschel, "On the proper motion of the Sun and solar system", Phil Trans, lxxiii (1783) 247 - 83; reprinted in Hoskin, Construction, 43 - 54.
 68. Hoskin, Construction, 53.
 69. See G. Piazzi, "Results of calculations of the observations made at various places of the eclipse of the Sun which happened June 3, 1788", Phil Trans., lxxix (1789) 55 - 65.
 70. For a description of the instrument see G. Piazzi, Della specolo astronomica di Palermo (Palermo, 1792), and for a discussion of instrumental developments during the eighteenth century see below, section 4.2.
 71. See G. Piazzi, Resultati delle osservazioni della nuova stella scoperta, il 1^o gennaio (Palermo, 1801).

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72. G. Piazzi, "Ricerche di Guiseppe Piazzi su la parallasse annua di alcune delle principali fisse", Memorie della Societa Italiana, xii (Modena, 1805) 40 - 62.
73. Ibid., 41: "Ma nel 1802 avendo insieme raccolte tutte le declinazioni della Lira, osservate nello spazio di dieci anni, ed in tempi diversi degli anni medisimi, vi notai delle differenze, le quali, comunque picciole, non lasciarono di farmi pensare che forse potessero essere cagionate della parallasse."
74. See G. Piazzi, Della specola astronomica di Palermo, 2nd. edition (Palermo, 1806) 8.
75. In fact, Bradley's work on aberration was done before he succeeded Halley as Astronomer Royal in 1742. While Bradley was working at Wansted, Halley was busy re-equipping the Royal Observatory, and once this was done Halley's main concern during the 1730s was the solution of the longitude problem.
76. Letter from James Bradley to the Earl of Macclesfield: "Concerning an apparent motion observed in some of the fixed stars", Phil Trans, xlv (1748) 1 - 43.
77. See Bradley, Works (ref. 1) 404 - 12.
78. See Appendix 1.
79. Several of Herschel's telescopes are described in J. A. Bennett, "On the power of penetrating into space: the telescopes of William Herschel", J. H. A., vii (1976) 75 - 108.
80. A. Chapman, Dividing the circle (D. Phil Thesis, Oxford, 1978) chapter 11, 349 - 61.
81. W. Herschel, "Account of the changes that have happened during the last 25 years, in the relative situation of double stars", Phil Trans, xciii (1803) 339 - 82.
82. However, as early as 16 February 1783, Nathaniel Pigott, an accomplished amateur astronomer living in Yorkshire, wrote to his friend John Goodricke: "... how far the discovery of proper motions in small stars may induce astronomers to chose them for finding a parallax I dare not venture to decide." Quoted from Hoskin, "Goodricke, Pigott" (ref. 50) 24.
83. P. L. M. de Maupertuis, Oeuvres, i - iv (Berlin, Lyons, 1758 - 68).

Notes to Chapter Four: Problems of precision in the early nineteenth century

1. This instrument is described by Roemer's pupil Peter Horrebow in his Basis Astronomiae (Copenhagen, 1735) 141; it has been studied recently by A. V. Nielsen, "Ole Roemer and his meridian circle", Vistas in astronomy, x (1968) 105 - 12.
2. A. Chapman, Dividing the circle (D. Phil. Thesis, Oxford, 1978) chapter 8: The precision graduation of full circles.
3. M. F. du Chaulnes, Nouvelle méthode pour diviser (Paris, 1768).
4. See W. Roy, "An account of the trigonometrical operations whereby the distances between the meridians of the Royal Observatories of Greenwich and Paris has been detected", Phil Trans, lxxx (1790) 111 - 270. In his paper Roy summed up the frustration he had experienced in his attempts to obtain the theodolite from Ramsden by saying, "on the completion of that first part of the business, it was little expected that nearly three full years would have elapsed before an instrument could be obtained from Mr. Ramsden for taking the angles".
5. Chapman (ref. 2) 259; the term altazimuth is used to describe this type of instrument because it measures positions in altitude (with the observer's horizon defining 0° and the zenith $+90^{\circ}$) and azimuth (with the point due east on the observer's horizon at 0° and due north at $+90^{\circ}$).
6. G. Piazzzi, Della specola astronomica di Palermo (Palermo, 1792).
7. S. Vince, A treatise on practical astronomy (Cambridge, 1790) 178 - 83.
8. W. Pearson, An introduction to practical astronomy (London, 1824, 1829). The drawing of Piazzzi's vertical circle is bound at the end of volume i, plate xviii, and the account is in volume ii, 413 - 28.
9. As pointed out by Derek Howse in his Greenwich Observatory: the buildings and instruments (London, 1975) 53, the greatest advantage of being able to make observations out of the meridian lay in their relevance to the lunar theory. When the Moon is new it transits around noon and because of its proximity to the Sun it is invisible; by being able to observe the Moon at other instants astronomers could accumulate data for the complete lunar cycle, which had not previously been possible.
10. Pearson (ref. 8) ii, 417.
11. The advantage of being able to evaluate instrumental errors was of course recognised by astronomers in the nineteenth century who wrote about the history of their subject; see for example R. A. Proctor and A. C. Ranyard, Old and new astronomy (London, 1892) 51. The point they fail to make is that the idea of allowing for instrumental errors in the reduction of data rather than trying to correct the instrument itself was new at the end of the eighteenth century.
12. Vince (ref. 7) 183.
13. Pearson (ref. 8) ii, 418.

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14. For example see M. A. Hoskin, "Sidereal astronomy in adolescence", Avant, avec, après Copernic (Paris, 1975) 315 - 42, 319.
15. Pearson (ref. 8) ii, 417.
16. In fact Ramsden died before the circle was finally completed. Nearly 20 years passed between the time when the instrument was ordered and its eventual delivery to Dublin. It was finished by Ramsden's successor Berge. For an account of the trouble Brinkley faced while trying to obtain the circle see S. M. P. McKenna, "Astronomy in Ireland from 1790", Vistas in astronomy, ix (1968) 283 - 5.
17. The best available description of the instrument is that written by Brinkley himself, "An account of observations made at the observatory of Trinity College, Dublin, with an astronomical circle, 8 feet in diameter, which appear to point out an annual parallax in certain fixed stars", Transactions of the Royal Irish Academy, xii (1815) 33 - 75.
18. J. Brinkley, "Extract from a letter ... to the Rev. Nevil Maskelyne ... on the annual parallax of α Lyrae", Phil Trans, c (1810) 204.
19. Pearson (ref. 8) ii, 425.
20. In 1809 Troughton was awarded the Royal Society Copley Medal for his monograph on instrument making, "On the graduation of astronomical circles", Phil Trans, xcix (1809) 105 - 45.
21. See J. Pond, "Description of an astronomical circle", ibid., xcvi (1806) 420 - 54.
22. Chapman (ref. 2) 263.
23. The reason for the movement of the first point of Aries (Υ) is that the axis of the Earth precesses and since Υ is defined relative to the Earth. if the axis moves, so will Υ .
24. J. le F. Lalande, Astronomie, i (Paris, 1764; 3rd. edition, Paris, 1792) 314: "En comparant 200 longitudes prises dans le catalogue britannique de Flamsteed pour 1690, avec celles du catalogue de Lacaille, je trouve... 50" $\frac{1}{2}$ par année."
25. R. Grant, A history of physical astronomy (London, 1852) 319.
26. J. Brinkley, "On the parallax of certain fixed stars", Phil Trans, cviii (1818) 275 - 302, 285.
27. J. Pond, "On the parallax of the fixed stars", ibid., cvii (1817) 158 - 75.
28. The reason for this is that, because the effect of precession is cumulative, the constant was known fairly accurately; the variation produced is always in the same direction, so that over a period of 50 years the difference in longitude of a star would be very noticeable. Also, the observations considered at either end of such a period need not be in themselves highly accurate in order to give a reasonably accurate value for the precessional constant.

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29. J. Bradley, "An account of a new discovered motion of the fixed stars", Phil Trans, xxxv (1728/9) 637 - 61; reprinted in S. P. Rigaud (ed.) Miscellaneous works and correspondence of the Rev'd James Bradley (Oxford. 1832) 1 - 16.
30. J. Bradley, "A letter to the Earl of Macclesfield concerning an apparent motion observed in some of the fixed stars", ibid, xlv (1748) 1 - 43; reprinted in Rigaud, 17 - 41.
31. Rigaud (ref. 29) 29,
32. F. von Zach, Tabulae speciales aberrationis et nutationis ... (Gotha, 1806 - 7).
33. F. von Zach, Nouvelles tables d'aberration et de nutation (Marseille, 1812).
34. Brinkley, 1818 (ref. 26) 299.
35. J. B. Delambre, Astronomie théorique et pratique, iii (Paris, 1814) 107.
36. Brinkley, 1818 (ref. 26) 282.
37. Ibid., 300.
38. See Delambre (ref. 35) chapter 31 and P. S. M. de Laplace, Traité de Mécanique Céleste, ii (Paris, 1799) livre v, chapitre 1.
39. Bradley, 1748 (ref. 30), Rigaud edition, 26.
40. P. S. M. de Laplace, "Sur l'application du calcul des probabilités à la philosophie naturelle", Connaissance des temps pour 1818 (Paris, 1815) 361 - 77.
41. Some of Bradley's work on refraction is reproduced in S. P. Rigaud, Supplement to Dr. Bradley's miscellaneous works (Oxford, 1833) 3 - 16. The supplement is bound together with the original book (ref. 29) in the 1972 Johnson Reprint edition.
42. The theoretical work on refraction is in P. S. M. de Laplace, Traité de Mécanique Céleste, iv (Paris, 1805) livre x, chapitres 1, 2, 258 - 315; Delambre's original tables, based on observation alone, were modified by Laplace then published in 1806 by the French Board of Longitude.
43. Both formulae are quoted from Pearson (ref. 8) i, 297, 299.
44. Brinkley, 1818 (ref. 26) 279 - 82.
45. See G. Piazzi, Praecipuarum stellarum inerrantium positiones mediae ... (Panormi, 1814).
46. J. Brinkley, "Analytical investigations respecting astronomical refractions ...", Transactions of the Royal Irish Academy, xii (1815) 77 - 103.
47. L. Tilling, The interpretation of observational errors in the eighteenth and early nineteenth centuries (Ph. D. Thesis, London, 1973) chapter 6, 243 - 88.

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48. This definition is from J. Topping, Errors of observation and their treatment (London, 1955) 86.
49. J. D. Fernie, "The historical search for stellar parallax", part 2 Journal of the Royal Astronomical Society of Canada, lxix (1975) 226.
50. Brinkley, "An account of observations" (ref. 17); read 9 May 1814.
51. Ibid., 35.
52. From the time of Robert Hooke's work in the 1660s and throughout the eighteenth century, the angle of parallax of a star meant the angle subtended at the star by the diameter of the Earth's annual orbit. However during the early nineteenth century astronomers began to speak of distances in terms of the astronomical unit, or radius of the orbit. As a result parallax was eventually defined as the angle subtended by the radius. Brinkley was working during the transition and his use in his calculations of 'p' to designate the single parallax but of '2p' when quoting the results reflects this.
53. Brinkley, "An account of observations" (ref. 17) 53.
54. Ibid., 71.
55. J. Brinkley, "Appendix to the account of observations made at the observatory of Trinity College, Dublin, which appear to point out an annual parallax in certain fixed stars", Transactions of the Royal Irish Academy, xii (1815) 119 - 24.
56. Both papers were entitled "On the parallax of the fixed stars" and both appeared in Phil Trans, cvii (1817) 158 - 75 and 356 - 63. The earlier paper was read to the Royal Society on 20 February 1817 and the second on 26 June 1817.
57. For a description of this instrument see Howse, Greenwich Observatory (ref. 9) 26 - 9.
58. Pond did not explain why this was so but according to Howse the instrument "... did not prove steady enough to be used for transits"; see ibid., 28 - 9.
59. Pond, February 1817 (ref. 56) 165.
60. From W. M. Smart, A text-book in spherical astronomy 6th. edition (Cambridge, 1977) 219, the effect of parallax in celestial latitude is given by:

$$\Delta\beta = -\pi \sin\beta \cos(\odot - \lambda)$$

where (λ, β) are the ecliptic coordinates of the star

π its parallax

$\odot$ the longitude of the Sun.

The maximum numerical value of $\Delta\beta$ thus occurs when $\cos(\odot - \lambda) = 1$

$$\text{i.e. } |\Delta\beta|_{\max} \propto \sin\beta$$

and the ratio of the maximum values for α Aquilae and Capella is given by

$$\frac{\pi_{Aq} \sin\beta_{Aq}}{\pi_c \sin\beta_c} \simeq \frac{\pi_{Aq} \times .49}{\pi_c \times .38}$$

For Pond's claim to be maintained he needed to assume $\pi_{Aq} = \pi_c$. If similar calculations are carried out for the effect of parallax in other coordinates the value 5:4 is not obtained.

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61. Howse, Greenwich Observatory (ref. 9) 117.
62. This is so because the solstitial colure is the only celestial great circle which is a circle both of longitude and of right ascension. The rule Pond was trying to apply to variation in declination for stars on opposite sides of the north celestial pole is true for variation in celestial latitude when considering stars on either side of the north ecliptic pole. The fact that only certain stars on the solstitial colure obey the rule in declination is due to the separation of the poles by $23\frac{1}{2}^{\circ}$; two stars lying on different sides of the north celestial pole, but on the same side of the north ecliptic pole could be selected. Pond was further restricted by his wish to choose stars with similar declinations (such that they would transit at similar altitudes, so that the telescope could remain fixed). By adding this criterion he eliminated all stars as candidates.
63. The ecliptic latitude of α Cygni is approximately 325° and that of β Aurigae approximately 90° . From these values the time of year at which maximum and minimum variation in declination as a result of parallax ^{occurs} can be calculated. (See Appendix 1). For α Cygni it turns out that the maximum and minimum are reached when the Sun is in longitudes 120° and 300° ; for β Aurigae they occur when the Sun is in longitude 90° and 270° . So in effect α Cygni is moving around its parallactic ellipse just one month later than β Aurigae moves around its own. By subtracting their declinations, therefore, Pond was subtracting two similar quantities.
64. J. Brinkley, 1818 (ref. 26); read 5 March 1818.
65. Ibid., 277.
66. Ibid., 287.
67. Ibid., 277.
68. J. Pond, "On the parallax of α Aquilae", Phil Trans, cviii, (1818) 477 - 80.
69. Ibid., 477.
70. Ibid., 478.
71. Ibid., 479.
72. J. Pond, "On the parallax of the fixed stars in right ascension", ibid., 481 - 5.
73. J. B. Delambre, "Histoire de l'astronomie pour 1804 et 1805", Connaissance des temps pour 1808 (Paris, 1806) 432 - 5.
74. Pond, "Parallax in right ascension" (ref. 72) 483.
75. His later contributions were: "On the changes which have taken place in the declination of some of the fixed stars", Phil Trans, cxiii (1823) 34 - 8, read April 1822, followed by an Appendix, ibid., 39 - 52, read November 1822; "On the parallax of α Lyrae", ibid., 53 - 72, read November 1822; "On the annual variations of some of the principal fixed stars", ibid., cxv (1825) 510 - 3, read June 1825.
76. F. W. Bessel, Fundamenta Astronomiae ... (Königsberg, 1818); discussed in detail below, section 5.3.

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77. J. Brinkley, "Results of further observations made at the Observatory of Trinity College, Dublin, for determining the obliquity of the ecliptic, and the maximum of the aberration of light", Phil Trans, cix (1819) 241 - 8, 248.
78. J. Brinkley, "An account of observations made with the eight foot astronomical circle, at the Observatory of Trinity College, Dublin, since the beginning of the year 1818, for investigating the effects of parallax and aberration on the places of certain fixed stars", Phil Trans, cxi (1821) 327 - 60.
79. In 1827 Brinkley retired from Dunsink to take up his duties as Bishop of Cloyne. He was succeeded as Astronomer Royal for Ireland by William Rowan Hamilton.
80. Brinkley wrote two further papers on parallax: "On the north polar distances of the principal fixed stars", Phil Trans, cxiv (1824) 50 - 84, read December 1823; "Remarks on the parallax of α Lyrae" ibid., 471 - 98, read March 1824.
81. R. Grant (ref. 25) 550.
82. S. M. P. McKenna, "John Brinkley", Dictionary of Scientific Biography, ii (New York, 1970) 468.
83. In July 1817 the President of the Royal Society, Joseph Banks, wrote to A. B. Granville that the controversy had been settled in favour of Pond; see P. B. Granville (ed.) Autobiography of A. B. Granville M. D., F. R. S. ..., ii (London, 1874) 109 - 10. (I am grateful to my supervisor Dr. M. B. Hall for bringing this letter to my attention.) Subsequently in the mid-nineteenth century both C. A. F. Peters and R. Grant claimed that Pond was shown to be 'right' when parallax was eventually found to be far smaller than the effect Brinkley had detected. This is clearly a very unfair assessment, since Pond was not even certain of the geometry of the problem; it is possible that had parallax been detectable Pond would have failed to recognise it for what it was.
84. See M. A. Hoskin, William Herschel and the Construction of the Heavens (London, 1963) and S. Schaffer, "Uranus and the establishment of Herschel's astronomy", J. H. A., xii (1981) 11 - 26.

Notes to Chapter Five: The early role of F. W. Bessel

1. See for example W. Fricke, "Friedrich Wilhelm Bessel", Dictionary of Scientific Biography, ii (New York, 1970) 97 - 102.
2. Letter from F. W. Bessel to H. M. W. Olbers, 9 December 1804; printed in Olbers-Bessel Correspondence, i, 3 - 5.
3. F. W. Bessel, "Berechnung der Harriot'schen und Torporley'schen Beobachtungen des Cometen von 1607", Mon Corr, x (1804) 425 - 40; also in R. Englemann (ed.) Abhandlungen von Friedrich Wilhelm Bessel (Leipzig, 1875, 6) i, 1 - 6.
4. Bessel to Olbers, 30 October 1806, Olbers-Bessel Correspondence, i, 45 - 8.
5. J. H. Schröter, "Beobachtungen der Ceres und Pallas und über die Parallaxe der Fixsterne", J. E. Bode (ed.) Astronomisches Jahrbuch für 1805 (Berlin, 1802) 197 - 202.
6. Bessel to Olbers, 30 October 1806 (ref. 4) : "Ich habe die Messungen, die Herr. J. Schröter über einige Doppelsterne gemacht hat, untersucht, aber über die Parallaxe nichts daraus schliessen können ... Allein die Beobachtungsfehler machen dieses Resultat sehr unsicher, und ich glaube, dass es nicht viel Gewicht hat."
7. In fact, contrary to general belief, Bessel's interest in the problems of the detection of parallax began very early in his professional career in astronomy. The available accounts of Bessel's early work on parallax are at best sketchy. General histories of astronomy do not mention it at all; the Canadian astronomer J. D. Fernie in his survey of attempted parallax measurements refers only to Bessel's chapter on parallax in the latter's 1818 publication the Fundamenta Astronomiae; C. A. F. Peters in his survey, and historians M. A. Hoskin and N. S. Hetherington all place Bessel's earliest work on the subject in 1815, when Bessel gave a negative value for the parallax of 61 Cygni. Even in the most detailed account of the first accepted measurements of parallax, that by F. G. W. Struve's descendent Otto Struve, the author, after remarking that "Bessel entered the field about 1812", goes on to make a brief reference to the work done in 1815 and 1816 and leaves his account of the early work at that. However, Bessel was clearly interested in parallax far earlier than any of these accounts suggests as will become apparent. For the accounts of Bessel's early work see: J. D. Fernie, "The historical search for stellar parallax" part 2, Journal of the Royal Astronomical Society of Canada, lxi (1975) 222 - 35, 230 - 5; C. A. F. Peters, "Recherches sur la parallaxe des étoiles fixes", Mémoires de l'Académie impériale des Sciences de St. Petersbourg, 6 série, v (1853) 1 - 180, 48 - 51; M. A. Hoskin, "Sidereal astronomy in adolescence", Avant, avec, après Copernic (Paris, 1975) 315 - 42, 321; N. S. Hetherington, "The first measurement of annual stellar parallax", Annals of Science, xxviii (1972) 319 - 25, 322; O. Struve, "The first stellar parallax determination", in H. M. Evans (ed.) Men and moments in the history of science (Seattle, 1959) 177 - 206, 196.

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8. G. Piazzi, "Ricerche di Guiseppe Piazzi su la parallasse annua di alcune pricipali fisse", Memorie della Societa Italiano, xii (Modena, 1805) 40 - 62.
9. For example ref. 5 and "Ueber die Parallaxe der Fixsterne von H. Prof. Schubert", J. E. Bode (ed.) Astronomisches Jahrbuch für 1796 (Berlin, 1792) 113.
10. Bessel to Olbers, 12 February 1807, Olbers-Bessel Correspondence, i, 78 - 9.
11. F. W. Bessel, "Ueber die Parallaxen einiger Fixsterne aus Bradley'schen Beobachtungen", Mon Corr, xix (1809) 183 - 9; also in Abhandlungen (ref. 3) ii, 204 - 6.
12. F. W. Bessel, "Formeln für die jährliche Parallaxe in Rectascension", ibid., 245 - 7. This extract from a letter to Lindenau actually appeared within the third part of an article by Lindenau entitled "Resultate der neuesten Untersuchungen über jährliche Parallaxe der Fixsterne", ibid., 234 - 55. Bessel's contribution may also be found in Abhandlungen (ref. 3) ii, 206 - 7.
13. Bessel to Olbers, 29 May 1808, Olbers-Bessel Correspondence, i, 181 - 3.
14. Ibid., 181: "Ich sage Ihnen meinen innigsten Dank für Ihnen gütigen letzten Brief, und für die Mittheilung der so äusserst leichten Methode, die Ascensions-Parallaxe in eine Reihe zu entwickeln. Ihre Formel, die die Länge der Sonne enthält, ist sehr bequem und wird Herrn. von Lindenau's Wünsche gewiss befriedigen."
15. Bessel to Olbers undated, ibid., 189 - 91.
16. Bessel, "Parallaxe in Rectascension" (ref. 12) 247.
17. The exact date of this letter is, as far as I am aware, unknown. When he published the relevant extract, Lindenau merely said that Bessel had communicated the solution for the parallax in right ascension, without saying precisely when. Lindenau wrote (p. 245) "Herr Inspector Bessel theilte uns eine sehr elegante Auflösung für die jährliche Parallaxe in A. R. mit ... "
18. Bessel to Olbers, 29 January 1809, Olbers-Bessel Correspondence, i, 200 - 1.
19. See J. Cassini, "Réflexions sur une lettre de M. Flamsteed à M. Wallis touchant la parallaxe annuelle de l'étoile Polaire", Mémoires de l'Académie royale des Sciences pour 1699 (Paris, 1702) 177 - 83.
20. Bessel to Olbers, 15 December 1808, Olbers-Bessel Correspondence, i, 197 - 8, 197: "Die Untersuchungen über die Parallaxe des Sirius und des α Lyrae ist nun beendigt."
21. Bessel, "Ueber die Parallaxe einiger Fixsterne" (ref. 11) 184: "Ich habe also zu meiner Untersuchungen, so wie es von Ihnen neuerlich vorgeschlagen wurde, die Rectascensionen angewandt und solche Sterne mit einander verglichen, die 12 Stunden von einander abstehen, wodurch man immer die Summe zweier Parallaxen erhält ... "
22. The most important paper known at the time on the measurement of relative parallax was that by William Herschel, "On the parallax

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- of the fixed stars", Phil Trans, lxxii (1782) 82 - 111; discussed above, section 3.4.
23. See W. M. Smart, A text-book in spherical astronomy, 6th. edition (Cambridge, 1977) 219.
 24. As mentioned above (section 4.4) John Pond fell into the trap of believing that he could detect the sum of the two parallaxes in declination of two stars lying 12 hours apart in right ascension. See J. Pond, "On the parallax of the fixed stars", Phil Trans, cvii (1817) 158 - 75; his conceptual error was pointed out by John Brinkley in "On the parallax of certain fixed stars", ibid., cviii (1818) 275 - 302.
 25. It would perhaps be more accurate to say that Bessel 'recontributed' this factor. since it had first been suggested by Ole Roemer in 1692 (see above, section 2.4).
 26. Bessel, "Ueber die Parallaxe einige Fixsterne" (ref. 11) 186: " ... aber [habe ich] nicht die genugste Spur einer Parallaxe daran wahrgenommen."
 27. Bessel to Olbers, 29 January 1809 (ref. 18) 201.
 28. Bessel to Gauss, 9 February 1809, Gauss-Bessel Correspondence, 98 - 101.
 29. Bessel wrote: "Indess beweissen diese Untersuchungen wenigstens, dass Calandrelli und Piazzzi die Parallaxen viel zu gross angaben.", ibid., 100.
 30. Fricke, "Bessel" (ref. 1). The new observatory was built at the order of Friedrich Wilhelm III of Prussia, and Bessel was recommended for the post of director by Humboldt.
 31. Abhandlungen (ref. 3) iii, 490 - 504.
 32. These objects lying between the orbits of Mars and Jupiter, were discovered as follows: Ceres by Piazzzi in 1801; Pallas by the joint work of Olbers and Gauss in 1802; Juno by Harding, Bessel's predecessor at Lilienthal, in 1804; Vesta by Olbers in 1807. See A. Clerke. A history of astronomy during the nineteenth century, 3rd. edition (London, 1893) 89 - 92.
 33. Bessel to Olbers, 26 April 1810, Olbers-Bessel Correspondence, i, 221 - 5, 222: "Herr von Lindenau hat eine Parallaxe von μ Cassiopeiae beobachtet; er besitzt 14 Beobachtungen, die ihm 3-4" geben, aus denen ich aber nur 1".18 ableiten würde. Sie sehen, dass die Zahl der Observationen zu klein ist, um einigermaßen auf dass Resultat rechnen zu können."
 34. Lindenau's results were not published. Bessel presumably heard of them through his correspondence with Lindenau, although he never in fact said how he obtained the observations.
 35. F. W. Bessel, "Über den Doppelstern Nro. 61 Cygni", Mon Corr, xxvi (1812) 148 - 63. The same subject was discussed by Bessel in a letter to Bode, 1 June 1812, part of which was published in J. E. Bode (ed.) Astronomisches Jahrbuch für 1815 (Berlin, 1812) 209 - 20.
 36. In the two historical papers which mention this method, those

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- by Struve (ref. 7, 196) and Fernie (ref. 7, 230 - 1), it is credited solely to F. G. W. Struve for his discussion in his Stellarum duplicium et multiplicium mensurae micrometricae (St. Petersburg, 1837) clxii - clxv. However, from the French edition of Struve's book, Étoiles doubles. Mésures micrométriques ... (St. Petersburg, 1837) 39 we know that Struve was aware of Bessel's 1812 paper as he refers to it.
37. This argument was first presented by the Revd. John Michell, "An enquiry into the probable parallax, and magnitude of the fixed stars ...", Phil Trans, lvi (1767) 234 - 64. See above, section 3.3.
 38. Observational evidence for the existence of binary stars had also been provided by William Herschel, "Account of the changes that have happened during the last 25 years, in the relative situations of double stars", ibid., xciii (1803) 339 - 82.
 39. Bessel, "Über 61 Cygni" (ref. 35) 149 - 50: "Dieses merkwürdige Sternenpaar ... ist der sehr aufmerksamen Betrachtung der Astronomen würdig, indem es uns zu interessanten Folgerungen über das Fixsternen-Gebäude führen kann."
 40. Ibid., 161: "Gelingt es uns, die jährliche Parallaxe dieses Sternenpaars zu beobachten ... so würden wir daraus die Summe ihrer Massen berechnen können."
 41. For a modern derivation of the formula see Smart (ref. 23) 355 - 7.
 42. See A. P. Norton and J. G. Ingliss, Norton's star atlas, 15th. edition (Edinburgh, 1966) notes to maps 13 and 14: "61 Cygni ... The two stars probably form a long period binary system, since they have the same parallax, and the path of B relative to A is slightly concave." The value given for a is 25".1.
 43. F. G. W. Struve, Mensurae micrometricae (ref. 36) clxv.
 44. Bessel to Lindenau, 8 February 1813; published in part in Mon Corr, xxvii (1813) 383 - 6; also in Abhandlungen (ref. 3) ii, 212 - 3.
 45. See Olbers-Bessel Correspondence, i, 338 - 78, and Gauss-Bessel Correspondence, 175 - 92 for the letters which were written.
 46. J. Brinkley, "An account of observations made ... to point out an annual parallax in certain fixed stars", Transactions of the Royal Irish Academy, xii (1815) 33 - 75, read to the Academy on 9 May 1814.
 47. J. B. Delambre, "Observations faites ... à Dublin ... qui paraissent d'indiquer une parallaxe en certaines étoiles ...", Connaissance des temps pour 1819 (Paris, 1816) 383 - 404.
 48. F. W. Bessel, "Ueber den Ort des Polarsterns", J. E. Bode (ed.) Astronomisches Jahrbuch für 1818 (Berlin, 1815) 233 - 41; B. A. von Lindenau, "... Ueber 61 im Schwan", ibid., 244 - 51; F. G. W. Struve, "... und der A. R. von α , δ und β klein Bären", ibid., 271 - 6.
 49. Bessel to Olbers, 14 November 1814, Olbers-Bessel Correspondence, i, 388 - 93.

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50. Olbers to Bessel, 12 January 1815, ibid., 398 - 402. The date of the missing letter from Bessel is given by Olbers as 1 December 1814.
51. Bessel to Olbers. 18 February 1815, ibid., 402 - 9.
52. Ibid., 406: "Indessem wurde es zu voreilig sein, wenn ich schon auf diese Beobachtungen die Behauptung gründen wollte, dass der Sterne wirklich eine merkliche Parallaxe hat ... die Parallaxe der Fixsterne ist ein Punkt, mit dem man, wie die vielen darin gemachten Misgrisse zeigen sehr vorsichtig umgehen muss ... "
53. F. W. Bessel, "Ueber die Parallaxe von 61 Cygni und μ Cassiopeiae", Zeitschrift für Astronomie von von Lindenau, ii (Gotha, Februry 1816) 134 - 8; also in Königsberger Beobachtungen, ii (Königsberg, 1816) v - viii; and Abhandlungen (ref. 3) ii, 213 - 6.
54. Bessel to Olbers, 23 June 1815, Olbers-Bessel Correspondence, i, 431 - 6, 435: "Leider haben wir hier jetzt immer heitere Tage und trübe Nächte, so dass ich meine Beobachtungen von 61 Cygni noch nicht wieder habe anfangen können."
55. Bessel to Olbers, 27 November 1815, ibid., 446 - 8, 448: "No 61 des Schwans scheint auch nach meinen diesjährigen Beobachtungen keine Parallaxe zu haben."
56. Bessel's determination to measure parallax is also illustrated by his decision to try the double star method; having failed in his attempt to measure the absolute parallax of 61 Cygni, he did not give up. Instead he turned to a different method. It is possible that he was guided in this by the hint of success in his analysis of the two neighbouring stars, μ and ϵ Cassiopeiae.
57. F. W. Bessel, Königsberger Beobachtungen, iii (Königsberg, 1817) ix; see also Abhandlungen (ref. 3) ii, 216.
58. Bessel to Olbers, 15 February 1816, Bessel-Olbers Correspondence, ii, 14 - 20, 17: "Die negative Parallaxe, die man wohl hin und wieder finden wird, und die ich bei dem Polarsterne wirklich aus Bradley's Beobachtungen gefunden habe, ist allerdings eine Wirkung von Beobachtungsfehlern."
59. Quoted from Peters, "Recherches" (ref. 7) 50: "Mais il y a une autre préoccupation plus dangereuse encore, dans les observations faites exprès pour la détermination de la parallaxe en ascension droite. L'astronome connaissant les époques du maximum et minimum, est exposé à faire, à ces époques, des estimes défectueuses dans un des deux sens opposés, et à produire ainsi des parallaxes apparentes, sans qu'elles existent dans la réalité, ou de détruire les parallaxes effectives. Ce dernier cas me paraît avoir été celui de Bessel. Voulant éviter le danger de trouver des parallaxes trop fortes, il a involontairement taxé les fractions des passages de la 6le du Cygne trop faibles à l'époque du maximum, et trop fortes à celle du minimum ... "
60. The most important of these are J. B. Delambre, Astronomie theorique et pratique, iii (Paris, 1814); F. von Zach, Nouvelles tables d'aberration et de nutation (Marseilles, 1812); P. S. M. de Laplace, Traité de la mécanique céleste, iv (Paris, 1805); G. Piazzi Praecipuarum stellarum inerrantium positiones mediae... (Panormi, 1814).

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61. F. W. Bessel, Fundamenta Astronomiae pro anno MDCCCV ... (Königsberg, 1818).

62. The section devoted to parallax is ibid., 110 - 33.

63. Ibid., 110. Unfortunately Bessel did not say where he obtained these figures. However, according to Smart (ref. 23) 221, the formula relating parallax in declination to the right ascension and declination of the star and the longitude of the Sun is

$$\Delta\delta = \pi (\sin\odot \sin\varepsilon \cos\delta - \cos\odot \cos\alpha \sin\delta - \sin\odot \sin\varepsilon \sin\alpha \sin\delta) \quad (1)$$

where π = parallax

$\odot$ = longitude of the Sun

ε = obliquity

(α, δ) = equatorial coordinates of the star.

$\Delta\delta$ varies with $\odot$ so to find its maximum value set $\frac{d(\Delta\delta)}{d\odot} = 0$

$$\text{i.e. } \tan\odot_m = \frac{\sin\varepsilon \sin\alpha \sin\delta - \sin\varepsilon \cos\delta}{\sin\delta \cos\alpha}$$

$\odot_m$ being the value of $\odot$ for which $\Delta\delta$ is maximum or minimum. From this, the relevant values of $\odot_m$ for Sirius, Vega and Procyon may be found and substituted into (1), giving the required multiples of π . This, or something very similar, must have been the method used by Bessel as he was dealing with these sorts of formulae all the time.

64. Olbers' formula as quoted by Bessel was

$$d\alpha = \frac{\pi \sin\alpha \sin(L - \varphi)}{\cos\delta \sin\varphi}$$

where (α, δ) = star's coordinates

L = longitude of the Sun

$\cot\varphi = \cos\varepsilon \cot\alpha$ (ε = obliquity)

$$\text{So } d\alpha = \pi \sec\delta \left[\frac{\sin\alpha (\sin L \cos\varphi - \cos L \sin\varphi)}{\sin\varphi} \right]$$

$$= \pi \sec\delta [\sin\alpha (\sin L \cos\varepsilon \cot\alpha - \cos L)]$$

$$= -\pi \sec\delta (\cos L \sin\alpha - \sin L \cos\alpha \cos\varepsilon),$$

which is the point from which Bessel starts in the Fundamenta.

65. In the early 1820s F. G. W. Struve tried to measure the parallax in right ascension for pairs of stars 12 hours apart, but again he was unsuccessful. (See below, section 6.1.) Bessel's formula was not entirely forgotten however. In his Dissertatio of 1834 G. R. Fockens ["Exponantur observandi et computandi rationes, quibus astronomi stellarum fixarum parallaxin annuam definire conati sunt", Annales of the Academia Lugduno Batava (Leyden, 1834) 1 - 257, 22 - 4] discussed in detail the beauty

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of Bessel's formula as a piece of mathematics. Then the English astronomer Robert Main also referred to it in his paper "On the present state of our knowledge of the parallax of the fixed stars", Memoirs of the Royal Astronomical Society, xii (1842) 1 - 60. 24. The point remains however that the formula was never applied successfully to any observations.

66. Bessel, Fundamenta (ref. 61) 112.
67. Bessel unfortunately does not give the details of this method. It appears however to be an adapted form of the method of least squares outlined by Gauss in a letter to Bessel on 21 October 1810, published in Gauss-Bessel Correspondence, 128 - 31. Bessel points out that Gauss had taught him the method in Fundamenta, 116.
68. Bessel, Fundamenta, 117 for the calculations relating to Sirius and Vega, 121 for those on the Pole Star.
69. Clerke, History (ref. 32) 38.
70. Fricke, "Bessel" (ref. 1) 99.
71. Olbers to Bessel, 10 May 1807, Olbers-Bessel Correspondence, 1, 96 - 8.
72. Bessel, Fundamenta (ref. 61) 130 - 2.

Notes to Chapter Six: The successful identification of annual stellar parallax

1. J. Schröter, "Beobachtungen der Ceres und Pallas, und etwas über die Parallaxe der Fixsterne", J. E. Bode (ed.) Astronomisches Jahrbuch für 1805 (Berlin, 1802) 197 - 202.
2. G. Piazzi, "Ricerche di Guiseppe Piazzi su la parallasse annua di alcune della principali fisse", Memorie della Societa Italiano, xii (Modena, 1805) 40 - 62.
3. G. Calendrelli, "Osservazioni e riflessioni sulla parallasse annua dell'alfa della Lira", Opuscoli Astronomici, ii (Rome, 1806).
4. J. B. Delambre, "Parallaxes des fixes", within a paper entitled "Histoire de l'astronomie pour 1804 et 1805", Connaissance des temps pour 1808 (Paris, 1806) 432 - 5.
5. B. A. von Lindenau, "Resultate der neuesten Untersuchungen über jährliche Parallaxe der Fixsterne", Mon Corr, xviii (1808) 401 - 16, and ibid., xix (1809) 38 - 60, 234 - 53.
6. F. W. Bessel, "Formeln für die jährliche Parallaxe in Rectascension", ibid., xix, 245 - 7; also published in R. Engleman (ed.) Abhandlungen von Friedrich Wilhelm Bessel (Leipzig, 1875,6) ii, 206 - 7.
7. F. W. Bessel, "Ueber die Parallaxe einiger Fixsterne aus Bradley'schen Beobachtungen", Mon Corr, xix (1809) 183 - 9; also in Abhandlungen (ref. 6) ii, 204 - 6.
8. See section 5.2 for Lindenau's analysis of the position of the star μ Cassiopeiae; see also B. A. von Lindenau, "...über die Parallaxe des Polarsterns & ...", J. E. Bode (ed.) Astronomisches Jahrbuch für 1819 (Berlin, 1816) 213 - 8, and "... Ueber μ im Schwan", ibid für 1818 (Berlin, 1815) 244 - 51.
9. See the following letters: Bessel to Gauss, 9 February 1809; c. 2 July 1815; 14 February 1816; 13 November 1816; printed in Gauss-Bessel Correspondence, 98 - 101; 214 - 8; 232 - 5; 245 - 6. Bessel to Olbers: 12 February 1807; 29 July 1807; 10 March 1808; 29 May 1808; 15 December 1808; 29 January 1809; 26 April 1810; 7 November 1814; 18 February 1815; 23 June 1815; 27 Nov. 1815; 5 Jan. 1816; 15 February 1816; 21 June 1816; 8 May 1817; printed in Olbers-Bessel Correspondence, i, 78 - 9; 112; 165 - 7; 181 - 3; 197 - 8; 200 - 1; 221 - 5; 388 - 93; 402 - 9; 431 - 6; 446 - 8; ii, 7 - 9; 14 - 20; 28 - 31; 46 - 51. Olbers to Bessel: 21 August 1808; 10 December 1809; 31 May 1809; 19 November 1814; 12 January 1815; 4 March 1815; 15 July 1815; 7 December 1815; 26 January 1816; 26 April 1816; 2 July 1816; 23 March 1817; 23 May 1817; 20 May 1818; 2 October 1818; printed in ibid., i, 185 - 6; 218 - 20; 225 - 7; 393 - 6; 398 - 402; 409 - 11; 439 - 41; ii, 1 - 5; 10 - 4; 20 - 5; 31 - 3; 42 - 6; 51 - 4; 90 - 3; 94 - 6. Olbers to Gauss: 18 February 1804; 29 February 1804; 22 June 1805; 28 March 1813; 28 December 1815; 30 June 1816; c. mid-April 1817; 9 May 1817; 4 February 1818; printed in Gauss-Olbers Correspondence, i, 174 - 6; 180 - 2; 260 - 2; 511 - 3; 614 - 6; 636 - 7; 649 - 51; 654 - 5; 680 - 3. Gauss to Olbers: 12 September 1809; printed in ibid., 439.

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10. F. G. W. Struve, "Beobachtung der Polhöhe der Dorpater Sternwarte und den A. R. von α , δ und β klein Bären", J. E. Bode (ed.) Astronomisches Jahrbuch für 1818 (Berlin, 1818) 271 - 6.
11. For a biographical sketch of F. G. W. Struve see A. J. M. Szanser, "F. G. W. Struve (1793 - 1864), Astronomer", Annals of Science, xxviii (1972) 327 - 46.
12. Evidence for this appears in Olbers-Bessel Correspondence, ii, 98; 101 - 2; 129; 132; 194.
13. F. W. Bessel, "Ueber die Parallaxe von 61 Cygni und μ Cassiopeiae", Zeitschrift für Astronomie vom von Lindenau, ii (Gotha, February 1816) 134 - 8; Königsberger Beobachtungen, ii (Königsberg, 1816) v - viii; ibid., iii (Königsberg, 1817) ix; these accounts are also printed in Bessel, Abhandlungen (ref. 6) ii, 213 - 6; Lindenau "Ueber 61 im Schwan" (ref. 8).
14. See the following letters: Bessel to Olbers, 8 May 1817, Olbers-Bessel Correspondence, ii, 46 - 51. Olbers to Bessel, 23 March 1817; 23 May 1817; 20 May 1818; 2 October 1818; 20 June 1820; 2 December 1823, ibid., 42 - 6; 51 - 4; 90 - 3; 94 - 6; 158 - 62; 252 - 5.
15. Of course his interests were not restricted to the stars. Like all his contemporaries he continued to observe closely the movements of the solar system; for example it was during the early 1820s that he developed "Bessel" functions to solve particular problems in the theory of planetary perturbations. See his "Untersuchungen des Theils der planitarischen Störungen, welche aus der Bewegung der Sonne entsteht", Abhandlungen der Berliner Akademie der Wissenschaften (Mathematische Classe) 1824 (Berlin, 1824) 1 - 31; also in Bessel, Abhandlungen (ref. 6) i, 84 - 102.
16. In fact, in the Connaissance des temps pour 1831 (Paris, 1828) 149 - 51, Delambre published a report he had written in 1822, concerning a number of observations made by le Comte d'Assas Montardier which the latter intended to use to demonstrate the existence of parallax and proper motion. Montardier appears to have tried to use one of Galileo's methods (see section 2.1): observing stars at a distant horizon. He obtained no convincing results, and it is uncertain precisely when the observations were made. Delambre first wrote of them in April 1822 and in the same month Olbers mentioned them (Gauss (Olbers to Gauss, 27 April 1822, Gauss-Olbers Correspondence, ii, 186 - 8) ; the observations were also referred to in art. xxi of the Edinburgh Philosophical Journal; x (1824) 325 - 7, but they seem to have been ignored on the whole by the astronomical community.
17. Piazzzi was succeeded at Palermo by Nicolas Cacciatores, and Calandrelli at Rome was followed by Andrea Conti. See C. André, G. Rayet and A. Angot, L'astronomie pratique et les observatoires en Europe et en Amérique; tome v: observatoires d'Italie (Paris, 1878) 113, 195.
18. F. G. W. Struve, "Observationes ad cognoscendas parallaxes aberrationesque stellarum nonnullarum", Observationes astronomicae Dorpatensis, ii (Dorpat, 1820) 199 - 214; "De numero constanti aberrationis et parallaxi annua fixarum ex observationibus stellarum circumpolarium in ascensione oppositarum", ibid., iii (Dor-

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- pat, 1822) li - lxxxix.
19. When giving his results, Struve took into account the different celestial latitudes of the two stars, and the effect this has on the amount of parallax exhibited by each in right ascension. Thus he did not equate any observed differences merely to the sum of the individual parallaxes: he weighted the two differently. The equation still has an infinite number of solutions, because Struve had no way of estimating the relative distances of the individual stars from the Earth.
 20. "On the astronomical observations made at Dorpat", Edinburgh Journal of Science, i (April - October 1824) 26 - 34.
 21. Ibid., 33.
 22. J. Pond "Remarks on Professor Struve's observations to determine the parallax of the fixed stars", Quarterly journal of Science, Literature and the Arts, xvi (1823) 364 - 5.
 23. Ibid., 365.
 24. Bessel to Olbers, 9 April 1821, Olbers-Bessel Correspondence, ii, 190 - 5.
 25. F. W. Bessel, Tabulae Regiomontanae (Königsberg, 1830).
 26. A. Pannekoek, A history of astronomy (London, 1961) 430.
 27. See "Auszug aus einem Briefe des Hrn. Prof. Struve an den Director der Sternwarte Seeburg", Zeitschrift für Astronomie und verwandte Wiss, iv (1817) 462 - 73. All Struve's publications are usefully listed at the end of Z. K. Novokshanova, Vasily Yakovlevich Struve (Moscow, 1964) 249 - 71.
 28. See ibid., 250 - 7.
 29. Ibid., 251 - 7.
 30. W. Herschel, "Account of the changes that have happened during the last 25 years, in the relative situation of double stars", Phil Trans, xciii (1803) 339 - 82.
 31. See the following letters: Olbers to Gauss, 16 May 1804; 27 July 1804, in Gauss-Olbers Correspondence, i, 189; 197.
 32. This lack of response to Herschel's paper is illustrative of the fact that at the turn of the eighteenth century astronomers were not interested in the realm of the fixed stars, other than as a background against which to study the solar system.
 33. See F. G. W. Struve, "A comparison of observations made on double stars. Letter to J. Herschel", Memoirs of the Royal Astronomical Society, ii (1826) 443 - 55.
 34. F. G. W. Struve, Catalogus novus stellarum duplicium et multiplicium (Dorpat, 1827).
 35. F. G. W. Struve, Stellarum duplicium et multiplicium mensurae micrometricae (St. Petersburg, 1837).
 36. See ref. 18.

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37. F. G. W. Struve, "Observations made on double stars", Edinburgh Philosophical Journal, ix (1823) 334 - 41; and "Observations of double stars", ibid., x (1824) 102 - 9, 331 - 8.
38. Ibid., ix, 334. [Struve's catalogue had not yet appeared in this country and this account was taken from von Zach's Correspondence astronomique].
39. Ibid., x, 102 - 9, 331 - 8.
40. Ibid., 331.
41. Reported in ibid., iii (1820) 191.
42. See J. F. W. Herschel and J. South, "Observations of the apparent distances and positions of 380 double and triple stars ...", Phil Trans, cxiv (1824) Part iii, 1 - 412.
43. Ibid., 3.
44. Giovan Battista Amici was a Mathematics teacher at Modena, and was fascinated by optical instruments and measuring devices. It seems likely that these interests led him to observe double stars, as a means of illustrating the high standard of his micrometers. See "Descrizione di un nuovo micrometro", Memorie di matematica e fisica della Società Italiana delle Scienze, xvii (1816) 344 - 9. His work was known in Britain only through the account of it in von Zach's Correspondence astronomique.
45. J. South, "Observations of the apparent distances and positions of 458 double and triple stars made in the years 1823, 1824 and 1825 ...", Phil Trans, cxvi (1826) 1 - 391, 4 - 5.
46. See under "Doppelsterne" in the indexes of the following volumes of correspondence: Gauss-Olbers; Gauss-Bessel; Gauss-Schumacher; see also the following letters: Bessel to Olbers, 27 October 1824; 28 August 1828, Olbers-Bessel Correspondence, ii, 311 - 2; 316 - 23.
47. South, "Double stars" (ref. 45) 4.
48. J. F. W. Herschel, "On the parallax of the fixed stars", Phil Trans, cxvi (1826) 266 - 80.
49. Ibid., 267.
50. The original mathematics contained an error which Herschel subsequently corrected; see J. F. W. Herschel, "Correction of an error in a paper published in the 'Philosophical Transactions' entitled 'On the parallax of the fixed stars'", Phil Trans, cxvii (1827) 126 - 7.
51. Lord Wrottesley, "On the results of periodical observations of the positions and distances of 19 of the stars in Sir John Herschel's list of stars, favourably situated for the investigation of parallax", Phil Trans, cxli (1851) 333 - 56.
52. See F. Savary, "Sur la détermination des orbites qui décrivent autour de leur centre de gravité deux étoiles très rapprochés l'une à l'autre", Connaissance des temps pour 1830 (Paris, 1827) 56 - 69, 163 - 71.
53. J. F. Encke, "Über die Berechnung der Bahnen der Doppelsterne", Berliner astronomisches Jahrbuch für 1832 (Berlin, 1830) 253 - 304.

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54. See A general index to the first 38 volumes of the Memoirs of Royal Astronomical Society (London, 1871); Contents of volumes i - lxiv of the Memoirs of the Royal Astronomical Society (London, 1929); A general index to the first 29 volumes of the Monthly Notices of the Royal Astronomical Society (London, 1870).
55. F. W. Bessel, Königsberger Beobachtungen, x (Königsberg, 1825); the catalogue was also published separately: "Verzeichniss von 257 auf der Königsberge Sternwarte beobachteten Doppelsternen", Astronomische Nachrichten, iv (1825) 301.
56. F. W. Bessel, "Vergleichung der gegenseitigen Stellungen von 37 Doppelsternen, welche sowohl in Königsberg als in Dorpat beobachtet sind", ibid., x (1833) 389.
57. F. W. Bessel, "Beobachtungen der gegenseitigen Stellungen von 38 Doppelsternen", Abhandlungen der Berliner Akademie der Wissenschaften: Mathematike Classe (1833) 41.
58. See M. A. Hoskin, "Sidereal astronomy in adolescence", Avant, avec, après Copernic (Paris, 1975) 314 - 42, 321; J. D. Fernie, "The historical search for stellar parallax", Journal of the Royal Astronomical Society of Canada, lxix (1975) 222 - 39, 230 - 33.
59. See M. von Rohr, Joseph Fraunhofers Leben, Leitsungen und Wirksamkeit (Leipzig, 1929); A. Seitz, Josef Fraunhofer und sein optisches Institut (Berlin, 1926); W. H. S. Chance, "The optical glassworks at Benediktbeuern", Proceedings of the Physical Society, xlix (1937) 433 - 43; H. C. King, The history of the telescope (London, 1955) *passim*.
60. F. G. W. Struve, Beschreibung des grossen Refractors von Fraunhofer (Dorpat, 1825).
61. The letter to von Zach was published as "Note sur la grande équatoriale de Fraunhofer", Correspondence astronomique, xii (1825) 282 - 95; that to Baily as "An account of the arrival and erection of Fraunhofer's large refracting telescope at the observatory of the Imperial University at Dorpat", Memoirs of the Astronomical Society of London, ii (1826) 93 - 100; the letter to Baily also appeared under the title "Farther account of the large achromatic refracting telescope of Fraunhofer in the observatory of Dorpat", Edinburgh Journal of Science, v (1826) 105 - 10.
62. Edinburgh Journal of Science (ref. 61) 105.
63. Ibid., 108.
64. Ibid.
65. F. W. Bessel, "Vorläufige Nachricht von einem auf der Königsberger Sternwarte befindlichen grossen Heliometer", Astronomische Nachrichten, viii (1830) 297 - 408. The paper was translated into English in R. Main, "On the present state of our knowledge of the parallax of the fixed stars", Memoirs of the Royal Astronomical Society, xii (1842) 1 - 60, 49 - 57, 51.
66. Ibid. (Main's translation) 50.
67. The heliometer was developed during the mid-eighteenth century as a micrometer, principally by John Dolland and Pierre Bouger; it was originally used to measure the diameter of the Sun. See King, History of the telescope (ref. 59) 150 - 2.

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68. Bessel, "Heliometer" (ref. 65) Main's translation, 49.
69. Ibid., 50.
70. Ibid., 56.
71. The other use to which Bessel frequently referred was the observation of Saturn.
72. For definitions and explanations of these terms see W. M. Smart, A text-book on spherical astronomy, 15th. edition (Cambridge, 1977) 244, 246.
73. See E. G. Forbes, Greenwich observatory: its origins and early history (London, 1975) chapter 4.
74. See J. F. W. Herschel, "Address ... on the presentation of the gold medal of the Royal Astronomical Society, to Professor Bessel ... for his observations and researches on the parallax of 61 Cygni"; published in Essays (London, 1857) 532 - 51.
75. F. W. Bessel, "Observations pour determiner la parallaxe annuelle de la 61e. étoile du Cygne; extrait d'une lettre de M. Bessel à M. de Humboldt", Comptes rendus (Paris, 1838) 785 - 95; partly reproduced by A Gautier in Bibliothèque universelle de Genève, xviii (Paris, 1838) 182 - 6. "Bestimmung der Entfernung des 61 Sterns des Schwans", Astronomische Nachrichten, xvi (1839) 65 - 84; also in Monatsberichte der Berliner Akademie der Wissenschaften (1838) 157 - 81. "Messung der Entfernung des 61 Sterns in Sternbilde des Schwans", Schumachers Jahrbuch für 1839 (Altona, 1839) 1 - 60. "On the parallax of 61 Cygni", Monthly Notices of the Royal Astronomical Society, iv (1839) 152 - 70; partly reproduced in The Philosophical Magazine, xiv (1839) 63 - 72.
76. Struve, Mensurae micrimetricae (ref. 35) chapter 14.
77. T. Henderson, "On the parallax of α Centauri", Memoirs of the Royal Astronomical Society, xi (1839) 61 - 8. Letter written 24 December 1838 and read to the Society 3 January 1839.
78. All the general accounts of the history of attempts to measure parallax of course cover this period; see also N. S. Hetherington, "The first measurements of stellar parallax", Annals of Science, xxviii (1972) 319 - 25; and O. Struve, "The first stellar parallax determination", in H. M. Evans (ed.) Men and moments in the history of science (Seattle, 1959) 177 - 206.
79. In grand nineteenth century fashion, Agnes Clerke tried to work out who announced parallax measurements first, and whom therefore to acclaim. A historically more sophisticated approach was adopted by Hoskin and Hetherington who tried to establish the sequence of events, and then whose observations contemporary astronomers believed. Finally Otto Struve, whose paper is by far the most detailed, tried to share the laurels.
80. See the following letters: Bessel to Olbers 5 December 1830, Olbers-Bessel Correspondence, ii, 340 - 51; Olbers to Bessel, 16 November 1830; 12 March 1831; ibid., 337 - 40; 351 - 5.
81. Paper in Philosophical Magazine (ref. 75) 68.
82. Ibid.

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83. In 1826 Bessel had introduced a new method for determining the length of the seconds pendulum; his method was adopted in several observatories and when new experiments on the pendulum were undertaken it was inevitable that Bessel would be consulted.
84. F. G. W. Struve, Mensurae micrometricae (ref. 35) clxix; translation from the French (see ref. 86) 41: "... α de la Lyre, la plus brillante du ciel boréal, est très propre à la détermination de la parallaxe, parce que sa grande distance de l'équateur fait que, pour nous qui habitons le nord, cet astre ne quitte jamais l'horizon, et que dans toutes les saisons de l'année il nous est permis de l'observer la nuit à une grande élévation ... "
85. Ibid., clxxii.
86. F. G. W. Struve, Étoiles doubles: mesures micrometriques ... (St. Petersburg, 1837).
87. See Astronomische Nachrichten, xiv (1837) cols. 249 - 52.
88. O. Struve (ref. 78) 203.
89. Bessel (ref. 75) Philosophical Magazine, 69.
90. Bessel to Olbers, 12 October 1837, Olbers-Bessel Correspondence, ii, 419 - 24.
91. Bessel to Olbers, 3 June 1838, ibid., 424 - 6.
92. Bessel is unclear about whether the values he selected for α and β were observed himself, or taken from catalogues.
93. Bessel (ref. 75) Philosophical Magazine, 71.
94. See the following letters: Bessel to Gauss, 28 June 1839, Gauss-Bessel Correspondence, 526 - 9; Bessel to Steinheil, 23 June 1839, printed in W. Englemann (ed.) Briefwechsel zwischen Bessel und Steinheil (Leipzig, 1913) 216 - 8.
95. Bessel (ref. 75) Philosophical Magazine, 68.
96. See J. D. North, "Thomas Henderson", Dictionary of Scientific Biography, vi (New York, 1970) 263.
97. It is odd that Henderson should still have been unaware of the high proper motion of α Centauri on his return from the Cape. This argues that he can have done no reduction of his data while he was away.
98. In order to improve the accuracy of the final position allocated to stars, John Pond had introduced the idea of observing stars directly, and observing their reflected images - usually in a trough of mercury - then averaging the two values of declination thus obtained. Clearly Henderson was applying a similar method.
99. Henderson (ref. 77) 68.
100. See Fernie, "The historical search" (ref. 58) 237.
101. G. B. Airy, "On the parallax of α Lyrae", Memoirs of the Royal Astronomical Society, x (1838) 265 - 70; letter written 23 June 1837 and read to the Society 24 November 1837.

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102. From A. P. Norton and J. G. Ingliss, Norton's Star Atlas, 15th. edition (Edinburgh, 1966) 53, the modern value of the parallax of 61 Cygni is 0".29.
103. See the correspondences between Bessel and Olbers, Bessel and Gauss for the years 1838 to 1840.
104. See ref. 75, Comptes rendus.
105. A. Arago and M. Mathieu, "Note sur la parallaxe ... " L'Annuaire du Bureau des Longitudes de 1834 (Paris, 1834); reprinted in ibid., 793 - 5.
106. It is significant that Arago and Mathieu should have started to observe 61 Cygni in August 1812, precisely the month when Bessel's study was published in the Mon Corr. See above, section 5.2.
107. Arago (ref. 102) 794. He offered no reduction of his data, and as he was trying to measure absolute parallax from observations of zenith distance his conclusions were worthless.
108. A. Gautier, "Détermination de la parallaxe annuelle de la 61e. du Cygne" (ref. 75).
109. Ibid., 183: "M. Bessel vient d'arriver, par une autre méthode d'observation, à un résultat assez rapproché du précédent, et d'où il croit pouvoir déduire une quantité déterminée pour l'élément cherché."
110. Main (ref. 65).
111. J. F. W. Herschel, Address (ref. 74).
112. Ibid., 533.
113. Olbers to Bessel, 5 July 1838, Olbers-Bessel Correspondence, ii, 427 - 9, 428.
114. See F. W. Bessel, "On his catalogue of 27 stars of the Pleiades; continuation of the observations on the parallax of 61 Cygni", Monthly Notices of the Royal Astronomical Society, v (1839) 7 - 11; reprinted in The Philosophical Magazine, xvi (1840) 150. It is interesting that the letter of confirmation should have been addressed to Baily rather than to Herschel; this indicates that it was not solely language problems that prompted Bessel to write to Herschel in the first place.
115. F. G. W. Struve, "Ueber die Parallaxe des Sterns α Lyrae ... ", Astronomische Nachrichten, xvii (1840) cols. 177 - 80.
116. T. Maclear, "On the parallax of α Centauri", Memoirs of the Royal Astronomical Society, xii (1842) 329 - 57.
117. Further evidence of this may be found in a letter from Bessel to Airy, 3 March 1839, R. G. O. MS 938 section 5: "The evidence of the accuracy of my result, respecting the annual parallax of 61 Cygni is provided only by the number of the observations of two stars ... "
118. F. G. W. Struve, Études d'astronomie stellaire (St. Petersburg, 1847) 94 - 108.
119. Compare ibid., 99 with Norton's (ref. 102) 53.

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120. Herschel, Address (ref. 74) 549 - 50.

Notes to Chapter Seven: Conclusions

1. R. Hooke, An attempt to prove the motion of the Earth (London, 1674) 2.
2. It is possible that the standard nineteenth and twentieth century view that astronomy in the eighteenth century was unreservedly Copernican could be investigated further. It is unlikely that much anti-Copernican feeling among astronomers would be revealed, but if doubts were being expressed it is important that their extent be assessed if we are to understand as fully as possible astronomical endeavour in the eighteenth century.
3. J. Pond, "On the parallax of the fixed stars", Phil Trans, cvii (1817) 161.
4. Ibid.
5. Ibid.
6. I. Newton, Mathematical Principles of Natural Philosophy, 3rd. edition (1726); English translation by A. Motte (1729) and revised by F. Cajori (1960), Cajori edition, 422.
7. S. Schaffer, review of N. Calder, The comet is coming, London review of books (February 1981).
8. See A. Clerke, A history of astronomy during the nineteenth century 3rd. edition (London, 1893) 87 - 8.
9. Letter from Olbers to Bessel, 21 October 1821, Olbers-Bessel Correspondence, ii, 212.

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APPENDIX 1

"Flamsteed's alleged measurement of
annual parallax for the Pole Star"

J. H. A., x (1979) 102 - 16

FLAMSTEED'S ALLEGED MEASUREMENT OF ANNUAL PARALLAX FOR THE POLE STAR

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Introduction

The problem of detecting annual parallax among the stars was of concern to most astronomers of the late seventeenth century. Genuine observation of parallax would provide strong support for Copernicus's claim that the Earth moves and, of greater importance by this time, an accurate measurement of parallax could be used to determine stellar distances.

During the second half of the century various attempts were made to measure parallax: Robert Hooke claimed a value of 27-30" for the star γ Draconis;¹ Jean Picard, one of the founder members of the *Académie royale des Sciences*, observed an annual variation in the position of the Pole Star over a period of ten years and published an account of this in his *Voyage d'Uranibourg* of 1680;² J. D. Cassini made a similar series of observations over an even longer period.³ But none of these attempts was considered to be genuine evidence of parallax by contemporary astronomers. Hooke had made too few measurements, Picard's observations were incomplete, and Cassini's inaccurate.⁴

Unlike the reaction to these accounts, that to Flamsteed's work on the Pole Star was quite favourable—initially at least: several eminent astronomers were convinced parallax had been detected at last. His results appeared in the form of a letter written to John Wallis, the Savilian Professor of Geometry at Oxford, which Wallis included in the third volume of his *Opera mathematica*.⁵ The value of 40" for the annual parallax of the Pole Star given in the letter seemed to be derived from very accurate observations and was initially accepted as genuine—particularly by many British astronomers.

It was not long, however, before Flamsteed's account was challenged. The letter to Wallis was first published in 1699, and before the end of the same year the Frenchman Jacques Cassini wrote to the *Académie des Sciences* claiming that what Flamsteed had observed could not be parallax, as, for the Pole Star, the variation of its position due to parallax would be different from that which Flamsteed observed. Cassini's objection was valid; and having studied it, Flamsteed was obliged to retract. Why was it, then, that Flamsteed should have been so certain he had detected parallax, and why was his account believed when it was first published? Several historians have pointed out that Flamsteed's observations provide very clear evidence of aberration, an effect which is 90° out of phase with that of parallax; but it has not been explained how this discrepancy could have escaped Flamsteed's notice.

Events leading to the publication of Flamsteed's results

Flamsteed's observations of the Pole Star were gathered over a period of seven years while he was employed by Charles II as his "astronomical observer" at the newly founded Royal Observatory at Greenwich.⁶ The original purpose of the observatory was to find new methods of calculating terrestrial longitude at sea, from detailed observations of the positions of celestial bodies.⁷ Flamsteed

was appointed as the first Astronomer Royal when the observatory opened in 1675, and was commissioned to provide tables of the positions of the fixed stars with as great an accuracy as possible.

The problem of stellar parallax was first investigated by Flamsteed in the late 1670s. In a letter⁸ he wrote to Seth Ward, the Bishop of Salisbury, in January 1680 he gave an account of a phenomenon he believed was due to parallax. Flamsteed selected seven stars lying near the plane of the ecliptic and measured the angular distance of each from the local vertical. Using a value corrected for refraction for the latitude of Greenwich, he calculated the differences in right ascension between successive stars, which differences he then added together. If his original measurements were absolutely accurate this sum should be 360°; however, Flamsteed calculated that because of refraction (which causes the star to appear higher in the sky than it actually is) the total should be 5'24" less than 360°. But when the calculation was carried out the difference was only 38", implying that some counter-effect was present in addition to the effect of refraction.⁹ According to Flamsteed, this could be "no other than the parallax of the Earth's orb".¹⁰ Unfortunately, Flamsteed was being hasty in this assumption: his letter to Ward can be seen more as an attempt to impress him in the hope of gaining his future patronage than as a genuine belief in the successful identification of stellar parallax.¹¹

It was apparently not until the 1690s that Flamsteed returned to the problem of measuring stellar parallax. Between his appointment as Astronomer Royal and the end of the 1680s, his time was devoted to making precise observations of the positions of celestial bodies. By 1689 he had made over 20,000 such observations using a sextant to which was attached a telescope with a fitted micrometer eye-piece. He claimed an accuracy of about 10" in using this instrument, but still he hoped to build another which would give even greater accuracy. By the end of the summer of 1689 a new 140" mural arc, the radius of which was almost seven feet, had been constructed under Flamsteed's supervision by the mathematician Abraham Sharp. Flamsteed believed that with this arc he could get accuracies to within 5";¹² it was this instrument which he used for all his observations over the next seven years, including therefore those of the Pole Star which he was to use to support his parallax theory.

Early in 1695, Flamsteed received a request from Newton for an accurate determination of the latitude of the Greenwich Observatory.¹³ To meet this request, Flamsteed chose to compare his various Pole Star readings, and he therefore gathered together and tabulated all the observations he had made of that star over the previous five or six years. These observations had been corrected for refraction, and for the slight subsidence of the wall to which the mural arc was secured (this had been noticed by Flamsteed before the end of 1689, so all his observations were corrected accordingly). But even after these corrections had been made, Flamsteed noticed that there was still a systematic discrepancy between the position of the star in summer and in winter, about which he said in his 1699 letter to Wallis: "I realized [they] could only have resulted from parallax."¹⁴ His first reaction was to inform his friend John Caswell, who was then lecturing in mathematics at Oxford. Caswell visited Flamsteed during the summer of 1695 to discuss the observations, and on his return to Oxford wrote to Wallis, informing him of the discovery of stellar parallax, and to Flamsteed urging him to publish his work.

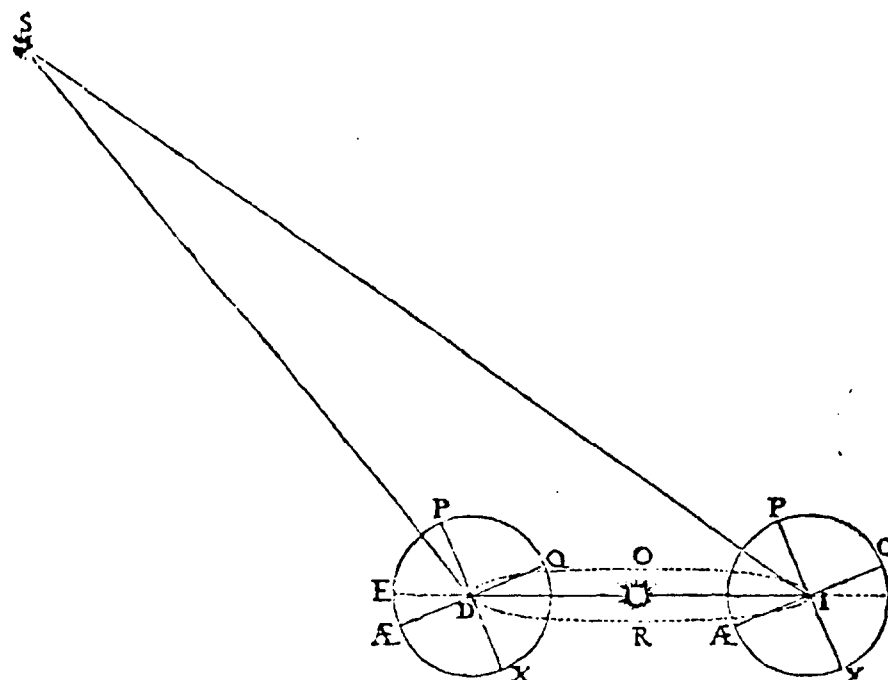


FIG. 1. From Flamsteed's letter to Wallis, reproduced in Wallis's *Opera mathematica*, iii, 706.

Partly because of other projects he was working on, for example the solar tables he was preparing, and partly because of his wish to confirm his theory with further observations, Flamsteed delayed publishing for another two or three years. Then in February 1698, he wrote to Caswell again,¹⁵ enclosing certain of his results, and asking him to forward them to Wallis. Having seen these results, Wallis wrote a series of letters to Flamsteed during the summer and autumn of 1698,¹⁶ suggesting he publish a full account of his observations as soon as possible, in case anyone else should publish their results first. Flamsteed therefore tabulated his seven years of observations and sent them, together with his explanation of annual stellar parallax, to Wallis in a letter dated 20 December 1698, which Wallis published the following year.

Flamsteed's results and theory

The first part of Flamsteed's letter is devoted to explaining why, in his view, his predecessors failed to detect parallax: he believed that their instruments were not sufficiently sensitive. As evidence he gives examples from Tycho's *Historia coelestis* of two observations of the Pole Star made within six days of one another, differing by about $\frac{1}{4}'$. As Flamsteed believed the total effect of parallax over six months amounted to about the same angle, Tycho's observations were obviously not accurate enough for him to have detected parallax. A detailed description of Flamsteed's own 140' mural arc then follows, as he wishes to emphasise its superior accuracy. His description includes an estimation of the limits of accuracy obtainable with such an instrument, and he concludes that because of the coarseness of the metal on which the scale is engraved, and the

limitations of human senses, the greatest accuracy possible must be within 5" or 10"; but he had achieved this, and his was the best instrument of its kind in the world.¹⁷

By securing the arc to a wall built in the meridian plane, with 90° of the arc going from the vertical to the south, and the other 50° from the vertical to the north, Flamsteed was able to observe the angular distance from the North Celestial Pole of any star within 11½° of the pole, as it crossed the meridian (the latitude of the observatory being about 51½° north). All such stars are circumpolar, and thus cross the meridian twice in twenty-four hours.

When a star crosses the meridian, it is at either its maximum or its minimum distance from the vertical, because only then are the star, the vertical and the North Celestial Pole on the same great circle. The observations Flamsteed gathered therefore were of the greatest and least distances of stars from the local vertical, he observing them as often as the season and weather conditions allowed. When presenting his results for the Pole Star he considers observations made with an interval of at least three months between them. He notes the distance of the star from the local vertical at the beginning of the interval, and then, allowing 20" per year for precession,¹⁸ he calculates the distance which would be expected at the later date. This is then compared with the recorded distance, and the difference between the two values is taken by him to be a measure of the parallax due to the orbit of the Earth. For example:¹⁹

VIII (Upper culmination)

On 17 July 1693 the shortest distance of the Polar Star from the vertex was observed to be.....	36°09'55"
By adding ²⁰ 10" for the movement towards the pole until the following December, we arrive at what should be the distance in December.....	36°10'05"
But on 9 and 19 December, it was observed to be.....	36°10'40"
Therefore its distance from the pole was discovered to be greater in July than in December as a result of parallax by.....	00°00'35"

IX (Lower culmination)

On 7 and 19 December 1693 its greatest distance was observed to be.....	40°50'30"
By taking away ²⁰ 5" or 10" for its movement towards the pole from then until the following May, we arrive at..	40°50'25" } 20"
But on 5 May 1694 it was observed to be.....	40°51'00"
Therefore its distance from the pole was greater in May than in the preceding December as a result of parallax by	00°00'40" } 35"

Altogether, Flamsteed records fifteen such collections of data, each one showing the star to be closer to the pole in the winter months than in the summer. To explain why this should be so he uses Figure 1. In it *IRDO* represents the orbit of the Earth around the Sun, *D* the position of the Earth in December, *I* its position in June, *☉* the Sun, and *S* the Pole Star. By studying the diagram it is clear that the celestial latitude (defined as the angle of elevation from the ecliptic plane *IRDO*) of the Pole Star in June, given by angle *SIE*, is less than that in December, given by angle *SDE*. According to Flamsteed, and we shall

see that it is here that he makes his fatal mistake, it follows that the star's declination (defined as the angle of elevation from the celestial equator) is less in June than in December, and thus its angular distance from the Pole is greater in June than in December. This is well substantiated by his observations.

In the remainder of the letter, Flamsteed explains that the readings he is presenting have already been reduced to allow for refraction, and for instrumental errors (such as the subsidence of the wall); thus the discrepancy he has observed between the positions of the Pole Star in summer and winter cannot be due to these phenomena. He also goes into some detail to show that the effect he has discovered cannot be due to a nutation of the Earth's axis²¹—as any displacement caused by nutation would be in such a direction that the Pole Star would appear to be nearest the Pole at the time of the equinoxes (that is, in positions *O* and *R* in Figure 1), and farthest from it at the solstices (*I* and *D*). So Flamsteed has now eliminated all the possibilities he can think of which might explain his observations; the only answer, as far as he is concerned, is that they represent the parallactic effect of the annual orbit of the Earth around the Sun.

Finally, Flamsteed gives a short account of Hooke's supposed detection of annual parallax for γ Draconis, and of the possible parallax of Sirius (calculated from a few readings Flamsteed possesses for that star). Unlike most of his contemporaries, Flamsteed was prepared to accept Hooke's value of parallax for γ Draconis, although probably only because it was in approximate agreement with the value Flamsteed gave for the Pole Star. He is less certain of the accuracy of the observations of Sirius, however, because of the presence of dust on his instrument, and because they were not all carried out by himself. He says they merely *suggest* a value of 30" for the parallax of Sirius. However, both these results, and Hooke's work on γ Draconis support Flamsteed's general belief that parallax due to the Earth's orbit is detectable, and that he has detected it.

Reaction to Flamsteed's account

One of the earliest references to Flamsteed's conclusions was made during a series of lectures on astronomy given by William Whiston at Cambridge,²² where he had recently succeeded Newton as Lucasian professor. In his lecture on stellar parallax, Whiston refers to Flamsteed as "our great observer", and goes on to give a summary of the letter to Wallis. From this lecture it is evident that Whiston welcomes the work done by Flamsteed and is convinced that his detection of parallax is genuine and that the distance of the Pole Star is some 9000 astronomical units.²³

Not everyone was convinced by Flamsteed's account. In 1702, the mathematician and astronomer David Gregory gave his opinion of the claims made by Flamsteed.²⁴ Gregory did of course believe the Earth moves, and he accepted that if parallax were detectable it would manifest itself as Flamsteed suggested; his argument was that he was "inclined to believe with Copernicus that the diameter of the Orbis Magnus in regard of the Fixed Stars (even the nearest) is insensible".²⁵ He gave an alternative explanation of Flamsteed's observations, based on his theory that the densities of the two hemispheres of the Earth are different. Having pointed out that Flamsteed's account "supposes the axis of the Earth . . . to be exactly parallel to itself"²⁶ in summer and in winter, he claimed this would not be so if the two hemispheres were of different densities.

This is because in summer and winter opposite hemispheres face the Sun, and are thus closer in turn to it. Therefore, if the hemispheres had different densities the net force on the Earth, and hence the tilt of the axis, would vary very slightly. Gregory's argument was severely criticised by Whiston, in a lecture he gave in October 1705 in which he condemns Gregory for not producing quantitative results to support his theory.²⁷ Had he done so (Whiston argues) he would have realized that if the phenomenon did affect the position of the Pole Star in the way Gregory suggests (which Whiston is far from believing anyway), the difference in positions in summer and winter would amount to less than a second of arc, and so this could not be the effect Flamsteed had observed. The concluding section of Whiston's lecture demonstrates that he was aware also of objections raised by Cassini to Flamsteed's account, but that he still believed the detection of parallax was genuine.

The only clues to Flamsteed's own reaction to Gregory's attacks can be found in the private letters he wrote to Caswell and Wallis concerning the various references made by Gregory to him in Gregory's *The elements of physical and geometrical astronomy*, first published in Latin at Oxford in 1702. In one of these letters, written to Wallis in October 1702,²⁸ Flamsteed says "I am obliged to you for . . . the candour in acquainting Dr. Gregory so freely with your dislike of his endeavours to invalidate a truth, which has been incontestably proved, by eight years continued observations". It is most likely that here Flamsteed is speaking of his account of parallax for the Pole Star, and this indicates that at this time both he and Wallis still believed that Flamsteed had detected parallax. (But, as we shall see, about one month later Flamsteed changed his mind, having seen the criticism of his theory given by Cassini.) However, this letter to Wallis was not published until 1835 (in Francis Baily's *Account of Flamsteed*) and so it appears that Flamsteed made no public defence of his theory against Gregory's attacks. Possibly he did not think the criticism was worth a reply in public, and it is certain that he was very busy with other projects during the early years of the eighteenth century.

Reaction from European astronomers to Flamsteed's letter to Wallis was not altogether favourable. The Danish astronomer Ole Roemer had spent several years during the 1690s trying to measure parallax for the fixed stars but, although he did detect an annual variation in some of them, he put it down to instrumental error and devoted much of his time to making his instruments more precise.²⁹ Roemer's initial reaction to Flamsteed's work was to claim that it was not parallax that had been detected. While thanking Flamsteed profusely for notifying him of his observations, he suggested, in a letter written to Flamsteed in the second half of 1701,³⁰ that the phenomenon of parallax would not manifest itself as Flamsteed believed. In this letter, Roemer gave his theory that the whole effect of parallax, in both June and December, must be embodied in the star's right ascension, with no difference in declination, therefore, between these dates; and that the maximum difference between the distance of the Pole Star from the pole should occur between March and September. His theory was in fact essentially correct, but rather tentatively suggested, and Flamsteed appears not to have been disturbed by it.

By October 1702, then, Flamsteed had heard of criticisms of his account from Gregory and from Roemer. So far, apart from defending himself against several attacks from Gregory in his own private correspondence, Flamsteed appears

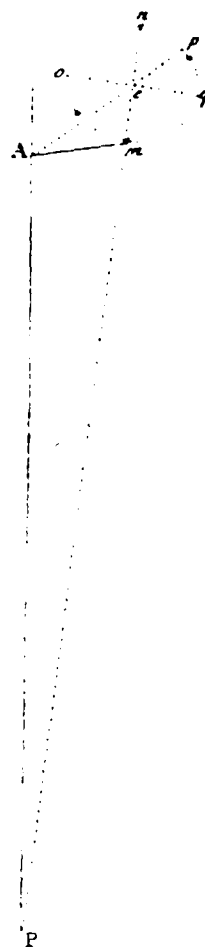


FIG. 2. From Flamsteed's letter to Sir Christopher Wren, reproduced from Stephen Wren, *Parentalia*, following p. 248. The angles are not accurately drawn but the figure illustrates Flamsteed's conception of the problem. *A* represents the North Celestial Pole and *P* the North Ecliptic Pole.

not to have been greatly worried by this criticism: the major defence of his theory came from Whiston. And from the comment made by Flamsteed to Wallis in October 1702, it may be assumed that at that date he still believed he had detected parallax. However, soon after writing this letter he must have read the arguments against his theory put forward by the Frenchman Jacques Cassini, for during November 1702 he wrote to Sir Christopher Wren, giving a revised theory of parallax.

Cassini's criticism of Flamsteed's work was by far the most substantial to be produced. It appeared in a letter written to the *Académie des Sciences* in 1699.³¹ In this, having given a synopsis of Flamsteed's account, Cassini pointed out that Flamsteed is wrong when he says that it follows that if the celestial latitude of a star is greater at one time of year than at another, then its declination obeys

the same rule. This criticism is well-founded; and since the validity of Flamsteed's account depends on the relationship he cites, by questioning it Cassini was undermining the whole basis of Flamsteed's letter to Wallis. The relationship between ecliptic coordinates (λ, β) and equatorial coordinates (α, δ) is not straightforward, as the following equations show:

$$\sin \delta = \cos \epsilon \sin \beta + \sin \epsilon \cos \beta \sin \lambda \quad (1)$$

$$\cos \delta = \frac{\cos \lambda \cos \beta}{\cos \alpha} \quad (2)$$

(where ϵ is the obliquity of the ecliptic).

In his letter Cassini goes further than merely pointing out Flamsteed's gross theoretical error: he shows that what Flamsteed has observed cannot be the effect of parallax as that "would demand a variation different from that which is observed".³² According to him, the Pole Star reaches its maximum and minimum distances from the North Celestial Pole in late March or early April, and late September or early October, respectively—not in June and December as Flamsteed believes. Again Cassini was right, although, as will be shown later, his explanation was not very clear.

To calculate when the star does pass closest to and farthest from the pole, the equations governing the variation in stellar positions as a result of parallax must be used. Working in ecliptic coordinates these are:

$$(\cos \beta) \Delta \lambda = \pi \sin (\odot - \lambda) \quad (3)$$

$$\Delta \beta = -\pi \sin \beta \cos (\odot - \lambda) \quad (4)$$

(where π is the star's parallax, and $\odot$ the geocentric longitude of the Sun). What is required is the value of $\odot$ for which $\Delta \delta$ is minimum or maximum.

From (1),

$$(\cos \delta) \Delta \delta = (\cos \epsilon \cos \beta) \Delta \beta + \sin \epsilon [(\cos \beta \cos \lambda) \Delta \lambda - (\sin \lambda \sin \beta) \Delta \beta].$$

On the celestial sphere, the differential may be considered as a two-dimensional vector displacement. If this vector is represented by x_i in the equatorial frame of reference and x'_j in the ecliptic frame of reference,

$$x'_j = R_{ji} x_i, \text{ where } x'_1 = (\cos \beta) \Delta \lambda, x_1 = (\cos \delta) \Delta \alpha \\ x'_2 = \Delta \beta, x_2 = \Delta \delta$$

and R is a rotational transformation, the angle of which is S , S being the angle between the equatorial and ecliptic coordinates at the star. Thus

$$\begin{pmatrix} (\cos \beta) \Delta \lambda \\ \Delta \beta \end{pmatrix} = \begin{pmatrix} \cos S & \sin S \\ -\sin S & \cos S \end{pmatrix} \begin{pmatrix} (\cos \delta) \Delta \alpha \\ \Delta \delta \end{pmatrix}$$

from which

$$\Delta \delta = (\sin S) (\cos \beta) \Delta \lambda + (\cos S) \Delta \beta.$$

Combining with (3) and (4) we get:

$$\Delta \delta = \pi (\sin S) (\sin (\odot - \lambda)) - \pi (\cos S) (\sin \beta) \cos (\odot - \lambda).$$

For extremal values we set the differential equation equal to zero. The resulting equation is:

$$\tan (\odot_m - \lambda) = \frac{-\tan S}{\sin \beta}.$$

$\odot_m$ being the required value of $\odot$.

To evaluate S , consider the spherical triangle AeP (Figure 2): $\angle AeP = S$; $\angle APe = 90 - \lambda$; $\angle eAP = 90 + \epsilon$; AP is the obliquity of the ecliptic, ϵ ; Pe is the

complement of β : Ae is the complement of δ . By using the sine rule for spherical triangles,

$$\sin S = \frac{\sin \epsilon \cos \lambda}{\cos \delta}.$$

By using the cosine rule,

$$\cos S = \frac{\cos \epsilon - \sin \beta \sin \delta}{\cos \beta \cos \delta}.$$

From the data of the Smithsonian Astrophysical Observatory Catalogue, one finds (for the mean equator and equinox of 1690.0) the position:

$$\alpha = 8^\circ 5' \quad \delta = 87^\circ 64'.$$

Adopting Newcomb's obliquity for 1690.0 ($23^\circ 48'$), we find:

$$\lambda = 84^\circ 23' \quad \beta = 66^\circ 06'.$$

From these data and the formulae above for S , we find that

$$S = 76^\circ 25'.$$

Thus, from our equation for extremal Polaris declination, we get:

$$\odot_m = 7^\circ \text{ or } 187^\circ.$$

If we wish to take into account the eccentricity of the Earth's orbit, taking $\odot$ as the Sun's true longitude, these results become:

$$\odot_m = 8^\circ \text{ or } 186^\circ,$$

which correspond respectively to

$$L_m = 6^\circ \text{ or } 188^\circ$$

where L is the Sun's mean longitude. As L is virtually a linear function of time, it is easily determined that the corresponding dates in the late seventeenth century are 28 March and 28 September respectively.³³ So Cassini's estimates were correct.

However, as pointed out before, Cassini's own justification of his beliefs was rather confused and depended very heavily on his third figure (see our Figure 3). In this figure, which depicts part of the northern hemisphere of the fixed stars, the small circles BDF , CBE represent the paths in the sky followed during the year by the pole P of the ecliptic and the North Celestial Pole A , in consequence of the movement of the Earth in its orbit. These poles move because they are always defined with respect to the Earth; and so, if the Earth moves, they must move too.

The important feature of the diagram is the position of the Pole Star S relative to the two circles: to plot its position, Cassini used ecliptic coordinates, which he gave as 24° from the North Ecliptic Pole (that is ecliptic latitude of 66°), and 22° into the constellation Gemini, or 82° from the first point of Aries. Cassini then shows that for the Pole Star Flamsteed's general rule for the relationship between ecliptic latitude and declination is incorrect. In the configuration given, the arc DS represents the complement of the Pole Star's ecliptic latitude in December, and FS the equivalent in September; the arc SB (where B is on the circle CBE) represents the complement of the star's declination in December, and SE that in September. Merely by inspecting these lines it can be seen that SB is longer than SE , while SD is shorter than SF , which is contrary to Flamsteed's general rule.

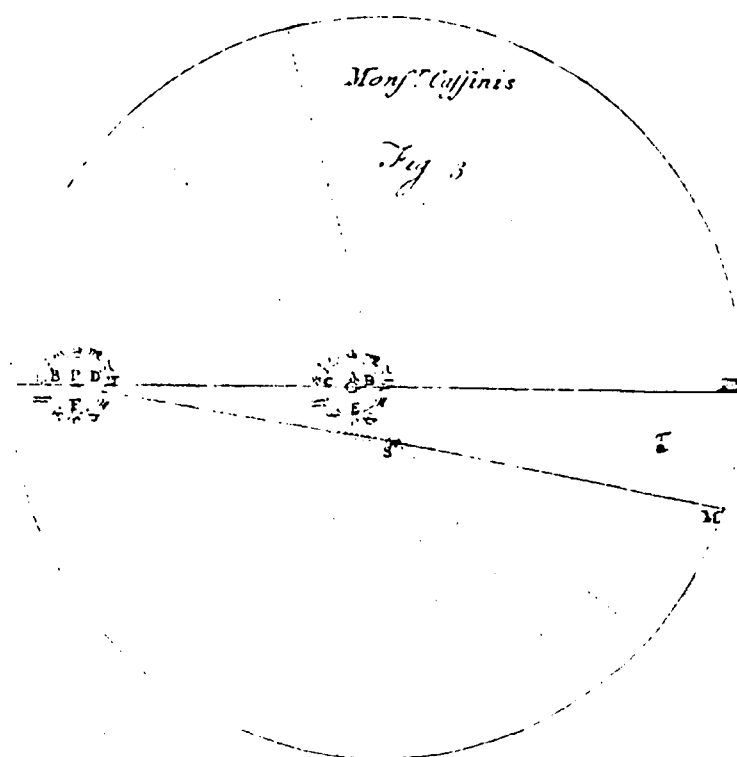


FIG. 3. From Cassini's letter to the *Académie des Sciences*, reproduced from Stephen Wren, *Parentalia*, following p. 248.

Having exposed the fallacy in Flamsteed's argument, Cassini constructs a line from the Pole Star through the circle representing the path of the North Celestial Pole. The points where the line cuts the circle mark where the pole passes closest to and farthest from the Pole Star. According to Cassini, the pole actually so passes at the end of September or beginning of October, and the end of March or beginning of April, respectively. He therefore concludes that what Flamsteed had observed cannot be due to annual parallax.

Cassini had obviously studied the phenomenon of parallax in great detail and was in a position to recognise the mistake in Flamsteed's account almost immediately. His own interest in the detection of parallax was probably inspired by the interest of his father, J. D. Cassini, in the problem, an interest that went back at least to 1671. From then until 1691 J. D. Cassini made a series of observations which (as we have mentioned) were published at the end of the century. The speed with which Jacques Cassini responded to Flamsteed's account—writing to the *Académie des Sciences* within a few weeks of the publication of Flamsteed's letter to Wallis—shows how certain he must have been about the way in which parallax would be exhibited by the Pole Star. He did in fact succeed in forcing Flamsteed into rethinking his theory of parallax, and the fruit of this was probably the first fully-illustrated and clearly-stated

account of how a star appears to move as a result of the annual movement of the Earth--a subject that, as Flamsteed said, "has not hitherto (as I know of) been handled by anybody".⁴¹

Flamsteed's reply to Cassini

Flamsteed answered Cassini's attacks in a letter addressed to Sir Christopher Wren in November 1702.⁴² The letter was not published during Flamsteed's lifetime, which may indicate that although he was forced to admit his first theory of parallax was wrong, he was reluctant to make a public retraction. It seems likely that he wrote to Wren in reply to a direct enquiry from him concerning Cassini's criticism. The tone of the letter as a whole shows Flamsteed as reluctant to concede anything to the Frenchman, and very ready to counter-attack wherever possible.

Before Flamsteed admits that Cassini may have a valid point, he gives a full account of how any star would appear to an observer on Earth to move as a result of the Earth's motion. By doing this he shows how Cassini could have presented his argument more straightforwardly. He begins by pointing out that it is far simpler to assume that the Earth is at rest, and that the star moves in an ellipse, parallel to the plane of the ecliptic, but in the opposite direction to which the Earth is moving. Then, with the aid of two diagrams, he gives an account of how this path will appear to an observer on Earth, when it is projected onto the celestial sphere, and how the path will differ for stars of different ecliptic latitudes. Stars lying in the ecliptic plane (that is, with zero latitude) will appear to move backwards and forwards along a straight line; a star coinciding with the pole of the ecliptic will seem to move in an ellipse, with the pole at one focus, and parallel to the ecliptic plane. At any latitude between these two extremes, the star will appear to move round an ellipse, whose major axis is parallel to the ecliptic plane, and whose minor axis increases in length as the latitude increases.

After describing parallactic displacements in general, Flamsteed comes to his discussion of the Pole Star, and of the times of year between which the maximum difference in the position of that star will be observed. It is then that he has to concede that they are "perhaps not much different from the times on which Mr. Cassini places them".⁴³ He arrives at this conclusion by considering the ecliptic coordinates of the Pole Star, constructing its parallactic ellipse, and showing on a diagram its position relative to the North Celestial Pole and the North Ecliptic Pole (see Figure 2).

From the diagram, it is obvious that the Pole Star appears to be closest to the North Celestial Pole when it is at r (the point of intersection of Ae with the arc omq), and farthest away when at p . To calculate the exact times of year when the star appears to be at these positions involves solving the following problem: "A Point being given, and an Ellipsis described in the Superficies of the Sphere, to strike two Arches of Great Circles through the said Point to the Ellipsis, so as one of them shall be the shortest, the other the longest that can be betwixt them."⁴⁴ The analytic solution to this problem was given above, but like Cassini when he is considering the movement of the North Celestial Pole relative to the Pole Star, Flamsteed gives a diagrammatic solution to the problem. In giving a clearer account of Cassini's own argument he is forced to the conclusion that his rival is right.

The difference between the position of the Pole Star in summer and winter which Flamsteed had observed he now puts down to instrumental error, and he outlines his programme with regard to the future detection of parallax. He intends to study his vast collection of observations to find out what defect in the mural arc could be causing the effect he has noticed; then, having corrected his data for this, he must study them all again in order to find evidence supporting his new theory of parallax. We note that he never considers the possible existence of any other physical phenomenon which could be causing the observed effect. Nor was there any reason why he should. He had no notion about the aberration of light; indeed, the idea that light had a finite speed had only recently been suggested by Roemer,³⁸ and was far from being accepted. For example, Cassini, who was a convinced Cartesian, rejected the idea altogether; and if Flamsteed did accept it, which is uncertain, it did not occur to him to consider whether this would affect his observations of the stars. He certainly never saw in the finite velocity of light the cause of the discrepancy he had detected in the position of the Pole Star. He was convinced that there must be some systematic alteration of the mural arc, and his course of action was therefore obvious: find the cause of the error, allow for it, and then look for evidence of parallax again.

It is interesting, however, that there seems to be no evidence that he ever did return to the problem of detecting parallax. A likely explanation is that he could not find an instrumental cause for the systematic discrepancy, because it did not exist: as C. A. F. Peters has shown,³⁹ the accuracy Flamsteed obtained was in fact very high. Also, much of his time from the turn of the century until his death in 1719 was taken up with arguments with Newton, Halley, Gregory and Wren over the publication of his star catalogue.

Conclusion

What remains to be considered now is *why* it was that Flamsteed should have observed evidence for one phenomenon, but put it down to another which is 90° out of phase with the first—and why his account was believed.

The basic fault in Flamsteed's account of what he believed was measurement of annual parallax for the Pole Star, is that the theory on which he based his conclusions is wrong. However, it is not obviously wrong; on first reading it seems to make perfect sense and appears to be a sound explanation of the observations made by Flamsteed. Careful consideration of the geometry involved is required before Flamsteed's error can be exposed. In the figure he draws (our Figure 1) the exact position of the North Celestial Pole is unclear, as is which points are coplanar (a situation not helped by the fact that all drawings must necessarily be two-dimensional). There is no reason to assume that the Pole Star and the North Celestial Pole lie in the same ecliptic longitude, although this is implicit in Flamsteed's diagram and reasoning. In fact they do not: the longitude of the pole is 90° and that of the Pole Star 84°15' (in 1690). But until this is realised, Flamsteed's argument seems complete and as such is very convincing. He systematically considers all the relevant phenomena then known, allowing for them or discounting them for clearly expressed reasons, leaving only one possible explanation—that the effect he has observed must be parallax. As for the measurements on which he based his argument, he was known to be a very careful observer. Taken as a whole, therefore, Flamsteed's letter to Wallis had the appearance of an account of a genuine detection of parallax well supported by reliable observations.

Flamsteed's reasoning certainly convinced almost all his contemporaries. Even Roemer, who had studied the phenomenon of parallax for some time and did not agree with Flamsteed's conclusion, admitted that what Flamsteed said "seems to be deduced clearly enough from [the] diagram and its explanation".⁴⁰ Jacques Cassini seems to have been the only person to seize on the significant sentence in Flamsteed's letter—where he says that if the latitude of the star is less in June than in December, then the same can be said of its declination.⁴¹ Cassini's own arguments against Flamsteed were rather confused, however, so it is not very surprising that they were not immediately accepted. And although Flamsteed conceded that Cassini was right, the letter in which he made this admission was not published until about fifty years after it was written. It is not surprising, therefore, that even after Cassini's criticism was published some astronomers continued to accept that parallax had been detected.

Flamsteed's own initial conviction that he had detected parallax can possibly be explained by his wish to detect it. He had begun his search about twenty years before his letter to Wallis was published; and although when in 1695 he collected his data for the Pole Star together he was not actually looking for evidence of parallax, when he noticed there was an annual variation the excitement he felt must have been considerable: at last he had found what he thought was concrete evidence that the Earth moves. His alleged measurement of annual parallax for the Pole Star was a clear example of someone finding something he was determined to find.

It was not until 1728, at the end of Bradley's account of aberration,⁴² that for the first time the correct explanation of what it was that Flamsteed had observed was given:

The observations of Mr. Flamsteed of the different distances of the Pole Star from the pole at different times of the year, which were through mistake looked upon by some as a proof of the annual parallax of it . . . do not all exactly correspond with each other, yet from the whole Mr. Flamsteed concluded that the star was 35" 40" or 45" nearer the pole in December than in May or July: and according to my hypothesis it ought to appear 40" nearer in December than in June. The agreement therefore is greater than could reasonably be expected. . . .⁴³

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2. Jean Picard, "Voyage d'Uranibourg" (1680) in A. Auzout, *Lettres du M. Auzout sur les grandes lunettes* (Paris, 1736).
3. Cassini submitted an account of these observations to the *Académie des Sciences* in 1693 in a letter entitled "S'il est arrivé du changement dans la hauteur du Pole, ou dans la cours du Soleil", published in *Mémoires de l'Académie royale des Sciences depuis 1666 jusqu'à 1699*, x (Paris, 1730), 360-75.
4. Picard's and Cassini's observations have been analysed in detail in a paper by C. A. F. Peters: "Recherches sur la parallaxe des étoiles fixes" in *Mémoires de l'Académie impériale des Sciences de Saint-Petersbourg*, 6 série, tome v (St Petersburg, 1853), 1-180, p. 13.
5. John Wallis, *Opera mathematica*, iii (Oxford, 1699), 701-8.
6. For a detailed account of the early years of the observatory, see Eric G. Forbes, *Greenwich Observatory: Its origins and early history* (London, 1975).
7. An account of these methods is given in Eric G. Forbes, "The origins of the Greenwich Observatory", *Vistas in astronomy*, xx (1976), 39-50.

8. Letter from Flamsteed to Ward, first published in F. Baily, *An account of the Rev'd John Flamsteed* (hereafter cited as *Baily, Account*) (London, 1835), 118–23. Professor Forbes kindly let me see a transcript of this letter from the copy belonging to the Royal Greenwich Observatory (P.R.O. 42, f. 12^v–17^r).
9. According to Forbes, in *Greenwich Observatory*, 38, this method was similar to one used earlier by Johannes Hevelius.
10. Baily, *Account*, 120.
11. This is the interpretation of Flamsteed's letter to Ward put forward by Forbes in *Greenwich Observatory*, 38.
12. On Flamsteed's arc 5" would be approximately 50 microns, which he measured by means of an endless screw with micrometer head attached. For an account of the mural arc see Derek Howse, *Royal Observatory, Greenwich: The buildings and instruments* (London, 1975), 19–21.
13. See Forbes, *Greenwich Observatory*, 38.
14. Wallis, *Opera mathematica*, iii, 703 ("animadverti, non aliunde has provenire posse, quam ex Parallaxi").
15. Letter from Flamsteed to Caswell, 5 February 1697–8. Professor Forbes also kindly let me see his transcript of this letter from the copy belonging to the Royal Greenwich Observatory (P.R.O. 42, ff. 82^v, 83^r).
16. These letters are reproduced together with Flamsteed's replies in Baily, *Account*, 160–8.
17. Wallis, *Opera mathematica*, iii, 702.
18. The effect of precession is due to the motion of the Earth's axis, causing a movement of the fundamental reference point from which all stellar positions are measured. The value Flamsteed took for the apparent movement of the Pole Star because of precession is very close to the one accepted today. For an account of precession, and the formulae giving its effect on right ascension and declination, see W. M. Smart, *A textbook on spherical astronomy* (Cambridge, 1931), chap. X. All subsequent definitions are taken from Smart unless otherwise stated.
19. Wallis, *Opera mathematica*, iii, 705.
20. When the minimum distance is being measured, the star is between the vertical and the Pole, and as the effect of precession is such that the star appears to be moving towards the Pole, in this case the precession effect must be added. For the maximum distance the Pole is between the star and the vertical, so the precession effect must be subtracted.
21. This effect is due to a slight shift of the Earth's axis brought about mainly through the gravitational attraction of the Sun and Moon. For a detailed account see Smart, *op. cit.*, section 134.
22. William Whiston, *Praelectiones astronomicae* (Cambridge, 1707).
23. *Ibid.*, 33–39. Whiston uses Flamsteed's figure for the annual parallax of the Pole Star to estimate stellar distances by first calculating the maximum parallax possible (which would be for a star at the pole of the ecliptic) from the value for the Pole Star, and then using a simple trigonometric relationship between the diameter of the Earth's orbit and the distance he wishes to calculate.
24. David Gregory, *Astronomiae physicae et geometricae elementa* (Oxford, 1702), 274–6.
25. *Ibid.*, 276.
26. *Ibid.*, 275.
27. William Whiston, *Praelectiones physico-mathematicae* (Cambridge, 1710), 194–202.
28. Baily, *Account*, 208–9.
29. For Roemer's attempts to measure parallax, see K. M. Pedersen, "Roemer, Flamsteed and the search for a stellar parallax", in *Vistas in astronomy*, xx (1976), 165–9.
30. Letter from Roemer to Flamsteed, from the printed copy in P. N. Horrebow's *Operum mathematico-physicorum tomus tertius* (Copenhagen, 1741), 80–81. Professor Forbes kindly sent me a copy of his transcript and translation of this letter.
31. Jacques Cassini, "Reflexions sur une lettre de M. Flamsteed à M. Wallis touchant la parallaxe annuelle de l'étoile Polaire", in *Mémoires de l'Académie royale des Sciences pour 1699* (Paris, 1702), 177–83.
32. *Ibid.*, 183.
33. I should like to thank Dr Dennis Rawlins for the help he gave me in the preparation of the mathematical section of this paper.
34. Letter from Flamsteed to Sir Christopher Wren, 19 November 1702, reproduced in Stephen Wren, *Parentalia, or memoirs of the family of the Wrens* (London, 1750) (hereafter cited as Wren, *Parentalia*), 252.
35. Wren, *Parentalia*, 247–53.

36. *Ibid.*, 251.
37. *Ibid.*
38. Roemer's conjecture that the speed of light is very high but finite was based on his observations of the passage of the satellites of Jupiter across the visible disc of the planet, or behind it. He noted that the times predicted by tables for the disappearance or re-emergence of the satellites, or for transits of them across the planet, differed from the observed times by an amount depending on the position of Jupiter relative to the Earth. Roemer's explanation was that light travelled with a large, but finite velocity.
39. C. A. F. Peters, "Recherches sur la parallaxe des étoiles fixes" (ref. 4), 8-12.
40. Letter from Roemer to Flamsteed, 1701 (see ref. 30).
41. Flamsteed to Wallis in Wallis, *Opera mathematica*, iii, 706: "quod Angulus *SIE* (latitudo stellae Polaris ab Ecliptica in Mense Junio) minor est quam *SDE* (latitudo visa in Dec) & proinde ejusdam declinatio minor est, & distantia a Polo major, in Mense Junio, quam in alio quovis Anni Mense."
42. Bradley's own account of the discovery of aberration may be found in his "Account of a new discovered motion of the fix'd stars" in *Philosophical transactions*, xxxv (1727-8), 637-61.
43. *Ibid.*, 661.

APPENDIX 2: MICHELL'S PROBABILITY ARGUMENTS

In his paper, "On the probable parallax and magnitude of the fixed stars" (Phil Trans, lvii, 1767, p. 243) John Michell argues that

"the probability, that any one particular star should happen to be within a certain distance (as for example one degree) of any other given star, would be represented (according to the common way of computing chances) by a fraction, whose numerator would be to its denominator, as a circle of one degree radius, to a circle, whose radius is the diameter of the great circle (this last quantity being equal to the whole surface of the sphere) that is by the fraction $\frac{(60')^2}{(6875.5)^2}$ "

Michell did not explain how he obtained the figure 6875.5; however, the required probability p may be written thus:

$$p = \frac{\text{area of spherical cap of radius } 1^\circ}{\text{area of sphere}}$$

$$= \frac{2\pi R^2 (1 - \cos 1^\circ)}{4\pi R^2}$$

for a sphere of radius R

ie $p = \frac{1}{2} (1 - \cos 60')$

let $60' = y$ radians

for small values of y , $\cos y = 1 - \frac{y^2}{2!}$

and $p = \frac{y^2}{4}$

but $y = \left(\frac{\pi}{60 \times 180}\right) \times 60'$

$\therefore p = \frac{\pi^2}{4(60 \times 180)^2} \times (60')^2$

$= \frac{(60')^2}{(3875.5)^2}$

The argument is then extended; Michell points out that $(1 - p)$ represents the probability that a second star does not lie within 1° of a particular star. Furthermore,

supposing there are n stars, $(1 - p)^n$ is the probability that none of the n stars lies within 1° of the given star. Then, since any one star could be selected in the first place, the probability that no two stars lie within 1° of each other will be $[(1 - p)^n]^n$. Michell's next step is to replace the specific value of 1° by the more general value x' , in which case

$$p_1 = \frac{x^2}{(6875.5)^2}$$

and the probability that no two stars lie within x' of each other is $[(1 - p_1)^n]^n$

$$\text{ie } \left[\frac{(6875.5)^2 - x^2}{(6875.5)^2} \right] n^2$$

It is from this formula that Michell obtains his value of about 80:1 for the probability that the two components of β Capricorni should lie within so small a distance of each other as observation shows. In his calculations he assumed $x = 3\frac{1}{3}'$, and that there are about 230 stars of similar brightness to β Capricorni (therefore $n = 230$).

There is no need to investigate Michell's arithmetic here (the details may be seen on p. 247 of his paper). It should, however, be noted that despite the ingenuity and originality of Michell's arguments, the formula he derived is incorrect. Apart from his failure to clarify at any point whether he is dealing with n or $(n + 1)$ stars in total, in the last stage of his argument, where he changes the power of $(1 - p)$ from n to n^2 , he is in fact counting every star twice. For if the stars were labelled (assuming for now that there are n of them) $S_1, S_2, S_3, \dots S_n$, Michell computes the probability that S_1 lies within x' of S_2 and that S_2 lies within x' of S_1 ; and similarly for all the other stars. He should thus introduce a square root into his formula. The corrected formula would be

$$\left[\frac{(6875.5)^2 - x^2}{(6875.5)^2} \right]^{(n-1)^{n/2} \frac{n}{2}}$$

for the probability that no two stars out of n lie within x' of each other.

The use of this formula makes little difference to Michell's numerical answer, or, therefore, to his epoch-making conclusions. Nevertheless it is interesting that he should have reached such important conclusions from slightly imperfect arguments.

APPENDIX 3: THE PROBLEM OF BRINKLEY'S MEASUREMENTS

With hindsight it is clear that Brinkley did not detect parallax; the values he calculated were simply too big, as can be seen by comparing his results with the modern values given, for example, in Norton's Star Atlas (1). He was, nevertheless, a very careful observer, one who was aware of the controversial nature of his claims and one who was eager to ensure that the observations were reduced to the best of his ability. Yet, he was still convinced that he had detected parallax. The problem which emerges therefore is what was the effect Brinkley had observed?

This is not a new question, but it is as yet unsolved. During the 1850s two historians discussed the problem. Robert Grant could only conclude in 1852 in his History of physical astronomy that

"... the anomalous results obtained by [Brinkley] have not been satisfactorily accounted for". (2)

The following year C. A. F. Peters tried to evaluate Brinkley's work and also came to the conclusion that it would be difficult to establish what effect it was that Brinkley had observed, since so good an astronomer as Brinkley himself had been unable to do so (3). Peters did make one or two tentative suggestions: that the plumbline might have been too near the limb of the instrument, that the illumination of the scale was not uniform or that the temperature might not have been allowed for properly. But it is most unlikely that any of these would have produced results as consistent as those Brinkley gave.

Much more recently, the question of Brinkley's results was broached by Professor Forbes in his volume on the history of the Royal Greenwich Observatory. Forbes declares

"We can now with hindsight ascribe it to the so-called variation in latitude caused partially by the annual displacement of

water masses between the arctic and antarctic regions, by melting and freezing, producing an annual oscillation of the terrestrial poles; but this came to be recognised only towards the end of the nineteenth century from entirely independent considerations". (4)

Is this a satisfactory explanation? The effect of "variation in latitude" was discovered in the 1880s by the German astronomer K $\ddot{u}$ stner (5); then during the 1890s the American astronomer S. C. Chandler wrote a whole series of articles on the subject, analysing as many sets of observations as he could from Bradley's time onwards (6). In one of his papers Chandler remarked that in his next article

"Bradley's later observations at Greenwich, [would] be discussed, and also those of Brinkley ... [He hoped] to show that the hitherto unsolved enigma of Brinkley's singular results [found] its easy solution in the fact of the polar motion". (7)

However, Chandler never did prove that Brinkley's results were caused by variation in latitude, one reason for this being that it is very unlikely that they were! The effect is periodic but it has a period of approximately 14 months, as Chandler showed, and the variation Brinkley believed he had detected certainly had an annual period. Also, Brinkley's results cannot be explained by any physical phenomenon affecting the position of the Earth's axis, and thereby the fundamental reference point from which all stellar positions are measured, because such a phenomenon would result in all stars in similar parts of the sky being affected similarly. This was plainly not the case with Brinkley's observations. Thus what caused them, or whether or not they were illusory, unfortunately remains a mystery.

Notes to Appendix 3

1. A. P. Norton and J. G. Ingliss, Norton's star atlas, 15th. edition (Edinburgh, 1966) 53; for example, for the stars Altair, Arcturus, Deneb, Norton gives 0".198, 0".090, 0".013 for their parallaxes whereas Brinkley gave 5".5, 2".2 and 2".1 respectively, in 1815.
2. R. Grant, A history of physical astronomy (London, 1852) 550.
3. C. A. F. Peters, "Recherches sur la parallaxe des étoiles fixes", Mémoires de l'Académie impériale des Sciences de St. Petersburg, 6 série, v (St. Petersburg, 1853) 35: "Neamoins nous savons maintenant que cette conviction de Brinkley était illusoire et qu'il faut attribuer les variations qu'il avait observées, à d'autres causes ... Il sera toujours difficile d'indiquer ces causes exactement, vu qu'un astronome aussi distingué que Brinkley n'avait pu parvenir à les découvrir ... "
4. E. G. Forbes, Greenwich observatory: its origins and early history (London, 1975) 162.
5. See A. Clerke, History of astronomy during the nineteenth century, 3rd. edition (London, 1893) 318.
6. These papers were published at a rate of two or three a year in the pages of The astronomical journal, xi - xviii (1891 - 7).
7. S. C. Chandler, "On the variation of latitude", ibid., xi (1891) 86.

APPENDIX 4

To derive the formula for the sum of the parallaxes in right ascension of two stars lying 12 hours apart.

From W. M. Smart, A textbook in Spherical Astronomy, 6th edition (Cambridge, 1977) 221, the formula for parallax in right ascension is:

$$\Delta\alpha = \frac{\pi}{\cos\delta} (\sin\odot \cos\varepsilon \cos\alpha - \cos\odot \sin\alpha)$$

where π = parallax of star

$\odot$ = longitude of the Sun at the time of observation

ε = obliquity of the ecliptic

(α, δ) = equatorial coordinates of the star

If the two stars under consideration have coordinates (α_1, δ_1) , (α_2, δ_2) and if we set $\alpha_2 = \alpha_1 + 180^\circ$ (so that $\cos \alpha_2 = -\cos \alpha_1$, and similarly $\sin \alpha_2 = -\sin \alpha_1$) then the parallax for each in right ascension is:

$$\Delta\alpha_1 = \frac{\pi_1}{\cos\delta_1} (\sin\odot \cos\varepsilon \cos\alpha_1 - \cos\odot \sin\alpha_1)$$

$$\Delta\alpha_2 = \frac{\pi_2}{\cos\delta_2} (-\sin\odot \cos\varepsilon \cos\alpha_1 + \cos\odot \sin\alpha_1)$$

Each of these corrections is added to the 'true' right ascension to give the observed right ascension, i.e.

$$\alpha_1 (\text{observed}) = \alpha_1 + \Delta\alpha_1$$

$$\alpha_2 (\text{observed}) = \alpha_2 + \Delta\alpha_2 = \alpha_1 + 180 + \Delta\alpha_2$$

By subtracting the two observed values, therefore, we get:

$$180^\circ + \Delta\alpha_1 - \Delta\alpha_2 = (\sin\odot \cos\varepsilon \cos\alpha_1 - \cos\odot \sin\alpha_1) \left(\frac{\pi_1}{\cos\delta_1} + \frac{\pi_2}{\cos\delta_2} \right),$$

which is 180° plus the sum of the two parallactic shifts.