# Astronomy and Patronage in Hanoverian England: The Work of James Bradley, Third Astronomer Royal of England.

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# Dedication

**This thesis is dedicated to my wife**

**Deborah Joy Fisher**

**and to the**

**memory of my parents**

**Joseph Peter Fisher (1911 – 1948)**
**and**
**Lilian Irene Bonell (1912 – 1988)**

**my brother**

**Peter Donald Fisher (1947 – 1976)**

**and**

**my stepfather**

**Raymond Thomas Bonell (1924 – 2002)**

**Astronomy and Patronage in Hanoverian England.**
**The Work of James Bradley, Third Astronomer Royal of England.**

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# Astronomy and Patronage in Hanoverian England.
# The Work of James Bradley, Third Astronomer Royal of England.

## Abstract.

James Bradley, the third Astronomer Royal of England, has long been acknowledged as an outstanding observational astronomer. After the publication of the *Fundamenta Astronomiæ* by F. W. Bessel in 1818, and the *Miscellaneous Works and Correspondence of James Bradley,* together with the *Memoirs of Bradley* by S. P. Rigaud in 1832, he was admired as an exemplar of the traditions of rigour, precision and accuracy which were valued within the contexts of 19th century astrometry. His work had largely been forgotten before Bessel reduced the great series of observations made by Bradley and his assistants at Greenwich from 1750 to 1762. He was commonly acknowledged as "the discoverer of stellar aberration and the nutation of the Earth's axis" in accordance with the acceptance that both were uncontentious empirical phenomena.

This thesis seeks to challenge the well established view that stellar aberration was an empirical "discovery". By analysing Bradley's work on the "new discovered motion", and the construction of his paper on the hypothesis, contrasting it with the work of the contemporary Italian astronomer Eustachio Manfredi's on "aberrations", it is possible to demonstrate that the aberration of light is a concept. This conclusion is emphasized in the recognition that aberration is a construct based on the working practice of an astronomer who worked within a milieu associated with the work of Isaac Newton and his followers. Bradley was trained by two notable Newtonian practitioners, James Pound, who was responsible for Bradley's induction into the tradition of accurate and precise observation and Edmond Halley, who introduced him to the mathematical and analytical skills of the Newtonian school. Bradley is presented as an exemplar of the Newtonian approach to astronomy and experimental philosophy. Bradley's commitment to Newtonian principles is demonstrated by an examination of his lectures in experimental philosophy at Oxford.

Bradley was born into impoverished gentry. In an age in which connections determined social advancement his entry into the elites of British science and society was due to the connections formed by his maternal uncle James Pound, who had formed powerful Whig connections, and was also a member of Newton's circle. Bradley's election as Savilian Professor of Astronomy at Oxford depended on Pound's Whig connections. Later, Bradley diversified his social and political connections and soon acquired the admiration of men of diverse political and social outlooks. His appointment as the third Astronomer Royal was dependent on these diverse connections. His reform of the Royal Observatory was facilitated through the patronage of Henry Pelham the Prime Minister and the personal approbation of George II. Bradley's ambitious career was greatly advanced by his close identification with many of the economic and political objectives and values of the ruling elites of Hanoverian society.

# Astronomy and Patronage in Hanoverian England.
# The Work of James Bradley, Third Astronomer Royal of England.

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# Astronomy and Patronage in Hanoverian England. The Work of James Bradley, Third Astronomer Royal of England.

## Chapter One.

## James Bradley, Newtonian Astronomer.

### Prologue.

In January 1729 James Bradley read his paper on a new discovered motion of the "fixed" stars at two meetings of the Royal Society.[1] The phenomenon he described, which had been isolated over three years of almost continuous observation at Kew and Wanstead, was eventually to be termed the aberration of light. It was widely accepted as the hitherto firmest observational evidence of the motion of the Earth.[2] In spite of Bradley's express qualifications, this modest, unassuming, yet ambitious man was the first to apparently validate the Copernican thesis observationally.[3] This alone should have made his name well known to posterity. Bradley remains a figure wrapped in obscurity, little known outside a specialist knowledge of the history of astronomy. In this thesis I will argue that this historical obscurity was in part a product of a reconstruction of Bradley's achievements during the early decades of the 19th century. Lacking any real appreciation of much of the contextual bases of Bradley's work, he was interpreted as a consummate technician, an observer of unusual ability, accuracy and precision. Much of the theoretical basis of Bradley's work on the aberration of light and terrestrial nutation was forgotten. Throughout the 19th and 20th centuries the historiography of science was largely a study of the development of scientific *theory*, or at least a study of scientific ideas. Bradley's work, interpreted "merely" as that of a practical observer, was in consequence ignored. He had his champions, including Friedrich Wilhelm Bessel, Stephen Peter Rigaud and Sir John Herschel, but the work leading to the paper on the aberration of light was sometimes interpreted as the report of a fortuitous accident, stumbled on by Bradley and his partners whilst they sought to measure the annual parallax of $\gamma$ Draconis, a task they failed in accomplishing.

This effective devaluation of Bradley's achievement was underlined by the 19th century response to much of his scientific work. First was his notable work on the nutation of the Earth's axis which was interpreted as a simple empirical discovery.[4] It was commonly interpreted by 19th century writers as a confirmation of Newtonian theory, but whilst Newton touched upon the subject he only referred to it in passing.[5] The publication of Bradley's long-awaited paper on the nutation of the Earth's axis in 1748[6] was viewed by many of his scientific contemporaries as a final vindication of Newtonian theory during a period when the previous conflict between the upholders of the rival Newtonian and Cartesian systems was moving to a resolution.[7] From 1737, when Bradley had first published his work on the phenomenon of nutation in a letter addressed to Maupertuis in Paris,[8] it was rapidly accepted by natural philosophers in England and France. Many of his scientific contemporaries recognised not only the immense theoretical significance of the work, but also the practical difficulties of observing such an infinitesimal phenomenon with such precision and accuracy for such a long period.[9] There was little doubt as to its worth. Bradley was awarded the Copley Medal of the Royal Society in 1748, the year of the publication of his account.

Under the stewardship of Edmond Halley, particularly after the early 1730s, the Royal Observatory began to decline as a centre of reliable and accurate observation. This was in part due to the infirmity and illness of the 2nd Astronomer Royal and the gradual decline of his observational abilities.[10] In addition, Halley had been unable to obtain sufficient funds to improve the condition of the observatory. Although Halley was often assisted by Bradley and John Machin, the decay of the observatory's instruments, including George Graham's 8 foot quadrant, went apace with Halley's growing infirmity. It was Bradley's primary task on becoming the 3rd Astronomer Royal in 1742 to place the Royal Observatory on a sound footing. When he inherited "the finest instrument in England"[11] Bradley found the observatory and its instruments in a pitiful state. Although Bradley had on occasion, sometimes in partnership with

Graham and Machin, attempted to rectify the instruments on behalf of Halley,[12] most of them were unusable when he took over the Royal Observatory.

Such was Bradley's drive and ambition that he had, in just 8 years, transformed it into the best equipped observatory in Europe. He constructed a new transit house, and fitted it with the most accurate instruments available, employing some of the finest instrument makers in London. His persistent training of "professional"[13] assistants ensured that observations were maintained continuously to levels of standardisation and precision that emulated his own. Bradley's assiduous attention to detail, a characteristic of his approach to all of his astronomical and experimental work, became the benchmark against which all future positional astronomy, not just at Greenwich, but at all of the leading astronomical institutions in Europe would be measured. Several national observatories commissioned Bradley's own instrument makers, including John Bird, to construct equally accurate instruments for their own programmes of observation.[14]

Arguably, Bradley's greatest achievement was the continuous series of observations undertaken at the Royal Greenwich Observatory between 1750 and 1762 by himself and his assistants. Bradley was a remarkable observer, who was notable for the rigorous way he constantly rectified his instruments, always seeking out any possible sources of systemic error, arguably the first observer who applied such a rigorous practice in England. One of his most far reaching contributions to the development of the astronomical sciences during the 18th century was the way he trained a number of highly competent assistants, one of whom, Nathaniel Bliss, was to succeed him as the 4th Astronomer Royal.[15] The reduction of these observations by F. W. Bessel in 1818[16] was an important juncture in the process in which Bradley was to be "reconstructed" as a consummate observer, without any reference to the contexts within which he made his observations. Bessel was highly impressed by the accuracy and the precision of the Greenwich observations, but even more he was moved by the manner in which Bradley had consistently recorded every possible factor which might impinge upon the accuracy of the recorded data, including atmospheric

temperature and barometric pressure. The work of his assistants also reflected this practice, though it was whilst reducing their observations in contrast to those made by Bradley that Bessel conceptualised the "personal equation", when he noted differences in measured repeated transit times between the various observers.[17] This catalogue of some 3,222 stellar positions of quite unprecedented precision and accuracy remains the earliest stellar database which can be used confidently by succeeding generations of astronomers.

Bradley's career was largely shaped within the mutual obligations of a patronage he assiduously pursued. Although he arose from impoverished gentry, his education being directed to obtaining a living in the church, largely under the direction of his maternal uncle James Pound, he was soon introduced through his uncle's connections to some of the leading lights of English science including Isaac Newton and Edmond Halley, and equally importantly to many important figures in the Whig ascendancy, a source of patronage for both Pound and Bradley. By the time of his maturity as an astronomer, when he was simultaneously Savilian Professor of Astronomy[18] and Reader in Experimental Philosophy[19] at Oxford and the Astronomer Royal at Greenwich,[20] the "discoverer" of annual aberration and terrestrial nutation had created a distinctive and respected role for himself in Hanoverian society. As a gentleman who epitomised many ideals of gentility and fortitude Bradley's sociability and easy conviviality allowed him to acquire and utilise patronage, fashioning himself as an exemplar of virtue. In Bradley's work and the role he created, the highest moral propensities of the age are exemplified in a virtuous life dedicated to the pursuit of precision, accuracy and rigour, epitomised by his immense patience, determination, industriousness and rectitude. He was a man widely admired not merely for his achievements and for the utility of his work, but for the generosity of his character.

**Justification.**

Much recent historiography of science has emphasised the value and importance of *practice*. The earlier emphasis on theory, and the commonly

attendant association with "internalist" accounts of the history of science, an approach that has tended to discount the significance of many important workers, has now become increasingly outmoded. In the study of scientific practice, a more contextual approach to the historiography of science has been encouraged. It is an approach which allows the historian to examine many aspects of the work of a man like Bradley in the context of the practice of astronomy in early Hanoverian England. As an astronomer Bradley was trained within the context of the methods of Newton's school. He was personally acquainted with Isaac Newton, who held a significantly high opinion of his abilities. His association with Newtonians as diverse as Halley and Maupertuis, and the development of so much of his scientific practice in terms of an explicit expression of the methods of this school, makes a study of the work of Bradley useful in any appraisal of the development of Newtonian astronomy in early and mid-18$^{th}$ century England.

Bradley transformed the practice of astronomy and astrometry at the Royal Greenwich Observatory, and the influence of his work on the continent, particularly in France was marked. It is not merely the acuity of his mind and the competence of his practice which is so obvious in any study of Bradley's work; it is the intellectual and industrial energy of the man that impresses. This boundless energy is obvious, not merely in his astronomical work, but also in his academic life. It is difficult to make many generalisations about the vitality of intellectual life at the English universities in the 18$^{th}$ century,[21] but in terms of reported matriculations it appears to have been in decline for much of the century.[22] The rise of alternative centres of academic excellence in the non-conformist academies casts a revealing light on the comparative poverty of scientific endeavour at Oxford and Cambridge. It is studies such as Graham Midgley's of Oxford University in the 18$^{th}$ century[23] which add substance to John Cannon's appraisal of university matriculations.[24] Yet there are exceptions to this overall impression, and the contribution of Bradley to the academic and scientific life of Oxford was considerable. The leisured routines of college and university life described by Midgley must have contrasted considerably with the industry displayed by Bradley in his mature Oxford career as the Savilian Professor of Astronomy, and

as Lecturer and later, Reader in Experimental Philosophy. His regular courses in experimental philosophy, given three, and later two times a year, for over thirty years at the Ashmolean Museum,[25] were well attended and influential.

The recent developments in the historiography of science which have tended to emphasise the study of the sciences in context, have begun to undermine the more rigid epistemic distinctions made between theory and practice, emphasising the importance of various forms of tacit knowledge and the working traditions through which many such non-reflective practices are made concrete. These shifts in historiographical practice make the reappraisal of many scientific figures a historical requirement. Bradley has been poorly served in many accounts of his work, even by those who may ostensibly have been his admirers. In most cases Bradley's work was interpreted from the viewpoint of later working traditions and practices, in which the original motives and contexts have been lost. Some early 19th century astronomers such as F. W. Bessel[26] and Stephen Rigaud,[27] who rediscovered the precision and accuracy of Bradley's work, were both ardent admirers of many of his achievements. Without their research on Bradley's earlier work much of importance may have been lost, as his papers were gradually dispersed. Later historians owe both men an inestimable debt of gratitude, for without their work it is highly unlikely that much of the material now available would have survived. Yet invaluable though their work was, they helped to lay the foundations of a tradition that served Bradley ill.

Both Bessel and Rigaud were working astronomers, and both emphasised the exactitude of Bradley's practice without always relating it to the contexts within which this practice developed. Rigaud was a scholar with an excellent library of seventeenth and eighteenth century scientific authors[28] whose opinion of Newton's theoretical abilities was highly laudatory. Although he indicated Bradley's philosophical insights when he worked on his papers, he emphasised Bradley's abilities as an observational astronomer, portraying him as a Newtonian acolyte. He displayed a tendency to underestimate the importance of the heuristic contexts within which Bradley's techniques developed. At the time that Bradley was "re-discovered" a debate was developing, led by various members of the

Astronomical Society. The controversy between Flamsteed and Newton was revived following the publication in 1835 of Francis Baily's *An Account of the Rev. John Flamsteed, the First Astronomer Royal,*[29] in which the debate was introduced as "the place of the practical observer and philosopher in the manufacture of knowledge".[30] On the one hand figures such as Francis Baily and Charles Babbage, and to a lesser extent, John Herschel, "viewed intellectual production as akin to material production".[31] Against this, David Brewster and William Whewell extolled the role of genius in scientific discovery, a view that was also supported by Stephen Rigaud, who also entered into the increasingly vociferous argument. As William Ashworth writes:

> For Baily and Babbage genius was ultimately a redundant concept within the workings of intelligence and a mechanical understanding of the world, while for Whewell and Brewster it was an intrinsic component of great scientific discovery.[32]

Brewster himself wrote that "a collection of scientific facts are of themselves incapable of leading to discovery…unless they contain the predominating fact or relation in which the discovery mainly resides."[33] In spite of Brewster's contention, this debate surely tended to overemphasise the distinctions made between theory and practice. In the context of this argument over the relative value of theoretical insight and practical competence it was almost inevitable that Bradley was overtly perceived as a practical astronomer. It was this interpretation that was clearly signalled when the first "anomalous" observation of the "new discovered motion"[34] was presented as the moment of "discovery".[35]

This interpretation in itself did not detract from Bradley's abilities, but in the hands of later authorities, who were further removed from the contexts of Bradley's astronomical achievements; it led to a radical misunderstanding of his contributions. Although some astronomers remained aware of some of the major contributions Bradley had made to the science, these were all too often forgotten. An appraisal of some of the literature concerning the history of astronomy, leads to the impression that where Bradley is referred to at all, it is as an observer. Some authorities, such as Arthur Berry,[36] do appear to be aware of the theoretical

basis of some of this observational work, but others, such as Agnes Clerke, whilst sometimes referring to Bradley in lavish terms appear to overlook this entirely.[37] Clerke's description of Bradley as "the founder of modern observational astronomy" is difficult to accept, for the science of observational astronomy has gone through so many technical and methodological transformations over the past few centuries, that such an epithet is meaningless. To describe Bradley as an "observational astronomer" fails to come to terms with the real core of his achievement.

Bradley was the Newtonian astronomer *par excellence*. To evaluate his work simply in terms of observation, even though he was in any sense of the term a quite extraordinary observer, fails to appreciate the complex interaction of tacit knowledge, theory and practice that makes his work so interesting. In addition, Bradley's work was conducted against the background of changing alliances of interest and patronage which not only imposed itself on his work, but made the publication of some of it an exercise fraught with political consequence.[38] This interplay of astronomical achievement and political interest will be discussed in the thesis, particularly in the final chapter. It will reveal that Bradley, as well as being an outstanding astronomer and natural philosopher, was also an ambitious and consummate player of patronage. In addition, almost every observation made by Bradley throughout his astronomical career was made within the expectations of some hypothetical or theoretical construct. Where anomalies were exposed that did not fit these presumptions, some rational construct was urgently sought or proposed. Almost the entire length of his career was made within a theoretical or practical ethos that may be termed or described as "Newtonian". The meaning of this term will be discussed and qualified, for it was often acquired and used by various interests throughout much of the 18$^{th}$ century to empower claims for the support of philosophical procedures or corpuses of knowledge which Newton would not have recognised.[39]

Although I intend to show that Bradley's work is an exposition of many tacit and implicit connections between theory and practice, his career also reveals the subtle interplay between scientific practice and the acquisition of scientific,

political and social patronage. These connections became specific when Bradley's different attempts to write his account on the "new discovered motion" were made during December 1728. I will show that Bradley was seeking a perilous course between Scylla and Charybdis. The result is a paper that describes a scientific "discovery" which is equally an appeal to political interest. As a gentleman Bradley was expected to honour the name and memory of his late friend and partner Samuel Molyneux, even though he recognised that his erstwhile friend's contribution was directed against the maintenance of his own interest. Molyneux quite clearly betrayed Bradley's own trust when he had made a premature approach to an elderly and infirm Newton, during an early stage of their joint investigation, to needlessly assert that he and Bradley had discovered a phenomenon which in John Conduitt's recall of Molyneux's words, "destroyed entirely the Newtonian system".[40]

Samuel Molyneux was a man of wealth[41] and connection who had no need to concern himself of other's opinions. Bradley, by contrast, was a gentleman of limited means who was overtly dependent on the support and patronage of those who possessed power and influence. His career, both initially in the church, and later as an astronomer, was constructed on the assertive and ambitious acquisition of patronage. To be associated with a partner who challenged the validity of Newton's work, may have been perceived by Bradley to put his hopes for further advancement within a scientific milieu that was dominated by Newtonian conventions and the power relations constructed by Newton's political and philosophical allies in jeopardy. Given that his mentor was Edmond Halley this fear may have been overblown, but Bradley was in a vulnerable social and financial position and anything that may have caused him discomfiture may have been magnified in his own mind. Bradley's account of his work on the new discovered motion, the phenomenon later to be termed the aberration of light, was therefore constructed in a mindful acceptance of the contributions made by his late friend, yet careful to avoid any association with any possible imputed "anti-Newtonianism". With a mentor like Halley, and the recently voiced approbation of Newton, there was little overt danger of Bradley being associated with

Molyneux's premature approach.[42] Bradley constructed an account that paid every compliment to the work of his partner, whilst distancing himself from any association with any possibility of an implied assault on the validity of Newtonian theory. This was clearly demonstrated by the nature of the work he conducted at Wanstead, which was in every sense of the term an implied demonstration of Newtonian principles and procedures.[43]

Bradley was a man who indicated his philosophical and political commitments, both to the Newtonian milieu within which his practice developed, and to the Whig ascendancy which he recognised formed the basis of his future social advancement. This is obvious in his short church career of less than three years.[44] In his ordination as a priest by Benjamin Hoadly, one of the most controversial churchmen of his age, and his acceptance of the post as Hoadly's private secretary, Bradley clearly determined that his future advancement would be greatly enhanced by a close commitment to and an association with the Whigs. To the new Hanoverian dynasty Tory ambivalence over the Act of Settlement of June 1701[45] made them suspect, and they were often regarded, justly or otherwise, as sympathisers of the Jacobite cause. By contrast, Whig acceptance of the Act of Settlement made them almost unassailable, and almost anyone with ambitions of advancement, especially after the failure of the Jacobite rebellion of 1715, attached themselves to the Whig cause. Yet. in this commitment Bradley was probably expressing his real sympathies. His uncle, James Pound, moved largely within Whig circles, and could also claim the acquaintance and support of William Wake[46]who was also to advance Bradley's career at Oxford.[47] Hoadly and his family were to remain firm friends of Bradley for many years.

Bradley moved in two quite different though related circles of influence. Politically he was largely dependent on Whig patronage, although he was later to draw on the admiration and friendship of those with quite definite Tory sympathies.[48] Many of his astronomical and natural philosophical colleagues belonged to a group which can be regarded as members of a circle closely associated with Newton. It should not be assumed that these "Newtonians" were necessarily all in sympathy with the Whigs, but in practical or pragmatic terms

most of them moved in such Whig circles. In the period of the Whig ascendancy these connections should not be too surprising, but there is plenty of evidence to show that Bradley was definitely associated by choice with some Whig figures of influence. As this thesis will show, it was the power of his political connections that advanced his astronomical career and without this patronage Bradley would in all probability have remained a churchman, and given the financial insecurity of his family may not have reached very high office. The most influential person in Bradley's life was his maternal uncle, James Pound, who not only took him under his wing in his preparation for a career in the church, but introduced him to the science of astronomy, and to sources of high patronage at the start of his career.

Bradley was an outstanding astronomer, but as I hope to show, the advancement of his career was the product also of his undoubted social and political skills. Both Pound and Bradley acquired reputations of conviviality and generosity, and whilst according to Rigaud Bradley was "abstemious",[49] there is no evidence to support the view that he was in any sense censorious. He was a man who acquired many friends and maintained friendships with people of many different backgrounds. Henry Pelham[50] later valued his friendship, and this was to stand him in good stead, not only when the post of Astronomer Royal became vacant in 1742, but also when Bradley needed to re-equip and reform the Royal Observatory in 1749 and 1750. The picture of "Bradley" that was constructed during the early decades of the 19th century was a cipher. Given the comparative lack of knowledge about Bradley, posterity should be grateful to Stephen Rigaud for the work he undertook. He not only arrested the dispersal of Bradley's many papers, saving them for later generations of historians, he also recorded a still extant oral history from surviving members of his family and from the descendents of some of Bradley's friends. Not too much credence should be placed on such oral sources. Much was mere hearsay, as undoubtedly was Thomas Thomson's account of Bradley's so called "discovery" of the aberration of light in 1808.[51] Thomson wrote:

> The boat in which they were was provided with a mast, which had a vane at the top of it. It blew a moderate wind, and the party sailed up and down the river for a considerable time. Dr. Bradley remarked that every time the boat put about, the vane at the top of the boat's mast shifted a little, as if there had been a slight change in the direction of the wind. He observed this three or four times without speaking; at last he mentioned it to the sailors, and expressed his surprise that the wind should shift as regularly every time they put about. The sailors told him that the wind had not shifted, but that the apparent change was owing to the change in the direction of the boat, and assured him that the same thing invariably happened in all cases. This *accidental observation* (my italics) led him to conclude, that the phenomenon which had puzzled him so much was owing to the combined motion of light and the earth.[52]

The 19th century historiographical construction of the work of Bradley continues to cloud the significance of his contributions to the astronomical sciences. There is an urgent necessity to return to the primary sources, in order to reconstruct the "Bradley" of the 18th century which these sources reveal. This may also throw light on the development of astronomy in Hanoverian England and the social and scientific interactions between astronomers in England and France. It is a primary task of this thesis to expose as much as possible about the social, political, philosophical and scientific contexts of the work of James Bradley.

**A Historiography of Bradley's Work.**

There is barely a secondary literature worth mentioning. Bradley was arguably the leading observational astronomer of his age, not just in England, but in Europe.[53] In terms of 18th century British astronomy far greater attention has been paid to the work of William Herschel. The work of Bradley may seem alien to the concerns of modern astronomy, and this may be regarded as a primary reason for his obscurity. On the other hand Bradley has long retained admirers. Ever since F. W. Bessel reduced the observations of Bradley and his assistants at Greenwich from 1750 to 1762 to epoch 1755,[54] his work as an observational

astronomer has been regarded as little short of awe inspiring.[55] In a recent article Freeman J. Dyson, a leading physicist of the late 20th century, described Bradley as "the founder of modern science".[56] This is an over-statement, for unarguably much of the significance of the work of Bradley lay in the way he demonstrated the rigour, accuracy and precision that was central to *Newtonian* "methodology". Bradley was arguably one of the most notable practitioners of Newtonian philosophy in the exact sciences in the first decades after the death of Newton. Dyson's claim that Bradley was the first scientist to make measurements of high precision, that he set the standards of astronomical observation throughout Europe, is simply unsustainable. I do not wish to contend Dyson's estimation of Bradley's concern with extreme accuracy and precision, for I support this view, but I must, however, take issue with his failure to recognise that Bradley was working within a context, a milieu in which such precision was inculcated as an essential core of his heuristic and his practice as an astronomer.[57]

In the decades between Bradley's death in 1762, and the discovery of the measure of his achievement during the first decades of the 19th century, his work was forgotten. In the reconstitution of this achievement by Bessel[58] and Rigaud[59] much of the context of Bradley's work was lost. In the absence of a context the secondary literature witnesses the development of a tradition, a very persistent tradition, which was unwittingly supported by Dyson. Bradley out of context is overtly perceived as an extraordinary observer of great accuracy and precision. In the absence of a context, and the creation of a tradition about Bradley the observer, there is very little that can be asserted other than he was a precise and accurate astronomer. General or wide interest is difficult to sustain in anything so arcane. I hope to show that a study of Bradley is an evaluation of the practice of the astronomical sciences in the development of Newtonian methods, and an exploration of the role of patronage in the advancement of mutually perceived interests. The advancement of Newtonian philosophy and the management of Whig patronage are both illustrative of the use of power. By whatever means at his disposal, Bradley's career is a demonstration of the manipulation of both.

The earliest reference to Bradley during the 19th century was by Thomas Thomson.[60] In Thomson's account, Bradley's "discovery"[61] of the aberration of light is reduced to a "leap of the imagination". Rigaud included it in the *Memoirs of Bradley*,[62] even though the "event" is not referred to in Bradley's own account.[63] An analysis of Bradley's account reveals that the concept of the aberration of light was created within the contexts of his working practice. Arthur Berry appeared to accept the "standard account" of Bradley as "observer".[64] He seems to use Rigaud as his main source, but he does take issue with the inclusion of Thomson's account in the *Memoirs*.[65] It is to Berry's credit that he challenges Thomson's story. He failed to find any documentary evidence to support an account written 80 years after the events described. It may have been that such a source existed, for some papers were lost between 1776 when Bradley's archive was presented to the University of Oxford and 1829 when Rigaud gathered many of Bradley's papers prior to writing his *Memoirs*.[66] However, Bradley appears to contradict the account in the paper on the "new-discovered motion". The point at issue is really whether the aberration of light can be understood as an empirical discovery, or whether it must be understood as a construction, rooted in a heuristic which also utilised the presumptions accepted by many contemporary astronomers. It was because it was rooted in these common presumptions and practices, that it was almost immediately accepted as firm evidence for the motion of the Earth around the Sun.[67]

Bradley possessed no greater admirer than Rigaud, so it is surprising that he included Thomson's questionable passage in his biographical account of Bradley's life.[68] Robert Grant's *History of Physical Astronomy from the Earliest Ages to the Middle of the Nineteenth Century*[69] relied heavily on Rigaud's *Memoirs*.[70] Undoubtedly, Grant brought a knowledge of Bradley to a wider public but he maintained a now established view that he was a precise and accurate observer and little more. He evidently read the published versions of the papers on aberration and nutation, for in connection with the latter he referred to Bradley's conjecture about the motion of the solar system relative to the stars. When

discussing variations apprehended during his observation of nutation Bradley wrote that

> if our own solar system be conceived to change its place with respect to absolute space, this might, in process of time, occasion an apparent change in the angular distances of the fixed stars, and in such a case, the places of the nearest stars being more affected than of those that are very remote, their relative positions might seem to alter, though the stars themselves were really immoveable.[71]

It is precisely in these areas, where Bradley makes his most perceptive cosmological conjectures,[72] and where he develops the theory and practice of astrometry in relation to nutation, precession, proper motion, refraction and annual aberration, that a wider knowledge of his contribution is most lacking.[73]

The emphasis on Bradley's practical and observational skills blinds even his admirers to his conjectural flair and his mastery of Newtonian theory. Bradley's emphasis on rigour, accuracy and precision only makes sense if it is perceived in terms of his practice, which is firmly constructed on Newtonian principles. To agree with Dyson, and to assert that Bradley was the founder of modern science, is to fail to recognise that Bradley's work was based on Newtonian practice, which was legitimised by the expectations and ambitions of a philosophical and social elite. It was Newton who promulgated the approaches to astronomy and natural philosophy practised by Bradley. This does not diminish his worth, for in a very real sense the Newtonian spirit becomes flesh in the work of Bradley. Dyson is the inheritor of an interpretation of Bradley that was most succinctly expressed by Agnes Clerke in her article, *James Bradley.*[74] Here Bradley is cited as "the founder of modern observational astronomy". In the age of the photographic plate and the spectroscope the contributions of an astronomer like Bradley were remote and it is was as an "observational" astronomer that he was celebrated. In his *History of Astronomy,*[75] Anton Pannekoek includes the work of Bradley in a chapter headed *Refined Practice*.[76] This was contrasted with the following chapter which dealt with theoretical astronomy during the 18th century. It underlined the view of Bradley as an "observational" astronomer, even

though his practice was constructed out of various elements that integrated the theoretical and the practical imperatives of Newtonian philosophy. Pannekoek evidently held a high opinion of the achievements of Bradley, but in creating a distinction between practical and theoretical astronomy he failed to reveal the dynamic that underlay so much of Bradley's work.

One year after Pannekoek's *History of Astronomy* appeared in English, the Royal Astronomical Society celebrated the bi-centenary of the death of Bradley at a meeting of the society. The President of the Royal Astronomical Society, W. H. McCrea, evidently held a high opinion of Bradley, yet McCrea perceived of Bradley as an "observational" astronomer, and the aberration of light as an empirical discovery and not a conceptual construct. McCrea presented a paper entitled, "The Significance of the Discovery of Aberration"[77], in which he argued that the full significance of the aberration of light was only evident with the development of Albert Einstein's special theory of relativity. There appeared to be no appreciation that the aberration of light was a theoretical construct, created in a different context or paradigm from that of special relativity. In its 18th century context the aberration of light was an advance in the development of a precise science, constructed on Newtonian principles, involving the manipulation of various presumptions commonly held. Having failed to grasp that the phenomenon was a theoretical construct and asserted that its significance was only apparent in the context of the development of relativity theory, McCrea effectually trivialised Bradley's achievement. The construction of aberration theory was a consummate application of Newtonian principles.[78]

**Objectives.**

In the prologue I set out a few of Bradley's achievements, including the work on the aberration of light, the nutation, and his mature work at the Royal Observatory. In the justification I set out the need for a reconstruction of Bradley's achievements during his work as an astronomer and a teacher. The brief historiographical analysis not only reveals the paucity of material, but also the lack of any appreciation of Bradley's heuristic. Almost no work of any

significance has ever been undertaken on the influential role played by Bradley in the practice and the dissemination of Newtonian natural and experimental philosophy.

When Bradley died in 1762 his executors entered into a protracted legal dispute with the Board of Admiralty over the custody of many of his papers, including the great series of observations made between 1750 and 1762 at the Royal Observatory. It is evident that his personal papers were retained by his family before the bulk of his papers were presented to Lord North in 1776.[79] Early Hanoverian England was a society with relaxed social and sexual attitudes but it nevertheless retained propriety in polite circles.[80] Bradley's family possibly believed they had cause to keep some aspects of Bradley's private life out of the public sphere. This may explain why there is such a paucity of material concerning his personal affairs. Elizabeth Pound, his uncle's widow, was a wealthy woman[81] who was evidently affectionate towards her late husband's nephew.[82] After the death of Pound in 1724, Bradley remained living in her house until he moved his main domicile to Oxford in May 1732,[83] taking up the residence that came with his professorship. Elizabeth moved to Oxford with Bradley and remained living with him until her final illness began in 1737.[84] Bradley was forbidden by Georgian family law from marrying his uncle's widow, but an indication of the closeness of their relationship may be indicated by her will, for she left a major part of her considerable fortune to him. He only married after Elizabeth died, to a widow, Susannah Peach, in 1744.[85] That Bradley lived for so long with Elizabeth Pound and only married after she died may be a coincidence, but the closeness of his relationship with the whole Wymondesolde family over many years, and the obvious affection displayed in the extant correspondence suggests that the relationship between Bradley and Elizabeth Pound was more than mere friendship. The Peach family may have believed that the relationship between Bradley and Elizabeth, even if entirely in accord with the demands of polite society, was something that might detract from his memory. Bradley's private and personal correspondence is strikingly absent from the archive. Having briefly focused on the problems imposed by the 19th century

construction of Bradley's work, and with the lack of personal papers in the Bradley corpus, this is an opportune moment to set out most of the major objectives of the thesis.

The first objective is a reconstruction of "the James Bradley of early Hanoverian England". An assessment of Bradley's role in the development of the theory and practice of Newtonian principles in the context of the social and political values of early Georgian England will be attempted. This will entail an analysis of aspects of Bradley's more important astronomical work at Wanstead, Kew, Oxford, Shirburne and Greenwich.[86] It will include a study of his training under Pound and Halley, and how this related to his practice as a Newtonian astronomer. I will examine aspects of the social basis of Bradley's commitment to Newtonian principles in the context of the values of early Hanoverian polite society. The origins of his far reaching reforms of the practice of positional astronomy will be firmly placed in the context of his commitment to Newtonian philosophy and the values of Hanoverian England.

The second objective will be an evaluation of the grounds upon which Bradley's Hanoverian reputation was based.[87] It will include an examination of some of the social, political and philosophical contexts within which his contemporary reputation was constructed. Bradley was widely accepted as the Newtonian astronomer *par excellence*.[88] His work on aberration and nutation analysed as applications of Newtonian principles impressed his astronomical and mathematical contemporaries, particularly in his commitment to the rigour, accuracy and precision that was seen as a virtuous exemplification of the ideals of the scientific gentleman. It will also examine the symbiotic relationship Bradley had with his principal instrument makers, particularly George Graham and John Bird.

The third objective will be to examine and evaluate Bradley's role as a teacher of astronomical theory and practice, and experimental philosophy. He was a lecturer and professor at Oxford University for almost 40 years. Until 1749 he gave three courses a year on astronomy and experimental philosophy, and from then until 1760, even when his commitments at Greenwich, his committee work at

the Royal Society and the Board of Longitude expanded, he gave two complete courses of lectures a year. His lectureship in experimental philosophy became a readership and the background to this change will also be examined.[89]

The fourth objective of the thesis will be to assess the role of patronage in Bradley's career. In 18th Britain, patronage was one of the social realities which all prospective office holders had to contend with. In Bradley's life the role of patronage was central to his advancement from obscurity to the office of Astronomer Royal. Bradley was associated very largely with members of the Whig elite and it was these connections and his evident ambitions that were largely responsible for his elections to office, and less his abilities as an astronomer or as a teacher. His advancement was also supported by his Newtonian associations. Bradley acquired a reputation as a supporter and promulgator of Newtonian theory and practice. These connections were also influential. His reputation in Europe was enhanced by his association with Newton. I will attempt to show that Bradley regarded these connections very highly, particularly when I examine the work he undertook with Molyneux on the new discovered motion, when Bradley defended his Newtonian associations in the face of Molyneux's own ambivalence.[90]

**An Exegesis of Bradley's Work.**

An analysis of the work of James Bradley poses an exegetical problem that is difficult to circumvent, and which if not carefully dealt with can lead to confusion and unnecessary duplication. On the one hand it would be possible to present Bradley's work in chronological order. The problem is that Bradley worked at more than one centre at any one time on activities which were sufficiently contrastable to yield analytical and narrative confusion if such a chronological approach were adopted, though it is be possible to present his early work in such a lucid manner when Bradley was working solely under the direction of Pound and Halley at Wanstead. But after Bradley was elected the Savilian Professor of Astronomy at Oxford in 1721, his activities were eventually to be divided between Oxford, where he taught, and Wanstead, Kew and

Greenwich where he undertook the bulk of his astronomical observations.[91] To attempt a strictly chronological account would serve little purpose but to confuse the reader. After 1732 Bradley's life becomes increasingly complex, for in partnership with George Parker, the 2nd Earl of Macclesfield, he also undertook astronomical observations at Shirburne Castle.

After January 1742 when Bradley was appointed as the 3rd Astronomer Royal,[92] and began his work transforming the Royal Observatory at Greenwich,[93] he continued his now increasingly demanding work as Savilian Professor of Astronomy and as Lecturer and then Reader in Experimental Philosophy at Oxford, as well as his observational work at Shirburne. He also visited Wanstead for several years up to 1747 as he continued his exacting work with the zenith sector on the nutation of the Earth's axis.[94] By 1750 he was also an industrious and continuous member of the Council of the Royal Society,[95] including several years under the Presidency of his friend and patron George Parker, and he also served as a member of the Board of Longitude. At Greenwich he trained several assistants to the exacting standards he demanded of his own observations,[96] built a new observatory with living quarters for his assistants, and successfully re-equipped the Royal Observatory with new instruments of unprecedented precision and accuracy.[97]

Given this complexity a strictly chronological approach has been abandoned. A thematic approach was adopted for it enabled a reasonably coherent and lucid entry into Bradley's work. It still raised problems. Originally the approach that was chosen was based on the locations of Bradley's work, Wanstead, Oxford and Greenwich, together with Kew and Shirburne as well as work at sea, but it soon became obvious that this led to unnecessary repetition and unforeseen duplication, especially when Bradley was working almost contemporaneously at three or four centres of activity. The thematic approach forced itself forward as the best way that the material could be organised. The problem is that if a strictly thematic approach were adopted it created an impression that each activity was being pursued in isolation from all the other activities that demanded Bradley's time. The approach that eventually emerged

was thematic but with frequent cross reference to his other contemporaneous activities, particularly if they impinged on the activity under attention. As an approach it is not entirely satisfactory, but in practice it proved to be the only possible way I could work the diversity of the available material in anything that approached lucidity. Before undertaking these tasks I think it necessary to examine the period when he was learning his craft under the direction of Pound and Halley, and when he was being prepared for the cloth under the guidance of his uncle. It is the period when he moved to his early maturity as an astronomer, and when he received his grounding in the Newtonian school. It is a critical phase in Bradley's life when he was introduced to the first patrons who would support his ambitions.

**Early Life and Education.**

James Bradley was a man of very modest origins, whose family were well connected but comparatively impoverished gentry. Repeated researches have failed to ascertain why the Bradley family's fortunes were so ill-served.[98] During the period of the civil wars and the interregnum the fortunes of many families were often transformed for the better or worse. The Restoration sometimes restored the fortunes of families, but the Bradley family appear to have remained in straightened circumstances. Bradley's well expressed sympathies suggest that the Bradley family may have supported opinions or connections that were not favourably received in the restored court. This is conjectural, but the only records that have been traced reveal members of the family holding positions of trust in the service of major landowners in Gloucestershire. In the case of the Dutton family, with whom Bradley's father was employed, their fortunes were skilfully upheld by John Dutton,[99] who astutely protected his interests.

Bradley's early life is shrouded in obscurity. His family possibly originated in Yorkshire,[100] but there are a few references to their appearance in and near Cirencester during the interregnum.[101] His grandparents Lewis and Eleanor Bradley lived in Hampnet, a village near the prosperous township of Northleach.[102] Their third son, William, married Jane Pound of Bishop's

Cannings[103] near Devizes in Wiltshire in 1678. James Bradley was the third son of this marriage. He appears to have been born in the village of Sherborne, in the Windrush Valley about 23rd March 1692/93.[104]

There is an oral tradition in Sherborne that the Bradley family resided in the Monks House.[105] Although William Bradley appears to have been employed as a steward on the Sherborne Estate, his income was never secure. At the time of the birth of James Bradley the estate was in the possession of Sir Ralph Dutton.[106] He was a heavy gambler whose extravagance threatened the management and finances of considerable holdings. Until his son, John Dutton, drafted an indenture of assignment, dated the 4th of February 1709/10,[107] and took control of the estate from his father the finances of Bradley's family remained insecure. It must be assumed that William Bradley, mindful of the insecurity of his income made arrangements with his brother in law, Rev. James Pound[108] for the education of his third son for the church.

Bradley was educated at Westwood's Grammar School in Northleach.[109] He was admitted as a commoner at Balliol College, on the15th of March, 1710/11. For much of this time Bradley resided with Pound in Wanstead.[110] He gained his BA degree on the 15th of October, 1714, and acquired an MA on the 21st of June, 1717. Due to the lack of any extensive private papers it is difficult to reconstruct the early years of Bradley's life with any confidence.[111] His early years in Wanstead are difficult to substantiate, though according to Rigaud, Bradley contracted smallpox during 1717, when his uncle nursed him back to health.[112]

Although it is possible that Pound may have developed an interest in astronomy during his extended sojourn in the East Indies, it was only after his return to England that he was able to undertake serious observations. By 1715 Pound had acquired a reputation as one of the keenest observers in the country, and was pioneering the use of the transit instrument in England. As the relations between Newton and Flamsteed reached breaking point[113] other astronomers were sought by the President of the Royal Society and Halley. Pound was amiable and approachable and was soon regarded by Newton and Halley as a reliable and trustworthy friend and colleague.[114] Halley made extensive use of Pound's

skills[115] and Newton used Pound's corrections for the determination of the orbit of the comet of 1680.[116]

**Early Astronomical Work.**

The circumstances that led to Bradley's astronomical career were serendipitous. As the third son of a gentleman it was not uncommon for his education to be directed to him taking holy orders. However, if his father's employer had not been so unreliable his education would not have been dependent on the goodwill and support of his maternal uncle, the man who was not only to introduce him to astronomy, but who was also give Bradley access to the patronage that would set his career on a firm footing. It is impossible to determine when he first developed his interest in astronomy, but it may date from the time he first went up to Oxford in 1711, for from this period he lived largely at his uncle's residence in Wanstead. Well before he had taken his MA in 1717, Halley was referring to the work of Bradley in a letter to Pound.[117] The earliest *recorded* observations date from 1715[118] and it is obvious from these entries that Bradley was already a skilled astronomer. Halley published one of Bradley's observations in 1717.[119] In this account Halley obviously holds a high regard for Bradley's abilities, writing that he was "a well instructed young person who also in his ingenious skills and enthusiasm revealed an inborn aptitude."[120] In 1718 Halley referred to G. D. Cassini's method of applying threads at half right angles in the common focus of the telescope and wrote of the exactness of Pound and Bradley. Halley wrote,

> Dr. Pound and his nephew Mr. Bradley did, myself being present, in the last opposition of the sun and Mars, this way demonstrate the extreme minuteness of the sun's parallax, and that it was not more than 12" or less than 9".[121]

The data is available in the *Miscellaneous Works*[122] where Bradley's abilities become apparent.

There can be little doubt that whilst Pound had acquired a reputation as the finest amateur astronomer in England, his skills were already being transcended by his nephew before 1720. During the following opposition of Mars in October 1721, Bradley repeated the observations he had made earlier with his uncle.[123] These observations are characteristic of his approach, which he was to develop with ever greater refinement throughout his entire career. The observations are more numerous than Pound's. Such industry and reiteration which is so indicative of Bradley's method enabled him to find the Sun's mean parallax more accurately by 1.2". An indication of the level of skill reached by Bradley during the early years of his work as an astronomer can be no better demonstrated than by reference to his work on double stars. John Wallis had earlier written on the parallax of the fixed stars and had indicated a possible means by which the relative motion of close double stars might offer a method of measuring annual parallax.[124] Bradley decided to utilise this method by observing the double star, *Castor,* the star designated as α *Geminorum* in Flamsteed's Catalogue.[125] Bradley measured the relative positions of the two bright components on the 30th of March, 1719 and the1st of October, 1722.[126] It did not lead to the discovery of annual stellar parallax, but the great precision and accuracy of this work was later confirmed by John Herschel and James South,[127] while they were undertaking their own survey of William Herschel's catalogue of double stars.

Another example of his consummate skill, which again is confirmed by later astronomers, is related to his work on Jupiter's satellites. Pound possessed tables that were based on G. D. Cassini's of 1693. Pound corrected these as he compared his observations with the tables, and communicated his results to Halley, who published Pound's results.[128] Bradley observed the four satellites of Jupiter consistently for several years, calculating each eclipse, observing it, and then recording the anomalies with great care. He drew up his Jovian Tables and presented them to Halley. Although the tables were complete by 1719 they were not published until the 1750s.[129] In 1752 Bevis, who was in close contact with Bradley at this time, translated the tables from Latin into English, and verified that much the greater part of Halley's tables were in fact the work of Bradley.

Bailli later gave Bradley the credit for detecting most of the anomalies which had been identified in the motions of the satellites of Jupiter.[130] The work that Bradley undertook on the satellites of Jupiter was to lay one of the most important foundations of his hypothesis of the new discovered motion, the phenomenon that was later termed the aberration of light.

Whilst Bradley was achieving a high reputation as an astronomer with such figures as Halley and Newton, there was no prospect of earning a living from the pursuit of his interest. By 1719 it would appear that Bradley was beginning to rival his uncle's skills as an astronomer, and this was recognised when Edmond Halley supported his election as a Fellow of the Royal Society on 6th November 1719.[131] At this juncture Bradley was beginning to accept that his future lay in the Church of England.

**Ecclesiastical Career and Early Patrons.**

From an early age it had always been the intention that Pound would prepare his nephew for a career in the church. From 1717 after obtaining his MA on the 21st June, Bradley began his studies, under Pound's guidance, to obtain holy orders.[132] Bradley was ordained as a deacon on the 24th of May, 1719[133] by the Bishop of London, John Robinson.[134] The vicarage of Bridstow, near Ross on Wye in Herefordshire, became vacant in 1719. On the 25th July 1720, Bradley was admitted to priestly orders by the Bishop of Hereford, Dr. Benjamin Hoadly.[135] On the 27th July he was given the living at Bridstow, and appointed as Hoadly's personal chaplain and secretary.[136] The roles of his connections are already evident.[137] Hoadly was an FRS, a friend of Pound, who also had connections with the Newtonian Circle. In 1738 Hoadly produced the only complete edition of Samuel Clarke's works.[138] In this association with Hoadly, a man who according to one writer lived for controversy and preferment,[139] Bradley was committing his future advancement and preferment to the Whig interest. The fortunes of High Church Tories were in rapid retreat because of the suspicion, usually unjustly, that they supported the Jacobite restoration. The association of one of the leaders of the High Church Tories, Francis Atterbury, with the Jacobite

cause,[140] did nothing to help their cause and helped the Whigs who were placed in the ascendancy.

Other connections were to offer Bradley a further sinecure in Wales. With the patronage of the parish of Llanddewi Velfrey,[141] near Narberth in Pembrokeshire, in the power of the Prince of Wales,[142] Bradley was able to take half of this divided living through the influence of his friend Molyneux, the Prince's private secretary, who in turn was a close friend and associate of Pound. For the third son of an obscure family of gentry Bradley was moving in very auspicious circles early in his life. Neither of his livings were very lucrative, so he remained to some degree in his uncle's beneficence. Bradley remained living for much of his time in Wanstead. When the Savilian Professorship in Astronomy at Oxford fell vacant in 1721 at the demise of John Keill[143], even more powerful sources of patronage were to come to his aid and support. These were to include Thomas Parker, the Lord Chancellor and William Wake, the Archbishop of Canterbury.[144]

The role of these powerful patrons in the election of Bradley as Keill's successor will be discussed later, when examining Bradley's work at Oxford. As I have already indicated Pound was at the centre of a powerful network of political, religious and scientific patrons. With his wife's considerable fortune,[145] and his lucrative livings at Wanstead, and following the demise of John Flamsteed, at Burstow in Surrey he was without doubt a person of wealth and influence, and it was this that was to stand Bradley in good stead in a society in which connections were the crucial factor when seeking social advancement.[146] Pound's role at the centre of a circle of influential astronomers and natural and experimental philosophers and the influence this had on Bradley's astronomical practice will be examined in the context of the development of his later career as an astronomer.

One of the objectives of this inquiry will be to demonstrate that Bradley's astronomical practice was created within a milieu that both encouraged and constrained this practice within the dominant values of early Hanoverian society. The work of Bradley was given its direction and its value in the contexts of the mores of a society in which reason and exactitude were often given a high value.

**James Pound and the Wanstead Circle.**

As I have intimated Pound was at the centre of an influential circle of astronomers and experimental philosophers. I will call this the Wanstead Circle,[147] not because the circle lived in Wanstead, but because so much of its activities were centred on Pound's residence in Wanstead. This section will examine the work that Pound undertook with or for Newton, Halley, and other members of this select group of capable astronomers, mathematicians and natural philosophers.

Although Newton is usually accredited with the construction of the first reflecting telescope,[148] an instrument which appeared to deal with many of the problems associated with spherical and chromatic aberration, most reflectors were not at all satisfactory and astronomers preferred using long focus suspended refractors because of their greater light grasp. The enduring problem stemmed very largely the mechanical difficulties of producing satisfactory specula or mirrors.[149] One of the Wanstead circle, the Rev. John Hadley of East Barnet, particularly applied himself to the problem of producing an efficient astronomical reflector, producing the first such instrument of sufficient light grasp to be useful to astronomers. His first success was a Gregorian reflector, constructed in 1720 with a mirror of about six inches in diameter with a focal length of 62 inches.[150] Although lacking in light grasp in comparison to the best refractors, it was nevertheless a practical instrument, and it was demonstrated at two meetings of the Royal Society on the 12th and the 19th of January, 1721.[151] During 1721 and 1722 the reflector was used extensively by Pound and Bradley, and the instrument eventually remained in the possession of Bradley.[152] Hadley had originally presented the instrument to Halley in order that the Royal Society might have possession of it.[153] Within weeks Pound and Bradley undertook a series of tests with the Hadley reflector and compared its performance with the 123 foot glass which had been presented to the Royal Society by Christiaan Huygens. Although the Hadley instrument possessed a focal length of only 62 inches, the images were as distinct as they were with the 123 foot telescope, its only problem being a

distinct lack of light grasp in comparison to the glass.[154] It was this problem which set Molyneux, Hadley and Bradley to experiment repeatedly with the synthesis of different alloys,[155] in the hope of improving the light grasp of the reflector.[156] Everyone using the telescope was impressed by the ease with which it could be used, with the facility of directing and focussing the instrument.[157]

The Huygenian refractor was celebrated for its efficiency. When Halley wanted to observe the transit of Mercury over the Sun's disc on 29th October 1723, he left Greenwich for Wanstead, in order to use the Huygenian glass with Bradley's assistance.[158] The Huygenian refractor, although the property of the Royal Society, had been made available to Pound by Newton in 1718.[159] The evidence of Pound's Account Book confirms this, for much effort was made in erecting the pole from which the Huygenian telescope could be suspended.[160] The entries include the following,

| | | |
|---|---|---|
| 18th September 1717 | Buy tin and brass work for Huygen's telescope. | £0 4s 6d |
| 25th April 1718 | Buy an eyeglass for the long telescope. | £1 2s 6d |
| 13th May 1718 | Buy drink for the men who raised the pole. | £2 0s 0d |
| 16th May 1718 | Paid several men half a day's work in assisting in lifting the pole | £0 17s 0d |

I include these entries to show that the erection of Huygens' telescope was a major undertaking. It remains an abiding folk memory in Wanstead, and there also remains much dispute about the exact location of the instrument after it had been erected.[161] Both Pound and Bradley became very skilled in the use of the long instrument.[162] This skill is clearly demonstrated in Bradley's later use of a telescope with a focus of 212 ¼ feet.[163] On 27th December 1722, Bradley used this contrivance to twice measure the diameter of the disc of Venus with a result that is in conformity with modern values. Rigaud estimated that the planet was barely 12° above the horizon,[164] so these measurements were an extraordinary feat using a telescope with such a long focus. It would have required a pole of at least 45 feet.

Bradley's first publication was on the comet discovered by Halley on the 9th of October 1723.[165] Halley immediately informed Bradley, who began observing it from the 10th of October, and continued to do so until the 14th of November, with the assistance of Pound.[166] Bradley then had to travel to Oxford

to take up his duties as Savilian Professor of Astronomy, where he alluded to the comet during a public lecture entitled *De Cometa* on the 28th November.[167] Although he referred to its appearance and forms conjectures, his approach combines his observations and calculations of the elements of the trajectory of the comet. It includes the parabolic path calculated using Newton's method.[168] It is obvious that Bradley must have calculated these elements almost immediately after he arrived in Oxford after 14th November 1723, for the elements published in the *Philosophical Transactions*[169] are almost identical to those given in his public lecture on the 28th November,[170] though his final data is more precise. Bradley's calculations run to 16 folios.[171] The methods of analysis which enabled astronomers to calculate the elements of an orbit with a handful of observations, pioneered by Daniel Bernoulli,[172] and later perfected by Pierre-Simon Laplace,[173] were not available to Bradley. When Newton undertook work on the 3rd edition of the *Principia* he included this work, for he asserted that it supported his view of comets.[174]

Pound was heavily involved with the work on Hadley's reflector, and his help was central to the erection and use of the Huygenian refractor.[175] He was also supportive of Bradley's work on the 1723 comet.[176] Pound's role should never be underestimated, for his knowledge, conviviality and generosity made him a man who was ready and able to act as a catalyst in the work of many of his circle. This is very evident in the preparatory work for an attempt to measure the annual parallax of a star.[177] It was due to various meetings between Pound and Molyneux, undertaken when Molyneux was working with Bradley and Hadley during the development of hundreds of different alloys[178] in the pursuit of more efficient specula for reflecting telescopes, that the notion was discussed of seeking the annual parallax of a star utilising the method pioneered by Robert Hooke in 1669.[179]

Hooke first presented his plan to the Royal Society in 1666, and according to Jean-Baptiste-Joseph Delambre, he was encouraged by the society to renew the project with all possible urgency.[180] Due to a combination of illness and overwork necessitated by his heavy work schedule as the City Surveyor,[181] Hooke was

unable to start work on the project until 6th July 1669.[182] The method utilised by Hooke in his attempt to measure the annual parallax of a star was ingenious and practical. One of the major problems facing any astronomer who sought to measure the annual parallax of a star was that of the distortion induced by atmospheric refraction. Hooke sought to overcome the problem by proposing to measure the passage of a star as close to the zenith as possible. The second magnitude star γ Draconis passed within two minutes of arc to the zenith at the latitude of Gresham College in London.[183] The method promised an opportunity of achieving the measurement of annual parallax in order to establish the veracity of the Copernican thesis.

Hooke did not publish his results until 1674, when he gave the first Cutlerian Lecture.[184] Concerning his claim Hooke wrote,

> 'Tis manifest then by the observations of July the Sixth and Ninth: and that of the one and twentieth of October that there is a sensible parallax of the Earth's Orb to the fixed star in the head of Draco, and consequently a confirmation of the Copernican System against the Ptolomaick and Tichonick.[185]

Hooke claimed to have observed an annual parallax of about 27" or 30".[186] In Appendix 7 of my MSc dissertation[187] I recorded Bradley's own comments on Hooke's results,

> According to my Observations the Star would have been 5½" more northerly Oct 21 than 'twas July ye 7th or 8th whereas Dr Hook make it 23" more Southerly Diff = 28½". And by my Observations twas 2" more Southerly on Oct 21 than on August 6th whereas Dr Hook makes it 17" more southerly that is 15" Difference from mine.[188]

Bradley was excessively generous in his evaluation of Hooke's results ascribing the hopeless inaccuracy of his observations down to the inadequacy of the instrument at his command. However, he could, given the thoroughness of his own observational practice, have been highly critical of Hooke's characteristically overblown claim to have measured the annual parallax of γ Draconis on the basis of four observations over a period of just three and a half months.[189]

In spite of the shortcomings of Hooke's own attempt, the method that he had utilised, of observing at the zenith, promised to be the means of achieving the objective of measuring the annual parallax of a star. When Pound and Molyneux began to discuss their plan to undertake the observation and measurement of annual parallax, the method used by Hooke promised to offer the means.[190] Pound paid various visits to Molyneux in Kew.[191] Pound and Molyneux began to discuss the problem with another member of the Wanstead Circle, George Graham the leading clockmaker, and one of the leading scientific instrument makers in London.[192] The problem, although not urgent,[193] was one that intrigued astronomers. Most astronomers in Western Europe were committed to the heliocentric theory, although a few resisted the Copernican thesis and maintained geocentric views.[194] After the untimely death of Pound on the 16th November 1724,[195] the project came under the direction of his partner Molyneux. Hooke had used a zenith sector with a focal length of 36 feet, but it had proved exacting to use and even more difficult to rectify. Following Pound's intentions, Molyneux commissioned Graham to construct an instrument with a focal length of 24 feet, a length determined by the height of Molyneux's house on Kew Green.[196]

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[1] On the 9th and 16th January, 1728/29 OS.

[2] It is a firm measure of Bradley's own judgement and character that he admitted that his conjecture rested on the basis of two factors which had not been independently established. The evidence of the motion of the Earth was dependent on the acceptance of the progressive motion of light, whilst the evidence of the progressive motion of light was dependent in its turn on acceptance of the motion of the Earth. In his account Bradley conceded that "the Anti-Copernicans have still room on that account to object against the motion of the earth." James Bradley "A Letter to Dr. Edmond Halley, Astronomer Royal and etc., giving an account of a new-discovered Motion of the Fixed Stars", in the *Miscellaneous Works and Correspondence of James Bradley,* p.16.

[3] At the very moment when James Bradley's paper was read to the Royal Society, the Italian astronomer Eustachio Manfredi was completing a thesis using the evidence of "aberrations", a phenomenon that was the equivalent of Bradley's "new discovered motion" as firm evidence against the validity of the Copernican thesis. As will be discussed in this thesis, this is a good example of compatible evidences being used to argue for opposing theoretical conclusions. Bradley's hypothesis was widely accepted.

[4] The word "nutation" translates as "nodding". The axis of the Earth's rotation is subject to a fine variation amounting to less than one arc second a year. This is due to the differential attraction of the Moon to the Earth's equatorial "bulge" as the orbit of the Moon precesses in a period of about 18 years. The Moon's orbit is about 8° out of the plane of the Earth's equatorial plane, and the Moon passes through this plane twice each month at the "nodes" of this orbit. Bradley was able to demonstrate that the nutation was directly related to the precession of the nodes of the Moon's orbit around the Earth.

[5] Isaac Newton, *Philosophiæ Naturalis Principia Mathematica,* 3rd ed. 1726, Bk.3, Prop.39, Prob.20. Newton states, "If the height of the earth at the equator exceeds its height at the poles by more than 17 ½ miles, matter will be rarer at the circumference than at the centre; and the precession of the equinoxes has to be increased because of that excess in height, and diminished because of the greater rarity". *A New Translation by I. Bernard Cohen and Anne Whitman,* London, 1999. The precession of the equinoxes is thus assumed to be slightly variable because of this variation in the figure of the earth. No attempt is made to quantify this possible phenomenon. Bradley observed the phenomenon, amounting to about one second a year for a period equal to a complete cycle of the nodes of the lunar orbit. It was an extraordinary feat, which demonstrated not merely observational precision and accuracy of a high order, but a thorough insight into the theoretical consequences of Newtonian mechanics.

[6] Bradley, *A Letter to the Rt. Hon. George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Star,* in *Philosophical Transactions,* No. 485, Vol. XLV, 1748. p1 passim, and *Miscellaneous Works and Correspondence of James Bradley*, Oxford, 1832, pp 17 to 41.

[7] An examination of the prize essays, written in the mid 1740s, of John Bernoulli's *Essai d'une nouvelle physique célèste*, with its distinct Cartesian characteristics, including an appeal to the existence of vortices, and Daniel Bernoulli's, *Recherches physiques et astronomiques*, which covers similar ground, but which reveals a distinct commitment to a more Newtonian interpretation and a distinct scepticism about these Cartesian constructs, reveals that the conflict between these systems was moving to a climax at the time when Bradley was preparing his paper on the nutation. I thank the late Eric Aiton for these insights, and for first bringing these sources to my attention. Eric Aiton stressed that there was, in effect, a rapprochement between the Cartesian and Newtonian approaches, between a search for causes and an analysis of physical processes.

[8] Bradley to Maupertuis, *Miscellaneous Works and Correspondence of James Bradley*, Oxford, 1832, pp 408 to 410.

[9] It was Allan Chapman who first mentioned to me the enormity of the commitment that Bradley set himself in his observation of nutation through a complete cycle of the lunar nodes. Bradley must have been aware that he might not have lived long enough to see the project through to the end.

[10] Halley is reputed to have wanted to resign the office in favour of his protégé Bradley.

[11] George Parker, the 2nd Earl of Macclesfield, used this term in a letter addressed to William Jones in January, 1742. It must be assumed that he was referring to the Royal Observatory, even though it was in a very parlous state in 1742, when Lord Macclesfield was lobbying his political connections in support of his friend Bradley.

[12] In July 1734 Bradley examined and adjusted the 8 foot quadrant at Greenwich. In July 1735 he was joined by Graham and Machin as various adjustments and repairs were made to the instrument.

[13] The "professionalisation" of science is a term that is usually reserved for a process that began during the 19th century. In the main I also have to concur with this view, but the assistants at Greenwich were trained to high standards of rigour, worked consistently, and were paid. By any sensible definition of the term this has to be regarded as professionalism.

[14] Shortly after constructing the new quadrant at the Royal Greenwich Observatory, John Bird was commissioned to construct a similar instrument for the Russian Imperial Observatory at Pulkovo near St. Petersburg.

[15] Nathaniel Bliss succeeded Bradley from 1762 until his own demise in 1764.

[16] Friedrich Wilhelm Bessel, *Fundamenta Astronomiæ pro anno MDCCLV deducta ex observationibus viri incomparabilis James Bradley in specula Grenovicensi per annos 1750 – 1762 institutis*, Königsberg, 1818.

[17] Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation." in *Science in Context,* Vol. 2.1 (1988), pp. 115 – 145, p. 115.

[18] From 1721 to 1762.

[19] He was Lecturer in Experimental Philosophy from 1729 and Reader from 1749. He resigned in 1760 as his health declined.

[20] From 1742 to 1762.

[21] L. S. Sutherland et al (eds.) *The History of the University of Oxford: The Eighteenth Century,* Oxford, 1984.
[22] John Cannon, *Aristocratic Century: The peerage of eighteenth-century England,* Cambridge, 1984. The decline of the English universities can perhaps be measured in terms of matriculations. Thus in the decade from 1710 to 1719, the period when Bradley graduated BA and MA, 3,062 matriculated at Oxford and 1,871 at Cambridge. In the decade from 1750 to 1759, the last decade during which Bradley taught at Oxford, 1,820 matriculated at Oxford and 1,392 at Cambridge. See Cannon, "Aristocratic Century", p. 45.
[23] Graham Midgley, *University Life in Eighteenth-Century Oxford*, London, 1996.
[24] John Cannon, "Aristocratic Century: The peerage of eighteenth-century England", Cambridge, 1984. p.45.
[25] The building is now termed the Old Ashmolean, and is now the site of the Museum for the History of Science, in Broad Street.
[26] F. W. Bessel, ibid, Bessel described Bradley's observations as "incomparable".
[27] S. P. Rigaud, *Memoirs of James Bradley*, Oxford, 1832. The memoirs were included in Rigaud's edition of Bradley's major works, *Miscellaneous Works and Correspondence of James Bradley*.
[28] Rigaud published *Correspondence of Scientific Men of the Seventeenth Century* in 2 volumes in 1841.
[29] Francis Baily, *An Account of the Rev. John Flamsteed, the First Astronomer Royal; compiled from his Manuscripts and other authentic Documents, never before published,* London, 1835.
[30] William J. Ashworth, "'Labour harder than thrashing': John Flamsteed, Property and Intellectual Labour in Nineteenth-Century England." in Frances Willmoth (ed) *Flamsteed's Stars: New Perspectives on the Life and Work of the First Astronomer Royal, 1646 – 1719.* pp. 199 – 216. See Ashworth, ibid. p. 199.
[31] ibid. p. 211.
[32] ibid. p. 211.
[33] ibid. p. 212.
[34] Later called the aberration of light.
[35] As this thesis will clearly demonstrate the theory of the aberration of light was a construct formed out of Bradley's heuristic, in which the theoretical and the practical are implicitly integrated.
[36] Arthur Berry, *A Short History of Astronomy from Earliest Times through the Nineteenth Century*, London, 1898. Dover reprints, New York, 1961, pp257 – 273.
[37] Agnes Clerke, *James Bradley*, Dictionary of National Biography, 1882, Vol.2, pp1074 – 1079. Clerke pays Bradley the complement of describing him as "the founder of modern observational astronomy".
[38] This will be demonstrated in a study of the publication of the paper on aberration.
[39] The terms *Newtonian* and *Newtonianism* are often used loosely, and so variously that they have almost become meaningless. Many 18th century itinerant lecturers often demonstrated various phenomena as *Newtonian* which in fact had little or nothing to do with anything recognisably so. Throughout this thesis, unless I clearly indicate otherwise, I shall be using the term in a much more rigorous way. Newtonian science will be that which involves a mathematical analysis of natural phenomena, the empirical testing of theories based on such mathematical analyses, and the emphasis on observational accuracy and precision, allied to theoretical rigour.
[40] John Conduitt, *Memorabilia*, Keynes MS. 130.5, Cambridge University Library, Microfilm MS. 659.
[41] Samuel Molyneux was the son of the Irish philosopher and politician William Molyneux who was the originator of the famous philosophical problem named after him. The Molyneux problem is about the correlation of visual and tactual perception, about which he corresponded with John Locke. The problem posed the possibility of a blind person having perceived a geometrical form like a cube by touch, acquiring the sense of sight. Would the blind person correlate his tactile perception of the cube with the shape of a cube by sight alone? Samuel Molyneux married Elizabeth Capel, one of the richest heiresses in England. He was the Prince of Wales' private

secretary. After his accession to the throne as George II, Molyneux was appointed as one of the Lords of the Admiralty.

[42] There is every likelihood that Halley was of a similar mind as Bradley about Molyneux's motivations. He was very fulsome in his encouragement in getting the paper read as soon as possible.

[43] The contrast between the work conducted at Kew by Molyneux and Bradley and the work conducted by Bradley at Wanstead reveals the different presumptions of each of the partners. This will be thoroughly discussed in chapter two.

[44] Bradley was ordained as a deacon by the Bishop of London on 24th May, 1719 and as a priest by Benjamin Hoadly, Bishop of Hereford on 25th July, 1720. On the 27th July, 1720 he was admitted to the living at Bridstow , near Ross on Wye. He was elected as the Savilian Professor of Astronomy at Oxford in October 1721, taking up his office on 18th December, 1721. It was a condition of the acceptance of the Savilian professorship that Bradley gave up all his church preferments.

[45] It determined that the Protestant succession would pass through the Electress Sophia of Hanover, who was the granddaughter of James I of England. It forbade any Catholic from accession to the monarchy, and specifically barred any descendent of James II of England. The Whigs fully supported the Act of Settlement. Most Tories also supported the Act but many were ambivalent about its legitimacy. Few were openly sympathetic to the Jacobite cause, but they were to remain suspect to the Hanoverians. Only after the irrevocable defeat of the Jacobite cause in 1746 were the Tories able to make a serious challenge to the Whig supremacy.

[46] William Wake was a leading Whig Churchman, and was Archbishop of Canterbury from 1715 until 1737. His nephew Martin Foulkes was President of the Royal Society, and both were to be patrons of Bradley at important stages of his career.

[47] Wake was also the Chancellor of the University of Oxford, and was a leading elector when the Savilian Professorship of Astronomy became vacant following the demise of John Keill in 1721.

[48] Particularly in the late 1740s and 1750s.

[49] This is the considered view of Rigaud. For various reasons later considered I firmly believe Bradley was a generous host and was certainly partial to the pleasures of the palate.

[50] Henry Pelham, the Prime Minister from 1743 until 1754, during the period prior to Bradley being appointed as the 3rd Astronomer Royal in 1742, stated that it was difficult to conceive of a kindly man like Bradley possessing enemies in an undated letter addressed to Walpole. Part of this letter is to be found in the Bodleian Library, Bradley MS. 46, *Papers in print prior to 1829.* Bradley entertained the Pelham family through the 1740s and 1750s.

[51] Arthur Berry, "A Short History of Astronomy", takes issue with Thomson's account. I will be discussing Thomson in the thesis. Berry wrote a footnote on page 261 stating that the story (of Bradley's boating party on the Thames) as given in T. Thomson's *History of the Royal Society*, published more than 80 years after (the events recalled) had no earlier authority for it. I have also made a search for any written or printed source which might have given rise to Thomson's account without success.

[52] Quoted in Rigaud, "Memoirs" pp. xxx – xxxi.

[53] In a letter addressed to Bradley by Nicolas-Louis de Lacaille on 28th July 1744, "Miscellaneous Works" pp. 429 – 430, he wrote, "C'a ete pour moi une occasion de vous faire connoitre l'estime singulaire que j'ai de vos talens, et combiens j'applaudis a la reputation que vous etes acquise parmi les astronomes, qui ne font aucune difficulté de vous reconnoitre pour le premier." ("It would be, for me, an opportunity of letting you know the singular esteem I have for your talents, and how much I rejoice in the reputation, you have acquired among astronomers, who have no difficulty in recognising you as the first among them.").

[54] Bessel, "Fundamenta Astronomiæ".

[55] What really impresses is the thoroughness of it all. It is accurate, precise and rigorous; recording all sources of error, and it is voluminous. It is thoroughly "professional".

[56] *Nature,* 1st July 1999, Vol. 400, p 27.

[57] Science is a corporate activity with many different origins and traditions. The tradition within which Bradley worked was "Newtonian" and the founder of the kind of scientific activity

undertaken by James Bradley was founded (if such a term can be applied) by Newton and his contemporaries.

[58] Bessel, "Fundamenta Astronomiæ".

[59] Rigaud, "Memoirs". Oxford, 1832.

[60] T. Thomson, *History of the Royal Society,* London, 1808.

[61] It reveals Thomson's belief that the aberration of light was a discovery based on an insight. In fact, as the account written by Bradley reveals, it was a complex process in which insight certainly played a part, but it was clearly constructed on his practice as an astronomer.

[62] Rigaud, "Memoirs". pp. xxx – xxxi.

[63] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", pp. 1 – 17.

[64] Berry, "A Short History of Astronomy".

[65] Rigaud, "Memoirs". pp. xxx – xxxi.

[66] Rigaud discovered that much of Bradley's corpus had become dispersed, much of it unaccountably. He discovered many papers in the possession of Thomas Hornsby and with his predecessor's permission retrieved all of the Bradley papers he could locate. *Memoirs of Bradley,* p. x passim.

[67] It confirmed the presumptions of most contemporary astronomers. It was perfectly understandable why many regarded it as confirmation of the Copernican thesis. Bradley himself was more astute and expressed the view that the "Anti-Copernicans" still possessed room to object. *Philosophical Transactions,* No. 406, Vol. 35, p. 660.

[68] As I have already mentioned, Arthur Berry was highly critical of Thomson's account for he could not find any evidence for its source.

[69] Robert Grant, *History of Physical Astronomy from the Earliest Ages to the Middle of the Nineteenth Century comprehending a detailed account of the establishment of the Theory of Gravitation by Newton with an exposition of the progress of research on all the other subjects of Celestial Physics.* London, 1852.

[70] Rigaud, "Memoirs".

[71] Bradley, "A Letter to the Rt. Hon. George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars" in the "Miscellaneous Works", p. 40.

[72] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc., giving an account of a new-discovered Motion of the Fixed Stars". In the third unpublished version, found in John Roger Fisher, MSc dissertation, University of London, 1994, Appendix 3, *Bradley MS 20*,* fols 29r – 30r. There is a discussion about the distances and the sizes of the stars related to work undertaken by Halley on the motions of the stars.

[73] Bradley's conjectures are constructed on the evidence offered by his and other's astronomers' observations and measurements.

[74] Agnes Clerke, *James Bradley,* Dictionary of National Biography, London, 1882. Vol.2, pp 1074 – 1079.

[75] Anton Pannekoek, *A History of Astronomy*, London, 1961. Reprinted, New York, 1989.

[76] Pannekoek, "A History of Astronomy", pp 289 – 296.

[77] W. H. McCrea, *The Significance of the Discovery of Aberration,* Q. J. Royal Astron. Soc. March 1963. pp 41 – 43.

[78] Constructed on a rigorous analysis of measured data.

[79] Lord North was Chancellor of the University of Oxford in 1776.

[80] There is an extensive literature on the subject but reference will be made in the thesis to three sources. These are G. J. Barker-Benfield, *The Culture of Sensibility: Sex and Society in Eighteenth-Century Britain,* London, 1992; Amanda Vickery, *The Gentleman's Daughter: Women's Lives in Georgian England,* London, 1998 and A. D. Harvey, *Sex in Georgian England: Attitudes and Prejudices from the 1720s to the 1820s,* London, 1994.

[81] Elizabeth Pound received a substantial settlement from her brother Matthew Wymondesolde, who made a fortune from the sale of South Sea Company shares only a day or two before the "South Sea Bubble" finally burst. In addition she inherited a large fortune from her husband.

[82] This is apparent from the extant correspondence between Bradley and Matthew Wymondesolde, Elizabeth's brother.

[83] Bradley, "Miscellaneous Works". p 22.
[84] Rigaud, "Memoirs". p xxxix. Elizabeth Pound died after a protracted illness on 10th September, 1740, and was buried in Wanstead according to the directions of her will.
[85] Ibid. p ci. Bradley married Susannah Peach on 25th June, 1744. They had one daughter, Susannah who was born in 1745 at Greenwich. She married her first cousin, Rev. Samuel Peach on 10th January 1771.
[86] The work Bradley undertook with Molyneux and Graham at Kew will be examined with Bradley's work at Wanstead, with which it is connected. The work undertaken at Shirburne by Bradley and George Parker, though initially connected with Bradley's work at Oxford, has greater relevance when connected with the work undertaken at Greenwich.
[87] This will include some evaluation of his European reputation.
[88] It was on the basis of Bradley's "Newtonian" connections and associated reputation that he was approached by Maupertuis by way of correspondence.
[89] Rigaud, "Memoirs", p xxxviii. When Lord Crewe, Bishop of Durham, died in 1721 he left £200 per annum to the University of Oxford. Following a Decree of Convocation a sum of £30 per annum was to be set aside for "a reader in experimental philosophy". The terms of Lord Crewe's will made provision for Lady Stanwell, and James Bradley was unable to benefit from the decree, or accede to the title of "Reader in Experimental Philosophy" or receive the £30 annual income, until Lady Stanwell died in 1749.
[90] Samuel Molyneux was the Prince of Wales' private secretary and a valuable source of patronage. The Prince of Wales' intervention led to Bradley obtaining a living in Pembrokeshire. As George II, he also intervened on Bradley's behalf in order to ensure he obtained the resources he needed to reform and re-equip the Royal Observatory.
[91] Bradley worked up to April 1724 in partnership with Pound at Wanstead. Between 1724 and 1732 he worked at Kew in partnership with Samuel Molyneux (1725 to 1727) and at Wanstead as he worked on his observations of aberration and nutation. He occasionally worked at Greenwich when he undertook work in partnership with Halley. At Wanstead, where he lived with his uncle's widow he also undertook work up to 1726 on behalf of Newton, and up to 1732 on behalf of Halley.
[92] Bradley was appointed as Astronomer Royal by Sir Robert Walpole. Walpole appointed him the day after he was finally defeated in the House of Commons but before he tendered his resignation to George II.
[93] He constructed the Transit House, the building which is now familiar as the site of the Greenwich Meridian. The meridian was originally centred at the mid-point of the building where Bradley placed his transit instrument. Airy later moved it a few yards to the east in order to house his own transit instrument. Although the Greenwich meridian is now in the position of Airy's transit, the maps of the Ordnance Survey are still based on Bradley's meridian.
[94] The long awaited paper on the nutation of the Earth's axis was published in 1748. It firmly established the validity of Newtonian physics. Bradley was awarded the Copley Medal.
[95] He was a Councillor of the Royal Society, several times prior to 1750. He served in this capacity from 1751 to 1760 when his terminal illness made regular attendance impossible. Macclesfield served as President up to his death in 1763, the year after Bradley died.
[96] His assistants included his nephew John Bradley, Charles Mason and Nathaniel Bliss the latter both Gloucestershire men like both Bradleys, and Charles Green.
[97] Most of which were constructed by John Bird and Jonathan Sisson. Sisson was in the employ of Bradley's friend and partner George Graham, who in 1750 was in retirement. Graham died in 1751.
[98] Extensive searches have been made in Gloucestershire Records Office, The Local Studies Department and the Genealogical Department of Birmingham Central Library, the Public Records Office and various Parish Records in Gloucestershire and Wiltshire. References to the specific Bradley family that gave rise to the 3rd Astronomer Royal are difficult to find or verify.
[99] John Dutton (1594 – 1657) opposed the payment of ship money in the 1630s, and was imprisoned twice for refusing to pay this tax. Yet he remained a staunch Royalist and fought for the Royalist cause as a colonel, taking part in the siege of Gloucester. He was discharged from the service due to his physical disabilities. He is reputed to have been a hunchback and was

nicknamed "Crump Dutton". A few years into the interregnum Dutton had become a personal friend of Oliver Cromwell, and was instrumental in effecting the marriage between his nephew William Dutton and the Lady Frances Cromwell, the Lord Protector's youngest daughter. Many of the Dutton family remained staunch Royalists, including his daughters, whom he disinherited for refusing to change their monarchist views. At the Restoration, John Dutton's changing allegiances were overlooked due to the continued loyalty of so many in the Dutton family. The Duttons even retained much of the fortune acquired during the interregnum. *Gloucestershire Records Office, Sherborne Parish Records.* Also quoted in Sybil Longhurst, et al, *Sherborne: A Cotswold Village.* pp 39 – 40.

[100] Rigaud claimed that the Bradley family originated from Bradley Castle, Wolsingham, County Durham. *Memoirs of Bradley,* p i.

[101] Gloucestershire Records Office, *Rendcomb Parish Record.* This source records some dealings of the Guise estate, with some references to the Bradley family, who appear to have been employed by the estate. There is a passing reference to this in Rigaud's "Memoirs", p. i, stating that they were employed as stewards, though he refers to the village of Rendscombe.

[102] Northleach was a major centre of the Cotswold wool trade.

[103] Rigaud refers to a village of Bishop's Canning.

[104] Gloucestershire Records Office, "Sherborne Parish Records". The parish records for the period 1690 to 1703 do not exist. Rigaud claimed they were missing, but the parish church was closed for this period for structural renovations and any record of baptisms, marriages and deaths for this period cannot be located. The parish record ends in 1690 and then continues in 1703 without a gap. I have searched for baptismal records of several adjoining villages, including Windrush, Clapton, Farmington, Aldsworth and Bibury but without success. I have also checked the records for Northleach and Bradley's grandparent's village, Hampnett, but again without success.

[105] During the Middle Ages, the Sherborne Brook, which runs through the village of Sherborne (the name means clear brook), was used by the monks of Winchcombe Abbey to wash and gather the fleeces of their extensive flocks. At this time they resided at a large stone house near the stream. After the dissolution of the monasteries the house became part of the Sherborne Estate, which at the time of Bradley's birth, was the property of the Dutton family.

[106] Sir Ralph Dutton was created a baronet by patent on 20th June, 1678.

[107] Sybil Longhurst, Walter Tufnell & Alice Tufnell, *Sherborne: A Cotswold Village,* Alan Sutton, 1992, p. 42.

[108] Pound took the degree of B.M. on 21st October, 1697 at Hart Hall in Oxford, but does not appear to have practised medicine. He took holy orders in 1698. He was employed as chaplain to the East India Company settlement at Pulo Condore off the coast of Cambodia from 1700. *Memoirs of Bradley,* p. ii.

[109] It was founded by Hugh Westwood and placed under the superintendence of Queen's College, Oxford.

[110] After the settlement at Pulo Condore was destroyed in an uprising in 1705, Pound escaped and navigated a ship through hazardous, pirate infested waters, to Malacca and then Batavia. He returned to London in July 1706. In July 1707 he was presented with the rectory at Wanstead by Sir Richard Child, one of the leading shareholders of the East India Company. Wanstead was one of the most lucrative livings in England, where Sir Richard developed his estate, about 6 miles east of the City of London, in order "to balance Hampton Court" in the west.

[111] Bradley's daughter Susannah certainly removed everything she considered to relate to her late father's personal affairs, before her husband, Rev. Samuel Peach presented Bradley's papers to Lord North, the Chancellor of Oxford University in 1776.

[112] Rigaud, "Memoirs", p. iii. Pound had nursed his first wife Sarah, who died in June 1715.

[113] John Flamsteed's account of the breakdown in his relations with Sir Isaac Newton is included in a monograph, John Flamsteed, *The Preface to John Flamsteed's Historia Coelestis Britannica,* National Maritime Museum, Greenwich, 1982. pp 160 – 180.

[114] Rigaud, "Memoirs", p. ii. Rigaud asserts in a footnote that Pound had a reputation for being kind and amiable.

[115] Bodleian Library, Bradley MS 21, *A book of observations made at Wanstead by James Pound and James Bradley.*

[116] Bodleian Library, Bradley MS 21. fol. 56r
[117] Bodleian Library, Bradley MS 21. The letter is referred to by Rigaud in the "Memoirs", p. iii.
[118] Bodleian Library, Bradley MS 21. fol. 73v.
[119] Philosophical Transactions. Halley "Accuratarum Observationum Astronomicarum, auno Superiore – currente, cum Reg. Societate communicatarum Sylloge: Observationes Lunae & Eclipsium, Anno 1717, January 12th, pp. 847 – 858. p. 853.
[120] The Latin reads, "eruditus juvenis, qui simul ingenio et industria pollens his studiis promovendis aptissimus natus est"
[121] Philosophical Transactions. Halley "Some Remarks upon the Method of observing the differences of Right Ascension and Declination by Cross Hairs in a Telescope." Vol. 31, pp. 113 – 116. p. 114.
[122] Bradley, "Miscellaneous Works" p. 353. Also Bradley MS 16. where the observations are recorded in Pound's hand, which means that Bradley was the observer as his uncle wrote down the data which he called out. Bradley also undertook all of the calculations, for these are in his hand.
[123] Ibid. p.350.
[124] Rigaud, "Memoirs", p. iv.
[125] α Geminorum, or *Castor,* is a multiple star with six components. It appears as a bright visual binary. Both components are spectroscopic binaries. These four stars are orbited by an eclipsing binary system in a period of millions of years. The visual magnitude of *Castor* is 1.58.
[126] Philosophical Transactions. W. Herschel, "Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double-stars; with an Investigation of the Cause to which they are owing. Read June 9, 1803. pp. 339 – 382. p.363 gives a report of Bradley's observations of *Castor* and *Pollux.* The originals are in Bradley MS 21.
[127] Rigaud, "Memoirs", p. iv.
[128] Philosophical Transactions. Pound, "New and Accurate Tables for the ready Computing of the Eclipses of the first Satellite of Jupiter, by Addition only – refers to Cassini's curious tables". Vol. 30, pp. 1021 – 1034.
[129] Bradley and Halley, *Tables of Jupiter's Satellites,* London, 1752.
[130] Rigaud gives reference to this in the "Memoirs", p. v. Rigaud wrote, "More justice was offered by Bailli who in the *Essai sur la Theorie des satellites de Jupiter* of 1766, p. xxxi, gave Bradley the credit for having detected the greater part of the inequalities which had been recognised in the motions of the satellites of Jupiter".
[131] Rigaud, "Memoirs", p.vi.
[132] There is a common misunderstanding that is often repeated in local and popular histories of Wanstead and north east London, that Bradley was employed as a curate under Pound. He lived with his uncle in Wanstead from 1711 until Pound's death in 1724. At no stage was he a curate in Wanstead, or indeed, in any other parish. Pound's account book, Bodleian Library, Bradley MS 23, shows no payments to Bradley as a stipendiary assistant.
[133] Rigaud, "Memoirs", p. vi.
[134] Jeremy Gregory and John Stevenson, *Britain in the Eighteenth Century, 1688 – 1820.* p. 237. John Robinson was in this office from 1714 until 1723.
[135] Benjamin Hoadly was one of the most controversial churchmen of early Hanoverian England. In 1716 he published his *Preservative against the Principles and Practices of the Nonjurors both in Church and State,* and on 31st March 1717 he delivered, as king's chaplain, a famous and controversial sermon, on the *Nature of the Kingdom or Church of Christ* in the presence of George I. The king had this printed by special command. The sermon caused great offence and led to vitriolic criticism from many clerics, including William Law. The sermon questioned the temporal authority of the church. The Convocations of Canterbury and York were both prorogued by George I in order to prevent their expected condemnation of Hoadly. Neither convocation was to meet formally until the mid-19th century. Hoadly upheld the cause of religious tolerance and the importance of individual conscience.
[136] Rigaud "Memoirs", p. vi.
[137] Benjamin Hoadly was a polemical Whig who entered into a dispute with Francis Atterbury, the leader of the Tory High Church party during the reign of Queen Anne. In 1710 Atterbury had defended Henry Sacheverell who was impeached by Parliament for opposing the principles of the

Glorious Revolution of 1688 – 89. After the Hanoverian succession Atterbury associated himself with the Jacobite cause and ended his days in exile.

[138] Samuel Clarke, *The Works of Samuel Clarke, DD,* ed. Benjamin Hoadly. 4 vols. London, 1738. Reprinted, London, 2002.

[139] *The Cambridge History of English and American Literature in 18 Volumes, Volume X. The Age of Johnson. 5, Hoadly and the Bangorian Controversy,* Cambridge, 1907 – 1921.

[140] Francis Atterbury went into exile in the service of James, the Old Pretender, in Paris.

[141] Rigaud, "Memoirs" p. vi, renders the name of this parish as Llan ddewi felfry.

[142] As George II he was to come to Bradley's aid later in his life, when he was Astronomer Royal.

[143] John Keill FRS was a leading supporter of Sir Isaac Newton, particularly in the dispute with Leibniz. Keill claimed that Leibniz had plagiarized Newton's method of fluxions, the calculus. In 1701 Keill had published the *Introductio ad Verum Physicam,* a useful introduction to Newtonian natural philosophy which was considered an excellent introduction to Newton's *Principia.* He died after a short illness on 31st August, 1721.

[144] Both men were associated with Pound's powerful circle of friends and patrons. The role of Pound in the advancement of Bradley's career was more than just astronomical. His celebrated generosity and conviviality was evidently used in the solicitation of powerful sources of influence and patronage which in turn Bradley was able to capitalise on.

[145] Elizabeth Pound, was a wealthy heiress, who additionally received a considerable fortune after her brother, Matthew Wymondesolde made a large fortune speculating in South Sea Company stock.

[146] Pound also deferred to those he considered might control his future preferment. Although he was a friend of both Newton and Halley, and was well aware of the antipathy which existed between them and Flamsteed, he agreed to translate Flamsteed's *Preface* from English into Latin. The technical content disqualified most people. In the correspondence which passed between Joseph Crosthwait and Abraham Sharp, *Royal Society Library, Sharp Letters, FISh,* xxiv, items 145 and 151, it is possible to see that Pound was working well on the preface and had produced 12 sheets of Latin translation when he stopped suddenly. The correspondence suggests that Halley or Newton had put pressure on him and that in deference to them he had put the translation aside.

[147] I have decided to call the loosely associated group of "scientific" workers the Wanstead Circle and not the Pound Circle, because the activities of the group continued after the death of James Pound on 16th November 1724, when Bradley joined Molyneux and Graham on the problem of observing annual parallax.

[148] Philosophical Transactions. No. 81, 25th March, 1672, pp 4004 – 4010. Included in I. Bernard Cohen, assisted by Robert E. Schofield, *Isaac Newton's Papers and Letters on Natural Philosophy and Related Documents,* Cambridge, 1958, "An Account of a New Catadioptrical Telescope invented by Mr. Newton", pp 60 – 67.

[149] Throughout much of the 1720s, Molyneux, John Hadley and Bradley made repeated experiments on the synthesis of new alloys to determine their reflective properties, with the object of producing efficient specula for reflecting telescopes.

[150] Henry C. King, *The History of the Telescope,* High Wycombe, 1955. p. 77.

[151] Philosophical Transactions, Hadley, "An Account of a Catadioptrick Telescope, made by John Hadley. With the Description of a Machine contriv'd by him for the applying it to use", Vol. 32, pp. 303 – 312.

[152] Rigaud, "Memoirs", p xi. Rigaud states that the Hadley reflector was retained by Bradley and that after his death in 1762 it passed to his youngest sister Rebecca. Rigaud did not state why the instrument passed to Rebecca or whether or not she had an interest in astronomy.

[153] Philosophical Transactions, Hadley, "An Account of a Catadioptrick Telescope, made by John Hadley. With the Description of a Machine contriv'd by him for the applying it to use", Vol. 32, p 303.

[154] Rigaud, "Memoirs", p ix.

[155] Philosophical Transactions, Hadley, "An Account of a Catadioptrick Telescope, made by John Hadley. With the Description of a Machine contriv'd by him for the applying it to use", Vol. 32, p. 304.

[156] Ibid. p. 304. Hadley reported that Samuel Molyneux had produced 150 different alloys for specula.
[157] Henry C. King, p. 82.
[158] Bradley, "Miscellaneous Works", *Miscellaneous Astronomical Observations.* p 355.
[159] Rigaud, "Memoirs", p ix, quotes William Derham, the preface to *Astro-Theology* as a source.
[160] Bodleian Library, Bradley MS 23, *James Pound's Account Book 1715 to 1724.* fol. 40v
[161] Most local historians have tended to locate the telescope on George Green, an area which still forms an extensive open space opposite the George Inn. Lacking firm documentary evidence there remains no conclusive way of confirming this conjecture. As the green was common land it may not have been an ideal site for astronomers to work with suspended telescopes. The nearby extensive grounds of Pound's patrons at Wanstead House may have offered more suitable areas for the purpose.
[162] Rigaud, "Memoirs", p ix, quotes Smith's *Optics,* p 892.
[163] Bradley, "Miscellaneous Works", *Miscellaneous Astronomical Observations.* p. 354.
[164] Rigaud, "Memoirs", p ix.
[165] Bradley, "Observations upon the Comet that appeared in the months of October, November, and December, 1723" in "Miscellaneous Works", pp. 42 - 48. Bradley states that Halley first observed it between 7 and 8 pm, on the 9th October, 1723.
[166] Bodleian Library, Bradley MS. 25, *Calculations and determinations of the orbital elements etc., of the Comets of 1723, 1742, 1748 and 1757.*
[167] Bradley, Bodleian Library, Bradley MS. 19, *Latin Lectures delivered at Oxford by James Bradley.*
[168] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. London, 1726. Book I, Proposition 19, Problem 11.
[169] Rigaud, "Memoirs", p. x.
[170] Bodleian Library, Bradley MS. 19, *Latin Lectures delivered at Oxford by James Bradley.* fol. 22r.
[171] Bodleian Library, Bradley MS. 25, *Calculations and determinations of the orbital elements etc., of the Comets of 1723, 1742, 1748 and 1757.* fols. 36 – 52.
[172] Charles Coulston Gillispie, with the collaboration of Robert Fox and Ivor Grattan-Guinness, *Pierre-Simon Laplace, 1749 – 1827, A Life in Exact Science,* Princeton, 1997, p 39.
[173] Pierre-Simon Laplace, *Théorie analytique des probabilities,* Paris, 1812, and *Essai philosophique sur les probabilities,* Paris, 1814. Laplace used his analyses of probability in order to calculate orbits with very few observations.
[174] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. 1726, Book 3, Proposition 42.
[175] Bodleian Library, Bradley MS 23, *James Pound's Account Book 1715 to 1724.* fol. 40v.
[176] Bradley, "Observations upon the Comet that appeared in the months of October, November, and December, 1723" in "Miscellaneous Works", pp. 42 - 48.
[177] Thomas Birch, *History of the Royal Society,* London, 1756, Vol. II, pp 98 & 109.
[178] Rigaud, "Memoirs", p x.
[179] Rigaud, "Memoirs", p xi.
[180] J-B-J Delambre, *Histoire de l'Astronomie au XVIII Siecle,* Paris, 1819, p 419.
[181] 'Espinasse, Margaret, *Robert Hooke,* London, 1956, pp 85 – 86. The rebuilding of London reached its highest levels of activity between 1668 and 1674.
[182] Bodleian Library, Bradley MS 20*, fol. 68. Bradley records the four observations made by Robert Hooke in pursuit of his measurement of the annual parallax of the circumpolar star, γ Draconis. These were made on the 6th and 9th July, 6th August and 21st October, 1669.
[183] The site of Gresham College is in Broad St, on the forecourt of the building formerly known as the NatWest Tower.
[184] Hooke, "An Attempt to Prove the Motion of the Earth from Observations", contained in R. T. Gunther, *Early Science in Oxford,* Vol. 8, pp 17 – 25. It is also to be found in *Royal Society Classified Papers,* 20, No. 61, ff. 129 – 136.
[185] Robert Hooke, "An Attempt to Prove the Motion of the Earth from Observations", contained in R. T. Gunther, "Early Science in Oxford", Vol. 8, p. 25.

[186] Hooke meant the angle subtended by the angle of the Earth's orbit, and not, as in modern practice the semi-major axis of the Earth's orbit. It is equivalent to an annual parallax of 14 or 15 seconds.

[187] John Roger Fisher, *James Bradley and the New Discovered Motion : The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars,* Imperial College, University of London, 1994.

[188] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.* fol. 68.

[189] Hooke, "An Attempt to Prove the Motion of the Earth from Observations", contained in R. T. Gunther, *Early Science in Oxford,* Vol. 8, pp 17 – 25. Hooke began to express doubts about the validity of his claim, which he appears to have abandoned within a few years. His observations ceased on 21st October, 1669 when the glass slipped and broke, making further observations impossible.

[190] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.* fol. 18r. See also John Roger Fisher, MSc Dissertation, Appendix 3, p. 101. This is the third unpublished version of Bradley's aberration paper, which was obviously written after the published version. I am certain that Bradley almost certainly meant it to be the final or definitive account but it was evidently overtaken by events, an intermediary account being published.

[191]Bodleian Library, Bradley MS 23, *James Pound's Account Book 1715 – 1724.* There are various entries in Pound's account book, which all itemise expenses which stemmed from regular visits to Kew.

[192] George Graham FRS 1674 - 1751, was the inventor of the dead-beat escapement, and the leading clockmaker in London. He succeeded to Thomas Tompion's business after marrying his daughter and perfected Tompion's cylinder escapement. He made several important astronomical instruments for Halley and Bradley, and partnered Pound, Molyneux and Bradley in their astronomical work.

[193] It was no longer urgent because the overwhelming majority of astronomers firmly accepted the heliocentric model. Many also accepted Newton's contention, based on photometric considerations that the stars were at considerable distances from the solar system with annual parallaxes under a second of arc.

[194] Gian Domenico Cassini and Eustachio Manfredi were two significant astronomers who resisted the heliocentric theory. Cassini remained a conservative astronomer and resisted the growing acceptance of heliocentricism amongst his colleagues at the Royal Observatory at Paris. Manfredi was the principal of the Papal College in Bologna. Roman Catholic doctrine, following the conclusions of the Tridentine Council from 1545 to around 1565, largely bowed to the teachings of the Church Fathers. This incidentally included an acceptance of geocentricism by default. The Holy Office resisted the acceptance of heliocentricism because it implied that the Church Fathers were in error with an implication that many other doctrines of the faith may also be in error. Whether Cassini and Manfredi opposed heliocentric theories on religious or on scientific grounds is open to debate.

[195] Pound, who had supported Bradley through the years of his youth and education, was a wealthy man, being supported by two lucrative livings and his second wife's inheritance. He appears to have died intestate with far reaching consequences for his nephew who for a few years remained partially dependent on the support and goodwill of Elizabeth Pound.

[196] Bodleian Library, Bradley MS 38, Molyneux's original draft, *A description of an instrument set up at Kew,* and Bradley, Bodleian Library, Bradley MS 39, James Bradley's copy of Samuel Molyneux's draft.

# Chapter Two.

# Kew and Wanstead: The Work on Aberration.

## The Problem of Annual Stellar Parallax.

The unexpected death of Pound in 1724 briefly delayed the setting up of the zenith sector at Kew, but Bradley was soon to take an even more active interest in the project.[1] The problem of the measurement of annual parallax remained active after the failures of several claims, for most astronomers were committed to heliocentric models.[2] The continued failure to measure annual parallax remained a source of apparent evidence in favour of the small but persistent community of astronomers who opposed the Copernican thesis.[3] The commonly held belief amongst astronomers that the stars were so distant that the measurement of annual parallax was beyond expectation, begged accusations from those opposed to the heliocentric world view, that such a perverse universe would not have been created by God, for it appeared to contradict the principle of sufficient reason.[4]

Newton argued decisively in favour of the idea that even the nearest stars were set at enormous distances, which he estimated to be a million astronomical units,[5] equivalent to several light-years. In support of his view Newton used a photometric method initially devised by James Gregory in his *Geometriæ pars universalis.*[6] Newton made two presumptions, first that the stars possessed the same intrinsic luminosity as the sun, and second, that light diminished as to the square of the distance. Given these, the apparent brightness of a star is a function of its distance and the brightest stars are the nearest to the earth. Newton estimated the comparative brightness of the sun by looking at its disc through a pinhole which allowed him to form an image with the estimated brightness of *Sirius*.[7] Newton then calculated the distance of the Dog Star. He then estimated that it possessed an annual parallax of about half a second of arc, far below the capacity of contemporary optical technology to resolve.[8]

Newton expressed the view that the observed stability of the solar system was a function of the immense distances of the stars. In Book III of the *Principia*[9] He wrote, "The aphelia and nodes of the [planetary] orbits are at rest." This leads to two corollaries, "The fixed stars are also at rest, because they maintain given positions with respect to the aphelia and nodes." And he adds,

> And so, since the fixed stars have no sensible parallax arising from the annual motion of the earth, their forces will produce no sensible effects in the region of our system, because of the immense distance of these bodies from us. Indeed, the fixed stars, being equally dispersed in all parts of the heavens, by their contrary attractions annul their mutual forces, by book 1, prop. 70.[10]

How Newton was able to reconcile the first corollary that the fixed stars were at rest with Halley's implied discovery of the proper motion of several bright stars is not even hinted at even in the 3rd edition of the *Principia*.[11]

In contradiction to the estimates made by Newton that the stars were at enormous distances from the solar system was a tradition of claims made by astronomers for the discovery of annual stellar parallax with implied annual shifts of 30 or 40 arc seconds.[12] These measured shifts were treated differently by astronomers committed variously to geocentric or heliocentric models. Geocentric observers tended to categorise the motions as "aberrations" or "anomalies". In many cases observational or systemic errors were often suspected.[13] On the other hand heliocentric astronomers often attempted to categorise the motions as evidence of annual stellar parallax, and these claims were usually challenged by astronomers who still upheld the geocentric cosmos, or heliocentrically inclined astronomers who were not convinced by the validity of the claim. Jean Picard, Gian Domenico Cassini (Cassini I) and John Flamsteed all observed implied motions of the pole star, *Polaris*.[14]

Picard, who was professor of astronomy at the Collège de France in Paris, was the first astronomer to accurately measure the length of a degree of meridian, and compute the size of the Earth.[15] In 1671, after Bartholinus sent him an exact copy of Tycho's observations from Copenhagen, Picard travelled with him to the



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long abandoned site of Tycho Brahe's observatory on the Danish island of Hven,[16] in order to determine its exact location so Tycho's observations could be precisely compared with those made elsewhere.[17] In comparing the observed motion of Polaris made by Tycho and himself, Picard was led to the conclusion that the pole star was not "fixed". The star appeared to wander. Picard was a precise and systematic observer[18] and he recognised that the implied motions did not conform to the theory of annual parallax. Picard was a priest and prior of Rillé in Anjou, a pupil of Gassendi and although educated at the Jesuit College at La Flêche, he certainly appears to have been inclined to favour the heliocentric model. In spite of this he was too astute an astronomer to claim the motions as evidence of annual stellar parallax. The author of the article entitled *Jean Picard*, in the Catholic Encyclopedia suggests that he had all the evidence necessary to have discovered annual stellar aberration.[19] However, as this thesis will hopefully demonstrate, the construction of the theory of aberration required far more evidence than Picard possessed.[20]

In addition to Hooke, who published the only account of a genuine *attempt* to measure annual stellar parallax in the 17th century,[21] one other notable English astronomer seriously examined the problem, and even claimed to have observed the phenomenon in his observations of the motions of *Polaris*.[22] The work is referred to in Bradley's published paper on the "new discovered motion" in January 1729.[23] Flamsteed claimed to have measured an annual parallax of 40" for the pole star, a motion close to that later observed by Bradley[24] so he used it as supporting evidence for his own work on the "new discovered motion", the phenomenon later referred to as annual stellar aberration. Flamsteed's claim was energetically challenged by Jacques Cassini (Cassini II).[25] In the face of criticism in general, and Cassini's in particular, Flamsteed withdrew his claim in a letter addressed to Sir Christopher Wren.[26]

As I will later discuss, Bradley systematically rectified his instruments, and carefully accounted for any possible sources of error. This contrasts with Flamsteed's earlier observational practice as witnessed by his letter to Wren.

> I shall return to my stock of night observations, to seek out such as are most proper for discovering the Errors of the Instrument; afterwards those that are most convenient for showing the parallaxes of the Orb.[27]

After the publication of Bradley's paper on the new discovered motion it was recognised that Flamsteed's observations were also of the phenomenon that was to be identified as "annual stellar aberration" or as the "aberration of light". Certainly Bradley recognised this and included Flamsteed's claims in his own account of his observations of the new discovered motion. All of the work undertaken by various astronomers through the late 17th and early 18th centuries seemed to suggest that the so-called "fixed stars" were not so fixed as had traditionally been supposed, nor as fixed as Newton had implied.[28]

From 1669 onwards astronomers had been observing regular motions of the fixed stars and while some made claims for the observation of annual stellar parallax most of the observations were accepted as observational or systemic anomalies. I have already referred to the significant but small community of astronomers that remained committed to a geocentric cosmos. Eustachio Manfredi was one such "anti-Copernican" who from 1707 observed and analysed the various stellar motions which many astronomers had claimed as evidence of annual stellar parallax.[29] These motions were described by Manfredi as "aberrations" from the Latin *aberrare*, a deviation from the normal state. The etymology of the word refers to moral failure. According to Aristotle any imperfection of the heavens implied an ethical shortcoming, for the motions of the heavenly bodies were guided by their souls.[30]

Manfredi observed these aberrations in right ascensions from 1707 to 1721, always with an assistant.[31] His conclusion was that all of the various claims for the discovery of annual stellar parallax were completely fallacious. It is an irony that Bradley would have agreed entirely with this conclusion, even though he was an orthodox Newtonian.[32] The work of Manfredi and Bradley was entirely complementary, and the evidence they acquired was capable of being used by each man in support of their own fundamental beliefs. It is a classic example of two scientists acquiring mutually supportive empirical evidence to argue for

almost diametrically opposed conclusions.[33] The work of many astronomers prior to Bradley, and the theory proposed by Manfredi, serve to emphasise the contention that the aberration of light is not an empirical "discovery" so much as a conjecture constructed on working practices.[34] Put quite simply, the phenomenon that was identified by Bradley had been observed consistently for at least 60 years by many astronomers. Prior to Bradley's work not a single astronomer had once claimed the observed motions as anything other than a mistaken claim for annual stellar parallax, or simply as anomalous or aberrant observations. Bradley's work was the product of a thoroughly ingrained Newtonian practice, the result of being trained in the context of a scientific community with shared values and expectations that included the presupposition that empirical observations were capable of eliciting laws of nature, though Bradley was perceptive enough to recognise that such observations were not in themselves sufficient to *establish* such laws.[35]

In the introduction to *De Annuis Inerrantium Stellarum Aberrationibus*[36] Manfredi stated that his investigations had successfully challenged the Copernican theory. He wrote,

> There are some however who, noticing that these strayings of stars recur at fixed times of the year, think there is no doubt the matter has to do with an annual movement which with Copernicus they attribute to the earth, and so they proclaim that a true universal system is disclosed and demonstrated by this very piece of evidence, in which however it is easy to be deceived, since the Copernican system is tottering together with the arguments by which they think it is supported, as I am confident that I shall show in this very work.[37]

In the penultimate and eighth chapter[38] Manfredi examined observations of aberrations in declination. Through the agency of Flamsteed's letter to Wallis,[39] Manfredi acquired the data of Hooke's four observations of 1669.[40] He conceded that Hooke's results did not obviously contradict the theory of annual parallax although with so few observations any firm conclusions were difficult to sustain.[41]

So Manfredi calculated how far away γ Draconis would be situated if Hooke's claim were to be substantiated, although he did not accept the veracity of it.[42]

> From this also the distance of the star from the sun, or the half diameter of the sphere in which the star moves will be laboriously worked out, if, like a 15 scruple arc to a radius, a half-diameter of the annual orbit is made to the distance sought, which therefore be of 13,755 half-diameters of this kind.[43]

Manfredi also examined other claims, including that made by Flamsteed following his observations of *Polaris* in 1696.[44] He asserted,

> in the observation of July 14th or 16th, the actual direction of the aberration is at variance with annual parallaxes, since from that day to December 2nd, the star moved according to the observations 48" to the north … So it seems that a reason must be sought for the movements of this star other than the annual rotation of the earth around the sun.[45]

Manfredi never determined any possible cause for the motions he observed but his object was to falsify the claims for annual parallax. He observed the annual aberrations of many so-called "fixed stars" at various ecliptical latitudes which ironically, given Manfredi's geocentric stance, were later used as corroborative evidence in support of Bradley's hypothesis of the new discovered motion. It was in the final chapter of his thesis that Manfredi discussed the results of his investigations of annual aberrations in right ascension.

> Rather more laborious is the consideration of those aberrations which happen to the fixed stars in ascension, since of course they hardly maintain the same straight ascension or arc of distance from the colures,[46] at all, but wander somewhat to the east or west. I am sure that this was noticed by no astronomer before the celebrated Maraldus,[47] who comparing the times at which *Sirius* and *Arcturus* cross the meridian, found a varying difference between those times on various days of the year, and warned me about this in his letter in the year 1707, he also mentioned that he had observed wanderings of this kind not only in the case of these stars, but in others

besides, which first induced me to attempt the same investigation at the Marsilian Observatory with Stancarius, and in this public observatory of the Institute of Sciences.[48]

Most astronomers had mistakenly perceived the observations as evidence of annual parallax, or as anomalies. Manfredi examined the various claims and felt very confident that none of them were sustainable.[49] His conclusions were constructed on a methodology much favoured by Jesuit natural philosophers who "took scientific knowledge to be fundamentally public".[50] Manfredi's method lent much to this Jesuit approach. He analysed the geometry of annual stellar parallax and determined its empirical consequences before observing many "aberrations" to determine whether or not they conformed to the predictions of theory. In obtaining a null result Manfredi then interpreted it as evidence of the falsity of the doctrine of annual stellar parallax. Peter Dear argued that "the point of Aristotelian scientific demonstration was *to derive conclusions deductively from premises that were already accepted as certain*"[51] (my italics). According to Dear this Aristotelian approach determined that "proper role for experience was to ground the assertions contained in the original premises."[52] Manfredi's initial premise was that the doctrine of annual stellar parallax was false. His conclusions were deducible from his premises.

**The Kew Experiment.**

Given the sorry history of attempts to measure annual stellar parallax Molyneux, Bradley and Graham must have given extensive thought about their proposed practice and methodology. When they began to undertake their well prepared attempt to measure the annual stellar parallax of the near zenith, second magnitude star γ Draconis,[53] they utilised the method that had been devised by Wren and Hooke, and used by Hooke at Gresham College in 1669.[54] The method avoided the problems associated with atmospheric refraction and promised to offer the best possible results for any attempt to measure minute motions. In his account Bradley wrote,

> Before I proceed to give you the history of the observations themselves, it may be proper to let you know, that they were at first begun in hopes of verifying and confirming those that Dr. Hooke formerly communicated to the public; which seemed to be attended with circumstances that promised greater exactness in them, than could be expected in any other that had been made and published on the same account.[55]

The account is not as transparent as it may first appear. Bradley's practice was centred on Newtonian models, with an emphasis on rigorous precision and extreme accuracy.[56] When Bradley stated that the object of the investigation was to verify and confirm Hooke's observations a problem immediately asserts itself on any historian who recognises the perversity of the statement. Hooke had claimed that he had discovered annual stellar parallax on the basis of four observations over a period of three and a half months.[57] This was hardly an investigation that would have impressed an astronomer noted for his reiterative, precise and accurate observational methods. There is a further perversity in the account that suggests that Bradley is not as candid as is suggested. As Manfredi's work reveals, acceptance of Hooke's claim would suggest that γ Draconis, a second magnitude star, was only about 13,755 astronomical units (AU) away from the solar system.[58] The contemporary commonly held presumption that the brightness of a star was a function of its distance meant that if a second magnitude star was less than 14,000 AU away, then the distance of the brightest star *Sirius* would be just over 4,000 AU away. Bradley was aware of Newton's concern that the nearest stars were at immense distances with annual parallaxes that were beyond the capacity of contemporary technology to measure. A confirmation of Hooke's *results*, with the inference that γ Draconis was less than 14,000 AU away would have posed a real problem for Newtonian theory.[59]

There is evidence that Bradley had reflected on this problem, and that he had seriously entertained the possibility of the stars being much closer to the solar system than Newton had ever dared to admit. He was prepared to accept that if Hooke's result was confirmed, it was possible to reconcile Newtonian theory with interstellar distances of the order of 10,000 AU. Bradley was familiar with

Halley's work on implied proper motions, when he compared the positions of a few bright stars with their recorded positions in Ptolemy's *Almagest*, and this suggested to his mind at least, the possibility that if stars moved perceptively in under 2,000 years then Newton's estimates could be queried. Newton was concerned with the long-term stability of the solar system if the stars were too close, but Bradley seems to have accepted Newton's own reasoning to support the contention that the stars may be comparatively close. Newton himself wrote, "the fixed stars, being equally dispersed in all parts of the heavens, by their *contrary attractions* (my italics) annul their mutual forces, by book I, prop. 70."[60] There is every reason to be sceptical about Bradley's apparent reasoning, for given the apparent evidence that the stars possessed proper motions the notion that the stars were separated by distances of about 10,000 AU made the long term gravitational stability of any planetary system questionable.

This raises the question about why Bradley, a thoroughgoing Newtonian, would even accept the conjecture that the stars were so close, given the difficulties that this raised for the Newtonian position. The prime motivator of the experiment at Kew was Molyneux, and it was he who commissioned Graham to construct the zenith sector. Bradley was the junior partner, invited after the death of Pound because of his outstanding observational abilities. Molyneux wrote an account of the instrument which was seemingly intended for publication.[61] This version of the account was published by Rigaud and can be found in the first part of *The Kew Observation Book.*[62] It is the last of three versions. The first is roughly written out in Molyneux's hand, whilst the second is in an unknown hand. It is neither Bradley's nor Graham's.[63] The instrument was set up at Molyneux's residence on 26$^{th}$ November, 1725.[64] No observable motion of the object star, $\gamma$ Draconis was expected at this time of the year so the next few days were devoted to adjusting the instrument.

Unlike Manfredi, who sought to detect "aberrations" in right ascension by timing the passage of various object stars through the central vertical hairline in the focus of the telescope, a difficult procedure even with excellent timepieces,[65] Molyneux, Bradley and Graham sought to detect annual parallax in declination.[66]

The observation of motion in declination is much more efficient and precise, because as the object star enters the field of view the observer can make fine adjustments so that any measurements made as the star passes through the central vertical line of the micrometer are far less time critical. Observation in declination enables accurate calibration of the position of the object star as it passes through the observational field. Bradley used an array of hairlines above and below the central horizontal line, as well as either side of the central vertical line.[67] This enabled Bradley and his partners to observe the passage of the star with great confidence as they evaluated the distance the star had shifted north or south with each succeeding passage through the telescopic field. Unlike the observations in right ascension which required precise timing, the declination method devised originally by Hooke and Wren allowed the telescope and the sight to be adjusted as the star entered the field of view. The turns of the micrometer screw helped to determine the shift in declination as it passed along the hairline.[68]

The star *Eltanen,*[69] designated γ Draconis, passes through the southern meridian at noon in December. On the 3rd December, 1725 its position in declination was noted by Molyneux.[70] Rigaud wrote in the *Memoirs of Bradley*, "The motion which might be produced in it by parallax towards the pole, would at this time have been very gradual."[71] Observations of the object star were repeated on the 5th, 11th and the 12th of December, in order to verify its position in declination.[72] On the 17th of December, Bradley repeated the procedure. He observed its passage through the field of view and perceived that the star had passed "sensibly" south of its former position. His initial response was that this was an anomaly as no sensible motion was to be expected at this time of the year. Bradley characteristically examined the equipment with great attention.[73] Molyneux recorded the incident in the *Kew Observation Book.*[74] He wrote,

> This day, Mr Bradley, one of the astronomy professors at Oxford, who had been acquainted with the whole contrivance of this instrument, and was present when it was first set up in its place, did with great care examine

the situation thereof. He found the index at 10.3, where it had been left on the 12th of December, and adjusting the mark to the plumbline, about 11h. 15' mean time, he found the index stood at 9.7, so that from the 12th of December to the 17th, it had been altered but a little more than ½ a second.[75]

This significance of this passage reveals that Molyneux regarded Bradley as a junior, though respected partner. In any discussion of the Kew experiment, it must be concluded that Molyneux was the prime mover, and any discussion of its possible motives must recognise that they were primarily those of Molyneux himself.[76] It also reveals the extraordinary precision of Bradley's work and the accuracy of Graham's instrument. Bradley perceived a motion of 0.6" at a time when other observers, including Halley, did not believe that any motion much less than 5" could be firmly established.[77] There is a revealing passage which was included in the *Miscellaneous Works* by Rigaud. In the *Zenith Observations at Wanstead*[78] Bradley recorded Halley's observations with the new zenith sector made for Bradley by Graham and suspended in Wanstead. The entry for 2nd September 1727 states,

> Dr. Halley observed *Capella*;[79] The star fluttered much, and he said that as it went out[80] It appeared to be more southerly in reality than the thread; he therefore fancied from this observation that the direction of the thread was not right; but as all other stars going out appear on the other side of the wire, this appearance must be owing to his not being able to bisect the star at the cross, because of its fluttering.[81]

With reference to this passage Rigaud commented,

> Though Halley's was certainly a very powerful mind, it was not always free from error; for he not only had too much contempt for what appeared to him to be trifles, but he was often too rapid in drawing his conclusions.[82]

One important consequence of Halley's practice was the maintenance of his viewpoint that any observations of motions of much less than five arc seconds were highly suspect. Halley never accepted Bradley's observations of the nutation

for instance, considering observations of motions of under a second of arc to be unascertainable. In December 1725 Bradley continued to adjust the instrument at Kew and was satisfied that it could be rectified to within half a second of arc.[83] The declination of the star was perceptively varying at a rate of about a second every three days[84] which was entirely unexpected and unattributable. On the 20th of December according to his account,[85] and the 21st of December according to the *Kew Observation Book*,[86] Bradley discovered that star had passed even more southerly. The discrepancy between these dates is easy to explain. The star had passed conjunction on the intervening days between the 17th and the 20th of December. Civil dates are reckoned from midnight, whilst astronomical dates from noon. γ Draconis having passed conjunction now passed through the meridian after noon on the 20th December according to the civil date, and the 21st according to the astronomical calendar. Rigaud referred to this observation as ***decisive*** [87] and it fitted the view that the discovery of the aberration of light was an empirical achievement.[88] It is a view that was supported by K. V. Hewitt.[89] Yet the observation was merely an anomaly, one of thousands that had been noted for decades, indeed from the inception of the use of the astronomical micrometer when small changes in location could be observed.

Because Rigaud perceived the discovery of aberration in empirical terms he felt obliged to defend Bradley's claim against the earlier observations of the phenomenon by Jean Picard. This particular claim was made on behalf of the French astronomer by Jean-Baptiste-Joseph Delambre in his *Histoire de l'Astronomie au 18e Siècle.*[90] Rigaud continued,

> Picard was the first to bring forward an undeniable objection to Hooke's supposed discovery of the parallax of the fixed stars. He also detected some extraordinary annual motion of the pole star, during a portion of the year when it could not be accounted for either by parallax or by refraction; but instead of his observations having given the direction to Bradley's, or having formed the foundation of his discoveries, it was by those very discoveries (which were necessary to explain them) that they were recalled into notice, and that their value was established.[91]

In other words, Picard's observations were only significant in the light of Bradley's later work. In the history of science there are many instances of anomalies that were only to achieve significance in the context of the construction of a new theoretical framework. Picard's observations, along with many others made by other astronomers, including those made by Bradley on the 17th and the 20th December 1725, were *anomalies*. They could not be validated by reference to any known phenomenon, either annual parallax or atmospheric refraction, or indeed any other. The observation of the 20th of December was not decisive. It was merely puzzling.[92]

It was only after a year of observation at Kew, and a further year at Wanstead with a different zenith sector designed on different principles, that Bradley was in a position to propose his hypothesis of the new discovered motion, and to express its parameters. He had to construct a new theory, to distinguish the motion from that of annual parallax. The theory was a product of Bradley's astronomical practice, itself rooted in a firmly constructed Newtonian methodology which emphasised rigour, reiteration, precision and accuracy. It was to be a singular achievement, but it was validated within the context of the shared values of a scientific community. It was these shared values and assumptions that led to the rapid acceptance of his aberration paper,[93] initially in England, but also in France.

As Delambre had stated, Picard had indicated the shortcomings of Hooke's claims for the discovery of annual parallax.[94] And in addition, I have already expressed the opinion that I find it difficult to accept that Bradley really believed that Hooke's results were in any way acceptable, given his own stated commitment to reiterative rigour, as well as the precision and accuracy of all of his mature observational work, let alone the implications the validity of Hooke's results would have had for Newtonian theory. Yet Bradley specifically stated,

> Before I proceed to give you the history of the observations themselves, it may be proper to let you know, that they were at first begun in hopes of verifying and confirming those that Dr. Hooke formerly communicated to the public; *which seemed to be attended with circumstances that promised*

*greater exactness in them*, (my italics) than could be expected in any other that had been made and published on the same account.[95]

Bradley wrote his accounts[96] during the second half of December 1728, after the sudden demise of his erstwhile partner and friend Molyneux who died on 13th April, 1728 of what was described as a brain seizure.[97] In my earlier MSc dissertation[98] I expressed the opinion that Molyneux was motivated by anti-Newtonian sentiments. I think there is sound documentary and circumstantial evidence to support this view. Like his former teacher and lifelong friend George Berkeley,[99] he may have been critical of some of Newton's claims, though unfortunately I have been unable to find any documents or correspondence to support this view. Berkeley was highly critical of some of the theological implications of Newton's conceptions of absolute space, time and motion. He wrote,

> I must confess it does not appear to me that there can be any motion other than *relative*, so that to conceive motion, there must be at least conceived two bodies; [thus] we are freed from that dangerous dilemma, of thinking either Real Space is God, or else that there is something else besides God which is eternal, uncreated, infinite, indivisible, unmutable. Both of which may justly be thought pernicious and absurd notions.[100]

The anti-Newtonian aspects of the Kew experiment are quite pronounced. Bradley believed he had to justify his work and present it in terms that deflected any suspicion that he was initially associated with work that might have been regarded as inimical to the Newtonian programme to which he was so committed. Molyneux was wealthy and well connected a successful politician and the personal secretary of the Prince of Wales.[101] Bradley, after the death intestate of Pound was left in a vulnerable position,[102] in which his future ambitions in society were dependant on his cultivation of the right political connections. His account of the work on the new discovered motion is a subtle recasting, that presents all of his work as a valid and disinterested programme of research conducted in the contexts of Newtonian method and practice.

The contrast between Bradley and Molyneux was equally marked in their approach to their chosen interest. Bradley was thorough and systematic, and as his later work on the nutation would display, extraordinarily patient. Molyneux was by comparison impetuous and impatient, and this was witnessed in his premature approach to Newton through John Conduitt.[103] After the 20th December 1725 the observational programme continued, though the partners were evidently puzzled as to the causes of the anomalous motion of γ Draconis. The star continued moving south in declination up to the beginning of March 1726 when it had shifted 20".[104] By the middle of April 1726 the direction of motion had begun to reverse and the star moved north in declination,[105] and by the beginning of June it passed at the same distance from the zenith as it had in December.[106] It continued moving to the north until September when it began to move back to the south.[107] By September it was 39" further north than it had been in March. In December 1726 it had returned to the same position as the previous year. The evident predictability of the motion of γ Draconis suggested that there was a regular cause, but Bradley and Molyneux knew it could not be annual parallax for the phenomenon was completely irreconcilable with parallax theory.[108]

During the year from December 1725 to December 1726, various conjectures were proposed by the partners concerning the possible causes of the phenomenon they were observing. Bradley's account in the version published in January 1729 by the Royal Society,[109]reveals that one of the conjectures made by the partners was that the motion was due to a nutation of the earth's axis. It stated,

> A nutation of the earth's axis was one of the things which first offered itself upon this occasion, *but it was soon shown to be insufficient*; (my italics) for though it might have accounted for the change in declination in γ Draconis, yet it would not at the same time agree with the phenomena in other stars, particularly in a small one almost opposite in right ascension to γ Draconis, at about the same distance from the north pole of the equator: for though this star seemed to move the same way as a nutation of the earth's axis would have made it, yet, it changing its declination but about

half as much as γ Draconis, in the same time, (as appeared upon comparing the observations of both made upon the same days, at different seasons of the year,) this plainly proved that the apparent motion of the stars was not occasioned by a real nutation, since, if that had been the case, the alteration would have been equal.[110]

The problem presented to Bradley was that in order to determine whether or not the observed motion was due to an annual nutation it was necessary to observe another star, preferably separated by 12 hours in right ascension.[111] The star chosen was severely constrained by the design of the zenith sector constructed by Graham for Molyneux,[112] for it was only capable of being shifted about 8 or 9 minutes of arc from the zenith,[113] and this limited the choice of suitable stars capable of being observed. It had to be bright enough to be observable in daylight. By the 3rd of January 1725/26 OS Bradley, Molyneux and Graham were seeking a suitable control star.[114] They found a star which they called *Telescopica in Auriga* which Bradley identified with star 35 in Flamsteed's Catalogue in the constellation of *Camelopardalis*. For a while, between the death of Bradley and the early 19th century, there was doubt about the identity of this star, for Flamsteed only gave its declination. The exact position of the star was indicated by Bessel in 1818.[115] The star was commonly referred to by Bradley and Molyneux as *Anti-Draco*.[116]

The control star was observed on the 7th January, and on the 3rd, 12th and the 20th of February 1725/26 OS. Its motion was consistently northward, while γ Draconis was moving south, but while the motion of *Anti-Draco* between the 7th of January and the 20th of February was 5" the opposite motion of γ Draconis between the 3rd of January and the 13th of February was 9.1".[117] The annual nutation hypothesis must have been short lived, though both stars were observed through much of 1726.[118] Given that the nutation hypothesis could not have been seriously maintained for more than a couple of weeks, for the motion of 35 Camelopardalis was soon evidently incompatible with the hypothesis, so the reaction of Molyneux is very difficult to understand unless it may be accepted as

evidence that he had made a very early approach to Newton because he was so opposed to his ideas.

An annual nutation of the order of 40" was simply irreconcilable with Newtonian theory.[119] Bradley would certainly have treated the hypothesis with reticence and caution, and would have preferred to have observed the motion of both stars for several months before making any pronouncement on the subject. By contrast, Molyneux made an ill-judged and premature approach to Newton through Conduitt in order to inform him that he, Graham and Bradley had discovered a

> certain nutation in the earth which they could not account for and which Molyneux told me *he thought destroyed entirely the Newtonian System* and therefore he was under the greatest difficulty how to break it to Sir Isaac.[120]

Throughout the Kew experiment there remains a suspicion that Molyneux was working to a different agenda from that of Bradley and Graham. There is no doubt that Molyneux respected and liked Bradley, for this is evidenced by his patronage and support when he interceded with the Prince of Wales in order to obtain a living for Bradley in Pembrokeshire.[121] Yet Molyneux was the prime motivator of the Kew experiment and the attempt to repeat Hooke's result of 1669, "in hopes of verifying and confirming those that Dr. Hooke formerly communicated to the public."[122] If an annual nutation of some 40"was inimical to the validity of Newtonian theory, then I have already intimated that any confirmation of Hooke's *result* must surely have been regarded as an equal threat to the Newtonian system.[123] It is reasonable to assume that the Kew experiment was driven by Molyneux at least in part to cause discomfort for the followers of Newton.[124]

A positive result would have posed problems for Newtonian physics. Bradley evidently thought there was a problem because he appears to have attempted to reconcile such a result with the validity of Newtonian theory.[125] The apparently premature approach by Molyneux to Newton can, in the light of these other factors, be accepted as further circumstantial evidence of a distinct antipathy to Newtonian theory.[126] There was a further difficulty with the hypothesis of the

annual nutation. According to Rigaud there was a paper presumably in Bradley's hand but endorsed by Molyneux with the words "Mr. Bradley, July 2, 1726, Nutation of the earth's axis."[127] I have not been able to trace this paper, even after examining the Molyneux archive.[128] If such a paper existed then it is extraordinary that Bradley should write it when all possible evidence for the nutation disappeared in February. Perhaps it was endorsed by Molyneux several months after it was written, but judgement must be suspended until the document is located and examined.

The approach to Newton would certainly not have received any encouragement from Bradley. Even if observational evidence had appeared reconcilable with such an annual nutation, his methodology was such that he would have suppressed any expression of it until he had fully confirmed the motion and ruled out all other possible explanations of the phenomenon.[129] Molyneux's approach was extremely devious. He was a successful politician. He was eager to inform Newton of the "nutation" and yet he attempted to display a reticence. Much of the circumstantial evidence of Molyneux's motives suggests an undue eagerness to cause Newton some degree of distress, and yet as Conduitt records the event Molyneux "was under the greatest difficulty how to break it to Sir Isaac."[130]

The Kew experiment had been set up in order to confirm Hooke's *result*, and the instrument had been designed with this specific task in mind. The zenith sector had been designed only to observe stars at the zenith, or at least within 5 or 6 minutes of it. This led to great difficulties when the partners sought a control star in order to test the hypothesis of the annual nutation. The experiment had been set up in order to test a presumption,[131] and when the observations ran counter to expectations, all manner of conjectures were proposed to explain the anomalous behaviour of $\gamma$ Draconis and the control star, *Anti-Draco.* After the abandonment of the nutation hypothesis a new one was proposed. This conjecture was even more inimical to Newtonian theory than either a confirmation of Hooke's result or an annual nutation of 40 arc seconds.

What was proposed was nothing less than a hypothesis with strong Cartesian associations. It envisaged that the motion was due to refraction, due to the earth moving through an extensive ether. In order to account for an annual motion of 40 arc seconds this ether would affect the Earth and the other planets in their passage around the Sun. Anyone who was familiar with the contents of Book 2 of the *Principia* would be hard pressed to make any such suggestion.[132] According to Rigaud this hypothesis was dated 18th June, 1726[133] as evidenced by Molyneux's endorsement of the paper, though again I have not yet been able to locate it even after an extensive search.[134] It is dated two weeks before the paper on the nutation, and yet it is difficult to envisage that it was proposed prior to the nutation hypothesis, which was being projected within two months of the initial observations.[135] Yet again, the effects would be expected to affect all places on the earth's surface more or less equally. It is difficult to understand how it would account for the differences in the relative motions of γ Draconis and *Anti-Draco*. I cannot even begin to conceive how a knowledgeable Newtonian like Bradley would have entertained the idea for a moment, unless he felt an obligation. This strange hypothesis was rapidly abandoned.[136] According to Rigaud's account in the *Memoirs of Bradley,* Molyneux ended his paper on refraction, "I have wrote down this hypothesis this day, not as a thing which I look upon as certain and demonstrative, but barely as a probable conjecture, to be examined by further observations."[137]

Rigaud asserted that another paper was written giving reasons for the final abandonment of both hypotheses, possibly as late as September or even November 1726.[138] Rigaud mentions that the partners, recognising that the two stars were moving very differently raised other possibilities. In Rigaud's words,

> It had been found that the quantity of the motion in declination was somehow connected with the latitude of the star. The precise nature of this connection does not yet appear to have been distinctly made out, but it was now clearly seen, that the few stars which passed within the narrow range of Molyneux's instrument were insufficient for the complete examination of the question.[139]

It must have been obvious to Bradley that well constructed though the zenith sector was the instrument was inadequate because it could not be directed very far from the zenith. Even before the end of December 1725 Bradley must have sensed that the entire experimental set-up was inadequate.[140] A zenith sector is really a modified transit instrument,[141] and it must have seemed obvious to Bradley that a new instrument was required, an instrument that combined the advantages of observation at or near the zenith, with the adaptability of a transit instrument, capable of observing many hundreds of bright stars as they passed through the meridian. Given Bradley's observational practice, which emphasised reiteration and precise analysis, he must soon have believed that he required a sector which could be adjusted to be directed several degrees from the zenith, in order to observe many bright circumpolar stars.

## The Wanstead Programme.

### Preparations for the Programme.

On the19th of August 1727, a new zenith sector, designed and constructed by George Graham, was set up at Elizabeth Pound's house at Wanstead.[142] Bradley acquired a new object glass for the instrument early in 1727,[143] and finished centring it early in April. He wrote,

> I likewise tried by looking at a fixed object through the 12½ feet glass with my eyeglass of 1¾ inch, to what certainty I could bring the thread of the micrometer to same object; and by many repeated trials, I found that the extremes did not differ above one division or 1/40 of a revolution of the screw, and therefore we may suppose that the eye can distinguish about half that quantity, or about 1/80 of a revolution through that eyeglass, or one of 2 inch. 100 revolutions of the micrometer being 2.46 inches, 1 revolution is 0.0246 inch, and 1/80 of that will be 0.0003075, or about 1/3252th part of an inch. If the focus of the object glass be 12 feet 6.5 inches, then 1/3252th part of an inch subtends about 25 thirds, or not quite half a second, so that it seems that one might observe through this object-glass charged with a two inch eyeglass, so as not to differ from the

> truth above half a second, supposing the object fixed; and a star moving along a thread may perhaps be as exactly observed as a fixed land object, when it does not flutter.[144]

I include this passage to emphasise the rigour of Bradley's work and planning months before he received the new zenith sector from Graham. It is entirely characteristic of Bradley's approach to his work in its attention to detail. As already revealed, the accuracy of the zenith sector was later confirmed by Maskelyne, who was able to determine that the instrument was accurate to within half an arc second.[145]

The investigations undertaken by Bradley at Wanstead can be interpreted as a classical example of a "Newtonian" enquiry. In contrast with the work undertaken at Kew in partnership with Molyneux he abandoned the attempt to determine the "causes" of an entirely inexplicable motion of the original object star γ Draconis.[146] He had already inferred from the Kew observations of the object and the control stars that the motion in declination may have been connected with the latitude of the star,[147] and now he was determined to test this conjecture by the use of an instrument capable of observing many bright near zenith stars in order to determine whether this hypothesis could be verified, and then to determine the parameters of this motion at different latitudes.[148] The new zenith sector designed and constructed by Graham for Bradley was allowed sufficient movement from the zenith to allow the observation of as many different near zenith stars as possible. With a capability of being moved more than 7½° from the zenith, Bradley was able to observe the first magnitude star *Capella*[149] throughout the year. Bradley was set on a course of determining *a general law* which would explain the apparently anomalous but regular motion of the object and control stars which had been observed at Kew. An analysis of the investigation made by Bradley into the "new discovered motion" reveals it as a model of "Newtonian" methodology, including the design, preparation and use of the instrumentation, and the way that the observations were analysed.[150] The contrast with the investigations undertaken at Kew could not be more marked.

Before I discuss the Wanstead programme it is perhaps instructive to briefly reflect on the nature of the investigation. The phenomenon that Bradley had been observing since December 1725 at Kew, and was now to be investigated in an entirely thorough and analytical manner from August 1727 at Wanstead, had been observed from the time that astronomers had first applied the use of sights and micrometers with their telescopes.[151] On reflection it seems extraordinary that a phenomenon that was responsible for an annual displacement of upwards to 40 arc-seconds per annum of every single object in the sky had not been isolated, analysed and explained before the investigation made by Bradley from 1725 to 1728. Astronomers had certainly observed the motion and had either claimed it as evidence of annual parallax,[152] or had ascribed the observation as an error or an anomaly.[153] What is particularly interesting is that in the few cases where a serious claim for the observation of annual parallax had been made, and then shown to be unsustainable because the observed shift did not conform to the theory of parallax, no one appears to have made a serious investigation in order to determine its parameters or the possible alternate reasons for such a large shift.[154]

The only person to investigate the motion thoroughly before Bradley was Manfredi, and he did not apprehend the intuition that the phenomenon might be general or universal. Manfredi clearly believed that his study of "aberrations" was a study of exceptions,[155] a study of motions which did not conform to the general pattern. In other words Manfredi believed that the stars really were fixed and the phenomena he was investigating were exceptions to the norm; they were called aberrations for this reason.[156]

In contrast, Bradley intuited that the phenomenon of the anomalous motion of $\gamma$ Draconis, in view of the observed motion of the control star, was likely to be a general one affecting all of the stars of the sky. He designed an experiment which allowed him to investigate as many bright stars as possible over the course of the following year to determine whether this working conjecture was valid; and whether there was a connection between the measured motion of a star in declination and its latitude. Bradley wrote, "I began to think of erecting an instrument for myself at Wanstead, that having it always at hand I might with

more ease and certainty enquire into the particular *laws* (my italics) of this new motion."[157] The shift in emphasis is radical. Bradley sought a "law" which would set out the parameters of "the new discovered motion of the fixed stars"[158] He sought to investigate as many stars with as wide a range of ecliptical longitudinal locations as possible.

There were, however, some restrictions that Bradley still had to contend with. Molyneux's house in Kew was sufficiently imposing to become a royal residence,[159] but the house taken by Elizabeth Pound after she moved out of the rectory was a more modest abode. The ground floor was only 7½ feet high[160], and above this there was a single story. This imposed limits on the length of the instrument. Thus, Bradley acquired a 12½ foot glass, to be fitted to an instrument roughly half the length of the instrument made to Samuel Molyneux's specifications.[161] In his papers there are calculations which suggest that he might have envisaged an instrument of 13 foot radius, but for various practical reasons he settled on 12½ feet.[162] In compensation, the shorter tube was much stronger and more robust than the tube for the instrument at Kew. This was essential, for as the instrument was designed to observe up to 7½ ° from the zenith it was necessary to avoid any distortion, particularly in the plane of the meridian,[163] for the telescope was supported by a screw which pressed against the lower end when it was out of the perpendicular. The instrument proved to be robust and reliable. Bradley was insistent that the instrument was capable of observing *Capella*, the brightest circumpolar star at the latitude of London, which approached to within 6¼ ° of the zenith.[164] Bradley had upwards of 200 or more possible bright stars listed in the British Catalogue, which he could now observe with his sector.

**The Stars Observed at Wanstead.**

I give the right ascension, the declination, and various notes, such as the apparent magnitude, any proper names, and other designations. Most of the stars are to be found in the Bayer and Flamsteed catalogues. A few more have been located in the *Uranometria 2000.*[165] Five of Bradley's stars have not been located

in modern catalogues, nor has it been possible to clearly identify them in the *Uranometria 2000.* The stars are listed according to their right ascension.[166]

| **Designation.** | **Right Ascension.** | **Declination.** | **Notes** |
|---|---|---|---|
| β Cassiopeiæ | 00h 09m 20s | 59° 10' 09" | mag. 2.4 *Caph* 11 Cas. Fl. |
| λ Cassiopeiæ | 00h 31m 56s | 54° 32' 29" | mag. 4.9 14 Cas. Flamsteed. |
| α Cassiopeiæ | 00h 40m 40s | 56° 33' 23" | mag. 2.5 *Shedir* 18 Cas. Fl. |
| π Cassiopeiæ | 00h 43m 37s | 47° 02' 36" | mag. 5.0 20 Cas. Flamsteed. |
| υ2 Cassiopeiæ | 00h 56m 50s | 59° 12' 00" | mag. 4.8 28 Cas. Flamsteed. |
| θ Cassiopeiæ | 01h 11m 17s | 55° 10' 05" | mag. 4.5 *Marfak* 33 Cas. Fl. |
| 34 Cassiopeiæ | 01h 20m 16s | 58° 14' 59" | mag. 5.3 φ Cas. Bayer. |
| ξ Andromedæ | 01h 22m 31s | 45° 32' 47" | mag. 5.0 46 And. Flamsteed. |
| φ Persei | 01h 43m 51s | 50° 42' 21" | mag. 4.2 |
| 4 Persei | 02h 02m 30s | 54° 30' 30" | mag. 5.0 |
| 65 Andromedæ | 02h 25m 49s | 50° 17' 38" | mag. 4.9 |
| τ Persei | 02h 54m 28s | 52° 46' 35" | mag. 4.1 18 Per. Flamsteed |
| γ Persei | 03h 05m 01s | 53° 31' 11" | mag. 3.1 23 Per. Flamsteed |
| α Persei | 03h 24m 32s | 49° 52' 23" | mag. 1.9 *Mirphak* 33 Per Fla. |
| δ Persei | 03h 43m 08s | 47° 47' 54" | mag. 3.1 39 Per. Flamsteed |
| 9 Aurigæ | 05h 06m 55s | 51° 36' 07" | mag. 5.0 |
| α Aurigæ | 05h 16m 55s | 46° 00' 55" | mag. 0.2 *Capella* |
| 18 Camelopardalis | 05h 32m 50s | 57° 13' 24" | mag. 6.5 |
| 27 Camelopardalis* | 05h 45m 50s | 49° 51' | mag. 5 Urano. 2000* |
| δ Aurigæ | 05h 59m 47s | 54° 17' 05" | mag. 3.9 33 Aur. Flamsteed. |
| 35 Camelopardalis* | 06h 04m 35s | 51° 34' | mag. 6 Urano. 2000* |
| 46 Aurigæ | 06h 25m 08s | 49° 17' 10" | mag. 5.1 |
| 13 Lyncis | 06h 47m 05s | 57° 09' 55" | mag. 5.5 |
| 27 Lyncis | 08h 08m 41s | 51° 29' 48" | mag. 4.9 |
| 35 Lyncis | 08h 52m 08s | 43° 42' 52" | mag. 5.2 |
| ι Ursæ Majoris | 08h 59m 24s | 48° 01' 43" | mag. 3.1 *Talitha* 9 U. M. Fla. |
| κ Ursæ Majoris | 09h 03m 49s | 47° 08' 36" | mag. 3.7 12 U. M. Flamsteed |
| θ Ursæ Majoris | 09h 33m 03s | 51° 39' 43" | mag. 3.3 25 U. M. Flamsteed |
| φ Ursæ Majoris | 09h 52m 18s | 54° 02' 54" | mag. 4.5 30 U. M. Flamsteed |
| 36 Ursæ Majoris | 10h 30m 48s | 55° 57' 47" | mag. 4.8 |
| β Ursæ Majoris | 11h 02m 00s | 56° 21' 51" | mag. 2.4 *Merak* 48 U.M. Fla. |
| ψ Ursæ Majoris | 11h 09m 49s | 44° 28' 51" | mag. 3.1 52 U. M. Flamsteed |
| γ Ursæ Majoris | 11h 53m 58s | 53° 40' 34" | mag. 2.5 *Phad* 64 U.M. Fla. |
| 3 Canum Venaticorum | 12h 19m 56s | 48° 57' 57" | mag. 5.6 |
| ε Ursæ Majoris | 12h 54m 08s | 55° 56' 30" | mag. 1.7 *Alioth* 77 U.M. Fla. |
| 21 Canum Venaticorum | 13h 18m 20s | 49° 39' 54" | mag. 5.1 |
| ζ Ursæ Majoris | 13h 24m 01s | 54° 54' 29" | mag. 2.4 *Mizar* 79 U. M. Fla. |
| η Ursæ Majoris | 13h 47m 37s | 49° 17' 50" | mag. 1.9 *Alkaid* 85 U. M. Fla. |
| β Draconis | 17h 30m 27s | 52° 17' 59" | mag. 3.0 *Rastaban* 23 Dra. F. |
| ι Herculis | 17h 39m 30s | 46° 00' 19" | mag. 3.8 85 Herc. Flamsteed |
| ξ Draconis | 17h 53m 32s | 56° 52' 22" | mag. 3.9 *Grumium* 32 Dra F. |
| γ Draconis | 17h 56m 38s | 51° 29' 22" | mag. 2.4 *Eltamin* 33 Dra. Fl. |
| 49 Draconis | 19h 00m 44s | 55° 39' 50" | mag. 5.5 |
| 51 Draconis | 19h 04m 57s | 53° 24' 10" | mag. 5.4 |
| κ Cygni | 19h 17m 08s | 53° 22' 32" | mag. 4.0 1 Cyg. Flamsteed. |
| ι Cygni* | 19h 27m 45s | 51° 50' | mag. 4 Urano. 2000* |
| θ Cygni | 19h 36m 29s | 50° 13' 46" | mag. 4.6 13 Cyg. Flamsteed. |
| 20 Cygni | 19h 50m 40s | 52° 59' 51" | mag. 5.2 |
| ψ Cygni | 19h 55m 40s | 52° 26' 55" | mag. 4.8 24 Cyg. Flamsteed. |
| ω2 Cygni | 20h 31m 22s | 49° 13' 57" | mag. 5.6 *Ruchba* 46 Cyg. Fl. |
| π1 Cygni | 21h 42m 10s | 51° 12' 21" | mag. 4.8 *Azelfafage* 80 C. Fl. |
| μ Cephei | 21h 43m 34s | 58° 47' 47" | mag. 4.0 *The Garnet Star.* |
| ζ Cephei | 22h 10m 56s | 58° 13' 08" | mag. 3.6 21 Cep. Flamsteed. |
| 3 Lacertæ | 22h 23m 39s | 52° 14' 48" | mag. 4.6 β Lac. Bayer |
| 5 Lacertæ | 22h 29m 38s | 47° 43' 28" | mag. 4.6 |

| | | | | |
|---|---|---|---|---|
| 7 Lacertæ | 22h 31m 23s | 50° 18' 01" | mag. 3.9 | α Lac. Bayer |
| 9 Lacertæ | 22h 37m 28s | 51° 33' 48" | mag. 4.8 | |
| 3 Andromedæ | 23h 04m 18s | 50° 04' 18" | mag. 4.9 | |
| 7 Andromedæ | 23h 12m 40s | 49° 25' 31" | mag. 4.6 | |
| 8 Andromedæ | 23h 17m 52s | 49° 02' 03" | mag. 5.0mag. | |
| σ Cassiopeiæ | 23h 59m 09s | 55° 46' 28" | mag. 4.9 | 8 Cas. Flamsteed. |
| τ Cassiopeiæ | 23h 47m 11s | 58° 40' 17" | mag. 5.1 | 5 |

Bradley's object was to select as many stars as possible which he could observe throughout the entire year, in order to determine the parameters of the motion over as wide a range of declinations as his arc could accomplish. Some twenty of the stars were capable of this exacting criterion, with apparent magnitudes up to about 3.9.[167] Although Molyneux had recently been appointed as one of the Lords of the Admiralty following the accession of George II, he appears to have maintained an interest in the project that Bradley was entering into at Wanstead. In his own copy of the Kew Observation Book,[168] Molyneux wrote, "Now Mr Bradley's instrument is set up, and we go on comparing notes from this time."[169] Both Molyneux and Bradley appear to have intended comparing their observations at Kew and Wanstead, with continued observation of γ Draconis and 35 Camelopardalis[170] at both sites. Unfortunately, for various reasons, Molyneux was only able to make six more observations.[171] Molyneux's duties at the Admiralty allowed him less time to observe,[172] but there is some suspicion that the illness that was to lead to his death on 13th April 1728[173] was beginning to have an effect. His final recorded observation was made on 29th December, 1727 and he was later to record this as, "The last observation made and entered at Kew"[174] At some time between December 1727 and February 1728 the instrument at Kew had become unusable.[175] It was Molyneux's apparent willingness to continue with the investigation, albeit as a secondary partner, which suggests that his inferred antipathy towards Newtonian philosophy might have been more ambivalent than some of the circumstantial evidence might otherwise suggest. However, although Molyneux remained a corresponding partner, there is no evidence that he went to Wanstead any later than 19th August, 1727. Bradley's Wanstead Observation Book notes his visitors and Molyneux is never mentioned.[176] Although Molyneux made his findings known to his partner sometime early in 1728, Bradley does not appear to have availed himself of his

friend's offer. He now had a clear apprehension of the task he had to accomplish, and recognised that he was committed to observing his selected stars for the period of a year. Bradley obviously believed that he would possess sufficient data to develop his own conclusions.

**The Observations at Wanstead.**

On the very first day of the new experiment on the 19th August 1727, Bradley undertook an initial observation of γ Draconis. One of the aspects of his practice that impresses is his apparent recognition that no matter how well constructed his equipment was, or how competent his own observational techniques were, he recognised that all of his observations were subject to error. He usually evaluated the parameters of his errors, and effected any actions that would diminish their magnitude. He examined his object glass minutely. As early as 28th March 1727 Bradley had discovered that the optical centre of his glass did not coincide with the centre of magnitude.[177] When he mounted his glass on the 19th August, he tested whether the apparent place of an object was affected by the part of the glass it was observed through and then ensured that the line joining the optical centre and the centre of magnitude was placed on an exact east – west alignment to ensure that when a star passed through the field of vision the apparent position of the image was not differentially affected.[178] He had previously experimented on whether the action of varying the aperture of the glass had any effect on the observation of a star when he used the sector at Kew.

> While I observed this star, (35 Camelopardalis on 20th March, 1726), which appeared very distinct, strong, and bright, with the utmost light we could give, I caused the north and south half of the object-glass to be alternately covered, and still the star seemed to move perfectly true along the thread.[179]

This took place at about 9.30 in the evening. At about 5.50 the following morning he got a servant to breathe twice on the object glass, to create moisture, to determine whether this affected the apparent position of an object.[180] Except for a

diminishing of the image he determined that there were no other discernable effects.

The record of Bradley's zenith observations at Wanstead reveals that he began to observe 35 Camelopardalis from 11th September, 1727.[181] Within a short time, certainly by the time he observed it on the 13th October;[182] he was beginning to abandon his initial hypothesis that its apparent position relative to that of γ Draconis was in proportion to their respective latitudes. He wrote in his account,

> I have before taken notice that it appeared from Mr. Molyneux's observations, that γ Draconis altered its declination about twice as much as the forementioned small star almost opposite to it; but examining the matter more particularly, I found that the greatest alteration of declination in these stars was as the sine of their latitude of each respectively. This made me suspect that there might be the like proportion between the maxima of other stars; but finding that the observations of some of them would not *perfectly* [my italics] correspond with such an hypothesis, and not knowing whether the small difference I met with might not be owing to the uncertainty and error of the observations. I deferred the farther examination into the truth of this of this hypothesis, till I should be furnished with a series of observations made in all parts of the year;[183]

It is both typical of Bradley's practice and of its distinctive Newtonian methodology to test a hypothesis against observational data, and to defer any conclusions until he had acquired sufficient data. He also assessed the initial hypothesis against the data he had acquired on the other stars he had been observing since the 19th August, and remained undecided.

In his account of Bradley's work in the *Memoirs*,[184] Rigaud refers to the account of the boating party on the Thames contained in Thomas Thomson's *History of the Royal Society*.[185] As I have already remarked it is almost inconceivable that Rigaud should have included Thomson's account without due reason. That he should have included it at this stage of his account of Bradley's work on the new discovered motion is quite simply incredible, especially when it referred him being "in despair", an absurd conclusion given his propensity for

extended and detailed investigation.[186] Rigaud must have been aware of the painstaking way that Bradley undertook his investigations, and he should have been sceptical of the Thomson account, both for its lack of authentication, and for the way the account reduced the solution of Bradley's own highly systematic investigation to a single inspired moment of insight.[187] It quite clearly contradicted Bradley's own account, and the manner in which he had directed his own enquiry. As will be shown, the means by which he determined the law of the motion and then proposed his hypothesis was based entirely on his mature astronomical practice.[188]

Bradley began to observe his chosen stars systematically from the 19th of August 1727. Up to about a dozen stars were observable all year in all conditions at all times of the day or night. When he began observing γ Draconis on the19th of August at Wanstead it was 18" further north than it had been when it was observed in December 1726 at Kew.[189] The star continued moving until it was a further 1.3"north from the 7th to the 14th of September,[190] when it began moving south. By the 10th of December it was back to same position it had been observed at Kew a year earlier.[191] On the 7th of March it had moved 19.3" south of this position[192] before returning to the position where it was located on about the 8th of June 1728.[193] Finally it returned to the position it was observed at on the 19th of August, exactly one year after the initial observation at Wanstead.[194] So far, the motion of γ Draconis merely confirmed the motion observed at Kew from December 1725.[195]

Bradley was also observing several other bright stars throughout the year from 19th August 1727. These included a bright star with a similar right ascension to γ Draconis, a group in opposition to γ Draconis, and two groups in quadrature, one six hours ahead and another six hours behind. The "object group" included γ and β Draconis, and the "control group" α Aurigæ (*Capella*) and 35 Camelopardalis. The two quadrature groups included β Cassiopeiæ, α Cassiopeiæ, τ Persei and α Persei, six hours in arrears, and γ Ursæ Majoris, ε Ursæ Majoris and η Ursæ Majoris, six hours ahead. It was readily apparent that the observational data pointed to a distinctive pattern.

| Star | RA | Dec | Stationary Periods |
|---|---|---|---|
| β Cassiopeiæ | **00h 09m 20s** | 59° 10' 09" | **late Nov. & late May** |
| α Cassiopeiæ | **00h 40m 40s** | 56° 33' 23" | **early Dec. & early June** |
| τ Persei | **02h 54m 28s** | 52° 46' 35" | **late Dec. & late June** |
| α Persei | **03h 24m 32s** | 49° 52' 23" | **early Jan. & early July** |
| α Aurigæ | **05h 16m 55s** | 46° 00' 55" | **early Feb. & early Aug.** |
| 35 Camelopardalis | **06h 04m 35s** | 51° 34' | **mid Mar. & mid Sept.** |
| γ Ursæ Majoris | **11h 53m 58s** | 53° 40' 34" | **late June & late Dec.** |
| ε Ursæ Majoris | **12h 54m 08s** | 55° 56' 30" | **early July & early Jan.** |
| η Ursæ Majoris | **13h 47m 37s** | 49° 17' 50" | **mid July & mid Jan.** |
| β Draconis | **17h 30m 27s** | 52° 17' 52" | **early Sept. & early Mar.** |
| γ Draconis | **17h 56m 38s** | 51° 29' 22" | **mid Sept. & mid Mar.** |

It was also apparent that the stationary points coincide with the period when the Earth is in quadrature to each star. The pattern is referred to in Bradley's account of the new discovered motion. He wrote,

> … after I had continued my observations a few months, I discovered what I then apprehended to be a general law, observed by all the stars, viz. that each of them became stationary, or was farthest north or south, when they passed over my zenith at six of the clock, either in the morning or evening. I perceived likewise, that whatever situation the stars were in with respect to the cardinal points of the ecliptic, the apparent motion of every one tended the same way, when they passed my instrument about the same hour of the day or night; for they all moved southward, while they passed in the day, and northward in the night; so that each was the farthest north when it came about six of the clock in the evening, and farthest south when it came at six in the morning.[196]

Bradley had also determined, from the observations of these stars and others, that the motion he had first observed in γ Draconis was a general law obeyed by all of the stars he was observing.[197] He remained uncertain as to the parameters of the motion, his first presumption that the magnitude of the motion was connected with the declination of the star being unsustainable.

| Star | Dec. | RA | Max. of Aberration. |
|---|---|---|---|
| α Aurigæ | **46° 00' 55"** | 05h 16m 55s | **8.07"** |
| η Ursæ Majoris | **49° 17' 50"** | 13h 47m 37s | **17.81"** |
| α Persei | **49° 52' 23"** | 03h 24m 32s | **11.43"** |

| | | | |
|---|---|---|---|
| γ Draconis | **51° 29' 22"** | 17h 56m 38s | **19.30"** |
| 35 Camelopardalis | **51° 34'** | 06h 04m 35s | **9.44"** |
| β Draconis | **52° 17' 59"** | 17h 30m 27s | **19.80"** |
| τ Persei | **52° 46' 35"** | 02h 54m 28s | **12.87"** |
| γ Ursæ Majoris | **53° 40' 34"** | 11h 53m 58s | **16.67"** |
| ε Ursæ Majoris | **55° 56' 30"** | 12h 54m 08s | **18.03"** |
| α Cassiopeiæ | **56° 33' 23"** | 00h 40m 40s | **16.55"** |
| β Cassiopeiæ | **59° 10' 09"** | 00h 09m 20s | **17.51"** |

There is no correlation between the declination of the observed stars and their maxima of aberration.[198]

Bradley continued his observations of the 69 stars, including those which he was observing all year round. Rigaud conjectured when Bradley formed his idea of the causes of the phenomenon he had been observing since December 1725, based on the idea that it was following the boating party, he suggested that it was probably around September 1728.[199] My own analysis, assuming the account of the boating party is at best apocryphal, points to an earlier date, possibly as early as March or April 1728. Such a boating party, presumably in the summer of 1728, may only have confirmed an idea already clearly formed in his mind. Rigaud is certain it couldn't have been earlier than August 1728, after a year of observation at Wanstead. I am sure Bradley would not have expressed any premature conclusions, for this would have contradicted his established practice, but he must have been developing his ideas after observing the phenomenon for about six months. Rigaud asserts,

> It will be seen[200] that, with the exception of about a fortnight which Bradley spent at Oxford in the beginning of November, there was no relaxation of the observations at Wansted during the last four months of 1728. He must have traced out the consequences of his hypothesis before he could have devised those rules[201] for its effects which he gives in his paper, and it is clear that they were the results of some regular demonstration.[202]

I must assert that Bradley was characteristically very circumspect in the claims he made in his paper. From reading Rigaud's account it can appear that he made a fundamental error in confusing Bradley's "hypothesis" with the "law" of the new discovered motion. This suggests to me that Rigaud perceived that Bradley had

made an empirical "discovery", which may explain why he included the Thomson account, for this also pointed to the view that Bradley "discovered" aberration.

In his paper Bradley acknowledged that his hypothesis was constructed on as yet unconfirmed conjectures, which including the orbital motion of the Earth and the progressive motion of light.[203] On the other hand, Bradley's elucidation of the law of the new discovered motion, which was later to be termed the aberration of light, was empirically demonstrable.[204] This apparent confusion in Rigaud's mind is what led to his assumption that the law of the new discovered motion was not identified until late in 1728, whereas there is every reason to believe that Bradley was beginning to form the idea of this law by April 1728.[205] It was not the law which caused Bradley any great difficulty, which could be confirmed by analysing the motions of the many stars he had been observing, but rather the explanation of that law. It was when Bradley referred the motions of the stars relative not to the *equator* but to the *ecliptic* that he derived the law. For this he only required half a complete revolution of each of the stars he was observing. In contradiction to Rigaud's confusion of the determination of the phenomenological law with the assertion of the hypothesis[206] it would appear that the basis of the law, which was the declinational motion of the stars relative to the ecliptic, was determined well before the conjectures which formed the hypothesis of "the new discovered motion of the fixed stars".

Rigaud asserted that Bradley resolved the problem of the law of the annual motion, but contended that there was no way of determining the year the demonstration[207] was written.[208] A comparative study of sources, however, can lead to the conclusion that the paper Bradley wrote on his demonstration must have been written no later than the end of 1729, and given the internal evidence of his paper on the new discovered motion, no earlier than the end of 1728.[209] In his demonstration Bradley consistently used the term "parallax on account of the motion of light"[210] to describe the new motion.[211] Comparison with other documents suggests that by 1730 Bradley had adopted the less ambiguous term "annual aberration", following the publication of Manfredi's thesis in May 1729.[212]

**The Hypothesis of the New Discovered Motion.**

Bradley was able to demonstrate from his observations that if the Earth moved in a perfectly circular orbit then a star situated close to the pole of the ecliptic would appear to describe a small circle. However, as the Earth's orbit was an ellipse, any star at the pole of the ecliptic would appear to describe a small ellipse over the course of a year. A star situated on the ecliptic would over the course of a year appear to move east and west in right ascension with no recorded motion in declination. The amount of motion of any star in declination is therefore a function of its *ecliptic* latitude. As such this would also account for the motion of any star due to annual parallax. In his demonstration Bradley began his account with the hypothesis that,

> If we suppose the distance of the fixed stars from the sun to be so great that the diameter of the earth's orbit viewed from them would not subtend a sensible angle, or which amounts to the same, that their annual parallax is quite insensible;[213]

As such this was the problem posed in any attempt to measure the annual parallax of the stars. Yet it was also obvious that the stars he was observing at Wanstead were moving considerably, with semi-annual motions of up to 40 arc-seconds. Moreover, the expected motions associated with annual parallax were not observed, for quite aside from the realisation that the stars were not moving at the times of the year when such parallactic motions should have been expected, *the observed stars were also moving in the opposite direction from that expected from parallax*. Bradley wrote,

> But if we further suppose that the velocity of the earth in its orbit bears any sensible proportion to the velocity with which light is propagated, it will thence follow that the fixed stars (though removed too far off to be subject to a parallax on account of distance) will nevertheless be liable to an aberration,[214] or a kind of parallax, on account of the relative velocity between light and the earth in its annual motion."[215]

This was a far-reaching insight, for it was universal,[216] and the phenomenon would be seen to affect every single object in the sky. As the Earth moved in its

orbit, the light from a star appeared to come from a different direction than if it were at rest. Thus if a star was observed through a telescope then it would not be pointed exactly at the star but at a small angle β directed ahead of the star. The magnitude of angle β would depend on the angle α made by the *apparent* direction between the Earth and the star and the direction of the Earth's motion. Thus the greatest deflection would appear when $\alpha = 90°$ when the angle of aberration was 20.496".[217] In Bradley's time the mean major axis and not the mean semi-major axis of the Earth's orbit was used to determine the angles of parallax and aberration. The observed angle of motion in declination would be a direct function of the star's ecliptic latitude so it would diminish to 0.0" when $\alpha = 0°$. Starlight would be deflected by 20.496" in the direction of the Earth's motion. A telescope would be moved in the appropriate direction defined by the ecliptic latitude of the star, the angle being defined by the equation,

$$\sin \beta = v/c \sin \alpha$$

where v is the velocity of the Earth in its orbit and c is the velocity of light. The law appeared obvious to Bradley once he intuited that the angle of deflection was partially a function of the velocity of the Earth in its orbit and the velocity of light from the star.

The law of the new discovered motion (the theory of the aberration of light as it was to become) was nevertheless constructed on Bradley's well established astronomical practice. Rigaud gave too much credence to the Thomson account of the boat trip,[218] for Bradley nowhere makes any reference to it in any of his papers, published or otherwise. Rigaud also ignored the account offered by Bradley.[219] The problem resides in Rigaud's belief that the hypothesis of the new discovered motion preceded the derivation of the law.[220] The hypothesis was in part derived from the law, which was constructed on the constituent presumptions which underlay Bradley's well established derivation of the variation in the motions of the Jovian satellites.[221] Bradley expressed his concept of the law of the new discovered motion clearly enough in the published paper.[222]




From what hath been premised, it will appear that the greatest alteration of the apparent declination of γ Draconis, on account of the successive propagation of light, would be to the diameter of the little circle which a star (as was before remarked) would seem to describe about the pole of the ecliptic, as 39" to 40.4". The half of this is the angle A C B, (as represented in the fig.) This therefore being 20.2", A C will be to A B, that is the velocity of light to the velocity of the eye (which in this case may be supposed the same as the velocity of the earth's annual motion in its orbit) as 10210 to 1, from whence it would follow, that light moves or is propagated as far from the sun to the earth in 8' 12".[224]

Bradley's hypothesis which is connected to the law of the new discovered motion, is constructed on the apprehension of several assumptions which he used also in the elucidation of the law. These assumptions included the notion that the Earth was in motion around the Sun,[225] that the velocity of light was finite,[226] that light was propagated in straight lines,[227] and even that the phenomenon he was observing was universal.[228] To Bradley and many of his educated contemporaries these assumptions appeared to be perfectly rational and acceptable, though Bradley clearly understood that they were presumptions nevertheless. Bradley wrote,

> There appearing therefore after all no sensible parallax in the fixed stars, the Anti-Copernicans have still room on that account to object against the motion of the earth; and they may have (if they please) a much greater objection against the hypothesis by which I have endeavoured to solve the forementioned phænomena, by denying the progressive motion of light, as well as that of the earth.[229]

In his paper Bradley illustrated the accuracy of his deductions by the application of his law or hypothesis to various observations of γ Draconis and η Ursæ Majoris over the previous year. He first offered 15 observations of γ Draconis from the 20th of October 1727 to the 6th of September 1728 out of a total of 59 made between and including those dates.[230]

**γ Draconis.**[231]

| Date | | Difference of Declination by Observation | Difference of Declination by Hypothesis |
|---|---|---|---|
| 1727 | October 20th | 4½" | 4½" |
| | November 17th | 11½" | 12" |
| | December 6th | 17½" | 18½" |
| | December 28th | 25" | 26" |
| 1728 | January 24th | 34" | 34" |
| | February 10th | 38" | 37" |
| | March 7th | 39" | 39" |
| | March 24th | 37" | 38" |
| | April 6th | 36" | 36½" |
| | May 6th | 28½" | 29½" |
| | June 5th | 18½" | 20" |
| | June 15th | 17½" | 17" |
| | July 3rd | 11½" | 11½" |
| | August 2nd | 4" | 4" |
| | September 6th | 0" | 0" |

Bradley also offered 14 observations of η Ursæ Majoris made between and including the 14th of September 1727 and the 20th of September 1728 out of a total of 50 such observations.[232]

**η Ursæ Majoris.**[233]

| Date | | Difference of Declination by Observation | Difference of Declination by Hypothesis |
|---|---|---|---|
| 1727 | September 14th | 29½" | 28½" |
| | September 24th | 24½" | 25½" |
| | October 16th | 19½" | 19½" |
| | November 11th | 11½" | 10½" |
| | December 14th | 4" | 3" |
| 1728 | February 17th | 2" | 3" |
| | March 21st | 11½" | 10½" |
| | April 16th | 18½" | 18" |
| | May 5th | 24½" | 23½" |
| | June 5th | 32" | 31½" |
| | June 25th | 35" | 34½" |
| | July 17th | 36" | 36" |
| | August 2nd | 35" | 35½" |
| | September 20th | 26½" | 26½" |

In none of his observations did he differ from the predictions of the law of the new discovered motion by more than 1½ arc seconds.[234]

One of the consequences of Bradley's observations was a new derivation of the velocity of light, presuming that the phenomenon be accepted as evidence of the motion of the Earth in its orbit around the Sun. It was to offer a value well within 1% of modern estimates.[235] Olé Rømer had estimated that light took 22 minutes to cross the full diameter of the Earth's orbit.[236] This was derived from his observations of the satellites of Jupiter, particularly of the innermost Galilean moon, Io.[237] Bradley calculated that light took 8 minutes and 12 or 13 seconds to travel from the Sun to the Earth.[238]

**The Paper on the New Discovered Motion.**

There is every indication that the paper was written under the most intense time pressure. This is clearly indicated by the postscript that Bradley added to the end of his published paper.[239] The postscript incorporated a disavowal of the correctness of Robert Hooke's results and an appeal to John Flamsteed's data of 1696 which formed the basis of the first Astronomer Royal's claim for the observation of the annual parallax of the pole star in his letter to John Wallis.[240] It is when the third unpublished version is analysed that the significance of the postscript becomes evident, particularly as this version, obviously completed after the published paper had been read to the members of the Royal Society,[241] clarified Bradley's motives in writing the paper. He was urged to complete the paper as quickly as possible by Halley, who was anxious that the priority of Bradley's claim be pressed before it could be forestalled. The third unpublished version was edited and included as Appendix 3 in my M.Sc. dissertation.[242] On the opening page of his published account Bradley wrote,

> Before I proceed to give you the history of the observations themselves, it may be proper to let you know that they were first begun in hopes of verifying and confirming those that Dr. Hooke formerly communicated to the public, which seemed to be attended with circumstances that promised

> greater exactness in them, than could be expected in any other that had been made and published on the same account.[243]

In contrast with this assertion of his apparent confidence in Hooke's results Bradley's expressed disavowal could not be bolder.

> And since I have been convinced both from Mr. Molyneux's observations and my own, that the Doctor's are really very far from being either exact or agreeable to the phænomena, I am greatly at a loss how to account for it. I cannot well conceive that an instrument of the length of thirty-six feet, constructed in the manner he describes his, could have been liable to an error near 30" (which was doubtless the case,)[244] if rectified with so much care as he represents."[245]

As earlier discussed there are a more than a few strong pointers, circumstantial and documentary which lend credence to the view that the investigations that took place at Kew possessed a distinct anti-Newtonian inclination.[246] Given Molyneux's premature approach to Newton and his insistence that he, Bradley and Graham had discovered a nutation that "destroyed completely the Newtonian system",[247] the motives must be suspect. Given the basis of Bradley's training as an astronomer, under the tutelage of Pound and Halley, and the development of his practice within the contexts of Newtonian theory, it must be assumed that if Bradley became aware of his partner's approach to his most eminent and influential mentor, he would have been highly discomforted. Bradley's difficulty lay in presenting an account of his researches at Kew and Wanstead which did not offer any suspicion that any questions might be raised about the character of the Kew experiment. At the time Bradley wrote his account he was still in a delicate financial position.[248] Although he possessed strong *bone fides* of his undoubted Newtonian commitments through his many connections with leading members of Newton's circle, it must have been a cause of embarrassment that he probably perceived that the experiment at Kew appeared on reflection to have been predicated on anti-Newtonian grounds.

Molyneux did not require the support of the influential elites who controlled patronage in English society, for he was himself a member of the

political elite, nor did he require the approbation of the Royal Society. As a wealthy, well connected gentleman he did not feel himself beholden to any other man's opinion, at least in matters philosophical. He was a skilled politician. This was demonstrated in his approach to John Conduitt, who wrote, "and therefore he was under the greatest difficulty how to break it to Sir Isaac, and when he did it broke it by degrees in the softest manner."[249] There was no need for Molyneux to have made his approach at all, particularly as the observations would soon have demonstrated that it was highly premature.[250] The precipitate way that Molyneux approached Newton, suggests an overwhelming eagerness to inform him that he and Bradley had discovered a nutation that undermined Newtonian physics. The manner of Molyneux's approach, breaking the news "in the softest manner" thus suggests the manipulative skills of a successful politician.[251]

Bradley, in contrast, was impecunious[252] and dependent on both patronage and the support of his mentor Edmond Halley. Bradley addressed his account to Halley, under his prompting, and he clearly recognised that he had to offer an account that did justice to the work at Kew and Wanstead, and yet did not offer any suspicion that the investigation was ever associated, even by inference, with any untoward motives. Bradley was guarded and ambiguous when discussing the motives of the work at Kew.

> And as [Hooke's] attempt was what principally gave rise to this, so his method in making the observations was in some measure that which Mr. Molyneux followed: for he made choice of the same star, and his instrument was constructed upon almost the same principles.[253]

Hooke's attempt had been to observe the annual parallax of a star.[254] Ostensibly, it would appear that Molyneux was also seeking to repeat Hooke's experiment in order to measure annual parallax, but Bradley's words are instructive for the observations were also "begun in hopes of verifying and confirming"[255] Hooke's results, in which he had great confidence. Given Bradley's practice of repeated accurate and precise observation, the notion that he would possess any confidence in the results of an experiment in which only four observations were made over a period of only three and half months is particularly absurd.[256] In addition it was

well known that the claim had been abandoned by Hooke.[257] I do think it is vitally important to underline this point. The observations at Kew were presented in the paper as an attempt to measure the annual parallax of γ Draconis. In itself this was a commendable objective, for Hooke's *method* was in every way effectual in the pursuit of this. However, the experiment at Kew was, under Molyneux's direction, predicated on the basis of confirming Hooke's *result.* Now the approach that was predicated on the utilisation of Hooke's method may be conceived of as an attempt to confirm the Copernican thesis, but an approach that was determined as an attempt to confirm Hooke's result, must be interpreted as an attempt to undermine Newtonian theory.

Bradley was being disingenuous when he claimed confidence in the Hooke result. He presents this as a perfectly commendable hope without any discussion whatsoever of the consequences this might imply. As I have already averred it appears inconceivable that Bradley could express such confident support in a result obtained from four such disparate observations. Hooke's work contrasted markedly with his own repetitive and cautious methods. Bradley's partnership with Molyneux and Graham was common knowledge in astronomical circles,[258] but the characteristics of that investigation with its incipient anti-Newtonian tendencies were not nearly so well appreciated.[259] Bradley adopted the strategy of presenting the work at Wanstead as a seamless extension of the work at Kew. Even though the design and objectives of the Wanstead programme were very different from the Kew experiment, Bradley believed his own interests would be advanced by presenting the work at Kew as a disinterested or unbiased investigation, motivated only by curiosity.[260]

In his account Bradley conflates the two experiments, and merges incidents from the former experiment into his work at Wanstead. Bradley's account deliberately makes light of the various conjectures proposed at Kew. Referring to his work at Wanstead he wrote,

> When the year was completed, I began to examine and compare my observations, and having pretty well established myself to the general laws of the phænomena, I then endeavoured to find the cause of them. I was

already convinced that the apparent motion of the stars was not owing to a nutation of the earth's axis.[261] The next thing that offered itself was an alteration in the direction of the plumb-line, with which the instrument was constantly rectified; but this upon trial proved insufficient.[262] Then I considered what refraction might do; but here also nothing satisfactory occurred.[263] At last I conjectured that all the phænomena hitherto mentioned proceeded from the progressive motion of light and the earth's annual motion in its orbit.[264] For I perceived that, if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object; and that when the eye is moving in different directions, the apparent place of the object would be different.[265]

This is a revealing passage, not so much because it sets out the hypothesis of the new discovered motion, but because it reveals Bradley's consummate skill in passing over the various conjectures proposed in Kew without giving any indication that they might have posed difficulties for orthodox Newtonian theory.[266] Bradley is not merely writing an account of his work on the new discovered motion, he is also setting out his Newtonian credentials. To transform a programme that may originally have been conceived as a possible assault on Newtonian expectations in terms of Newtonian orthodoxy is a consummate achievement. The account is in no way a simple narrative of his work at Kew and Wanstead but a brilliantly conceived piece of propaganda.[267] He is appealing to most of all to Halley, but also to the intellectual elites that supported Newtonian orthodoxy. Bradley was very aware of how these sources of influence and patronage could advance his own interests.

It is when the third unpublished version of this paper is examined that the full power of this strategy is appreciated. There are several variant passages between the published version of Bradley's paper,[268] and the third unpublished paper.[269] The most interesting passage in the latter unpublished version, because it is obviously directed to Halley, is concerned with his mentor's scepticism

regarding the observation of angles as small as half an arc second. Bradley does so by appealing to work undertaken by Halley himself, and as such he would have hoped that it appeared persuasive to his friend. Bradley wrote,

> If the distance of the fixed stars is so immensely great as my observations imply, it will no longer seem strange that their diameters should be so small as you have indisputably proved them (in your remarks on Mr. Cassini's Essay on the parallax of Sirius)[270] by the most proper method we can have of determining them, viz: by the occultations of the brighter stars by the Moon. For if we suppose them no bigger than the Sun, and that they are placed above 400,000 times farther from us as it (as γ Draconis, a star of the second magnitude seems at least to be)[271] then their diameters would not be 1/200$^{th}$ part of a second.[272] There is room enough therefore still left to make allowance, both for the different distances of the stars of the first and second magnitude and also for the difference of their real bigness without supposing their diameters so much as one second,[273] which the observations of *Aldebaran* and *Spica Virginis* will not allow us to do. But if we should attempt to deduce the real magnitude of the fixed stars from their apparent diameters as seen through a telescope, the aperture of whose object glass is so contracted, as to take off the spurious rays and make the stars appear distinct and round, we shall find them of an enormous and incredible bigness. For even γ Draconis seems in the night through the telescope of my instrument (when I leave an inch aperture) 4" or 5" in diameter; whence (if we proceed in this manner) we should find its diameter at least 4 or 5 times bigger than the diameter of the Earth's annual orbit. But I presume no one will think this at all probable, since we may from other considerations be convinced, that this apparent diameter of a fixed star is an optic fallacy.[274]

Bradley worked to very precise tolerances, which Halley always remained skeptical of,[275] and after observing many bright stars over the course of a year felt confident enough to assert that he had not observed any evidence for an annual parallax of even a single second of arc.[276] Graham's instrument constructed for

Bradley's observations at Wanstead was capable of matching his extraordinary skill as an observer.[277] Therefore no star could be nearer than 400,000 AU. Arguing thus, that if the stars were of the same order of size as the Sun (which by analogy would have been accepted by Bradley as an average star), he reasoned that the stars would therefore be expected to reveal "real" diameters of up to 1/200$^{th}$ of an arc second. Halley's studies of the extinction rates of first magnitude stars through the process of lunar occultations revealed that they vanished virtually instantaneously.[278] This suggested they possessed extremely small angular diameters, clearly contradicting the appearance of first magnitude stars in contemporary telescopes, where images of 4 or 5 arc seconds were common. It reveals that Bradley was thinking of stars as physical systems, and must be accepted as an early attempt to consider the diameters of stellar systems, as analogues of the solar system. Bradley was incorporating the results of some of Halley's work, and confirming Newton's belief that the stars were at such vast distances that the observation of annual parallax is not to be expected using contemporary technology. The contrast with the Hooke result, which Bradley rather disingenuously attempted to convince his reader he found convincing, could not be more marked.[279]

**The Reception of the Paper on the New Discovered Motion.**

Before discussing the reception of the paper it is necessary to assert what the paper did **not** claim. Bradley's paper did not claim that he had "discovered" the aberration of light. The paper was an account of the observation of a phenomenon that Bradley had isolated over three years of observation, before determining its parameters and proposing a hypothesis which might account for it. Nowhere is the term "aberration" used. And yet, within a short period the term "stellar aberration" was being widely used to describe the new discovered motion.[280] The gradual adoption of the term "aberration" after the publication of the paper on the new discovered motion offers a valuable insight into the way that Bradley's paper was received.

There is an early document dating from the 1730s, possibly as early as 1729 or 1730, in which Bradley refers to "Rules for computing the Aberration or Parallax of the Stars arising from the Motion of Light."[281] An earlier document than this suggests why Bradley was forced to seek a new unambiguous term that clearly distinguished the phenomenon from that of annual parallax. Bradley wrote,

> The parallax of the stars on account of the motion of light[282] causing them to appear always removed from their true places in lines parallel to the tangents in the respective points of the Earth's Orbite; tis thence evident that if the earth's orbite were perfectly circular and the sun in its center the stars would describe exactly the same Ellipses by their apparent motion, as if they were subject to an Equal annual parallax upon account of Distance;[283] only the longer Axis in the Parallax arising from Light[284] would lie in the same Direction as the Shorter Axis in the Parallax arising from Distance;[285] the visible place of the star in the former case[286] being 3 signs farther back in the Ecliptick,[287] than in the Latter."[288]

The reference to "parallax on account of the motion of light" is not only confusing, it also quite misleading, for the phenomenon of the new discovered motion had nothing to do with "parallax".

Bradley's paper on the new discovered motion was read at the Royal Society on the 9th and the 16th of January 1729 (OS). Five months later on the 30th of May 1729 (NS) Manfredi published his treatise, *De Annuis Inerrantium Stellarum Aberrationibus*.[289] Based on a programme of observation from 1707 until 1719 Manfredi's work was an attempt to reveal that the Copernican system was unsupported by observational evidence. He sought to demonstrate that the various motions observed amongst the "fixed" stars, which were often claimed as evidence of annual parallax, did not conform to parallax theory. These anomalous motions, which he termed "aberrations", were usually quite contrary to the theory of annual parallax. From this Manfredi argued for the falsity of the Copernican theory. In the introduction to his treatise Manfredi wrote,

> There are some however who, noticing that these strayings of stars recur at fixed times of the year, think there is no doubt the matter has to do with an annual movement which with Copernicus they attribute to the earth, and so they proclaim that a true universal system is disclosed and demonstrated by this very piece of evidence, in which however it is easy to be deceived, since the Copernican system is tottering together with the arguments by which they think it is supported, as I am confident that I shall show in this very work.[290]

It is therefore ironical that Manfredi's use of the term "aberration" to describe these various "strayings" of the motion of various stars should have been adopted by Bradley, for the theory of annual aberration became widely accepted as direct empirical evidence of the motion of the Earth around the Sun, even though Bradley was adamant in his paper that this was merely hypothetical. In applying the term to the new discovered motion of the fixed stars, Bradley was able to develop his concept and acquire a clearer distinction from that which underlay annual parallax. Over the following months and years the use of the lucid term "aberration" in place of the inelegant and misleading "parallax of the stars on account of the motion of light" facilitated an easier acceptance of his hypothesis. In his letter to Antonio Leprotti in 1730[291] Manfredi stated that although there were a few exceptions, he found his own observations indicated a motion in right ascension analogous to that which Bradley had detected in declination.[292] Earlier in the same correspondence Manfredi mentioned that Sir Thomas Dereham[293] translated Bradley's paper on the new discovered motion into Italian soon after it was published. If Manfredi was so soon apprised of Bradley's work, then assuming that Dereham was a regular correspondent with the Royal Society, it may be accepted that Bradley quickly learned of Manfredi's treatise. This appears to be evidenced by the appearance of Bradley's early "transitional" papers on "aberration".[294]

It appears that the term, "aberration of light" was being used within a few months by Bradley.[295] Manfredi's term "aberration" was precisely what was needed to describe the "new discovered motion",[296] and the use of the term

"annual aberration" to distinguish it clearly from the term "annual parallax" must have clarified the radical contrast between the two quite distinct phenomena. What is certain is that the term "the aberration of light" was in general circulation by the time the correspondence between Maupertuis and Bradley began in 1737. In a letter addressed to Graham after the receipt of the first letter from Maupertuis, Bradley wrote, "As I must soon send Mr. Maupertuis an answer to his request about the quantity of aberration on account of light in those stars which they observed",[297] before going on to discuss the advisability of correcting Maupertuis' observations from the Lapland expedition[298] with further adjustments due to the hitherto undisclosed phenomenon of the nutation of the Earth's axis which he had been observing for ten years. Certainly this can be taken as evidence that by 1737 the aberration of light was being utilised to reduce the observations of astronomers throughout Europe and overseas.[299]

**The Ontological Status of the Concept of the Aberration of Light.**

Various claims have been made on behalf of other astronomers for the discovery of the aberration of light. Strong claims have been made on behalf of Picard and Manfredi who both made extended studies of the phenomenon, Picard in connection with his studies of *Polaris*, and Manfredi in connection with his studies of aberrations. Many other astronomers, including Cassini I, Hooke, Flamsteed, Cassini II and Maraldi, observed motions which have now been identified as instances of the phenomenon later identified by Bradley as examples of the new discovered motion. Such claims, however, have all been made on the basis that the aberration of light was discovered by simple observation.[300] Anomalous motions of the stars had been observed by many astronomers for decades before Bradley published his paper.[301] They remained anomalous motions until a new theoretical construct had been forged by Bradley from 1726 to 1728. Until this had been achieved the many observations of the phenomenon remained errors or anomalies. It is precisely for this reason that all the earlier observations of "aberration" cannot be accepted as claims for the discovery of the aberration of light.[302] Bradley wrote his paper on the "new discovered motion" and it was the

reification of his hypothesis of the new motion that formed the basis of the theory of the aberration of light. The reification process was very largely a process of socialisation.[303]

The development of aberration theory as an important conceptual construct which transformed the science of positional astronomy created Bradley's reputation not merely in England, but in France and later throughout much of Europe and Russia. It was a concept that was accepted almost immediately, for although Bradley explicitly asserted that his hypothesis of the new discovered motion of the fixed stars was conjectural, for neither the motion of the earth, nor the progressive motion of light had been demonstrated independently, it conformed to the beliefs and the conceptual expectations of the huge majority of astronomers in Europe during the early 18th century.[304] To many with minds less acute than Bradley's, the theory of the aberration of light was accepted as evidential proof of the motion of the Earth just as surely as if he had measured the annual parallax of a star.[305]

Although the ontological status of the aberration of light would remain debatable until either the finite velocity of light had been verified experimentally, or the motion of the earth was independently confirmed, Bradley's primary achievement amongst his fellow astronomers was in his transformation of positional astronomy.[306] With his careful calibration of the parameters of the new discovered motion, whatever its cause, Bradley increased the positional accuracy of the science at least tenfold. With the search for longitude becoming so important, the astronomical route was greatly facilitated by the inception of Bradley's accurate and precise determination of the apparent positional variation of every object in the sky.[307] The achievement was characteristically Newtonian in its scope. Newton had determined the parameters of universal gravitation without asserting the causes of gravity, and likewise Bradley, his protégé, had determined the parameters of annual aberration without need of determining its causes.[308] It is of great importance that this is appreciated. Bradley had carefully determined the exact parameters of the new discovered motion. He also proposed a hypothesis to account for the phenomenon, a conjecture that was widely, if not

universally accepted by astronomers, but Bradley also conceded that those opposed to the Copernican theory still possessed sufficient grounds of objection. In the absence of any coherent explanation of the new discovered motion in geocentric terms, even by such apparently committed geocentricists as Manfredi, the argument was won by default. The theory of the aberration of light was socialised as evidence of the motion of the earth.[309] Yet the record of Bradley's observations at Wanstead from the 19th of August 1727 did not conform exactly to the predictions of the theory of the aberration of light. Bradley began to examine whether these observations were merely anomalies that might be accounted for by instrumental or observational error, or whether they evidenced another phenomenon hidden in the residuals of Bradley's observations of the new discovered motion?

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[1] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.*
[2] M. E. W. Williams, *Attempts to Measure Annual Stellar Parallax: Hooke to Bessel.* PhD Thesis, University of London, 1981.
[3] Most notably Eustachio Manfredi, the principal of the Papal College in Bologna, who worked extensively at the Bologna Observatory with several partners, including Francesco Algarotti, the later author in 1737 of *Newtonianismo per la dame,* one of the most popular general introductions to Newtonian theory on the continent. Algarotti was a regular visitor to Voltaire. Manfredi was a corresponding member of the Royal Society and supported the dissemination of Newtonian optical theory throughout Italy, where he was celebrated as a leading contemporary poet.
[4] This principle was developed by G. W. Leibniz. Many other philosophers utilised the concept. According to Leibniz the ultimate "sufficient reason" was the free will of God.
[5] The term "astronomical unit" is an anachronism in early 18th century astronomy. It is the length of the semi-major axis of the distance from the Earth to the Sun, the baseline from which annual stellar parallax may be determined using trigonometrical methods.
[6] James Gregory, *Geometriæ pars universalis,* Padua, 1668.
[7] In fact, *Sirius* which is designated ά Canis Majoris has an absolute magnitude of -1.5, which makes it about 23 times more luminous than the Sun.
[8] Bradley, "Miscellaneous Works", p. 208. Edmond Halley's practice was predicated on the belief that motions of less than four or five arc seconds could not be observed. James Bradley developed observational techniques which allowed him to calibrate motions of less than one arc second. Rigaud expressed views that were highly critical of Halley's observational abilities, and of Halley's expressed amusement at his protégé's claims at the observation of such fine angles. See S. P. Rigaud, *Memoirs of Bradley,* p. xxix.
[9] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. Book III, Proposition 14, Theorem 14. London, 1726. Translated by I Bernard Cohen and Anne Whitman, London, 1999, p819.
[10] Ibid. p.819.
[11] In 1718 Edmond Halley compared the observed positions of three bright stars, *Aldebaran, (ά* Tauri*), Arcturus, (ά* Boötis*)* and *Sirius (ά* Canis Majoris*),* and compared them with the positions recorded by Ptolemy in the *Almagest* or the *Syntaxis Mathematica.* In the belief that Ptolemy was sufficiently a dependable astronomer not to have made major errors in his location of these stars

Edmond Halley assumed that the stars had changed their positions in the meantime. Contrary to Newton's first corollary in Book I, Proposition 14, Theorem 14 of Book I of the *Principia,* Halley appeared to have claimed to have discovered the "proper motion" of the so-called "fixed stars".

[12] These shifts were observed between about 1669 and 1721 by Jean Picard, G. D. Cassini, Robert Hooke, John Flamsteed, Jacques Cassini, Jacques Maraldi, Eustachio Manfredi and others.

[13] Claims of parallax were sometimes abandoned by those making the initial claims. The claims of Jean Picard and Robert Hooke were either not seriously entertained or were abandoned in the face of general scepticism.

[14] ά Ursæ Minoris or *Polaris* is temporarily the pole star. As the Earth's axis precesses it will cease being the pole star in some 2,500 years time.

[15] From 1669 to 1670 Jean Picard surveyed the distance from Sourdon, near Amiens, to Malvoisine, south of Paris. He calibrated the distance of 57,060 toises (1 toise = app. 6.4 feet) for one degree of arc. It is a distance only 14 toises short of the modern accepted distance. It was sufficiently accurate to enable Isaac Newton to establish the *bone fides* of his universal theory of gravitation.

[16] Hven is now located in Sweden.

[17] Jean Picard, *Voyage d'Uranibourg,* Paris, 1671.

[18] G. Picolet (ed.) *Jean Picard et les débuts de l'astronomie de précision au XVIIe siècle: actes du colloque du tricentenaire.* Paris, 1987. Jean Picard and his early partner Adrien Auzout were early pioneers of the use of telescopic sights in France. Sometime in 1667 or 1668 they constructed a telescopic sight and attached it to a 38 inch quadrant.

[19] *Catholic Encyclopedia,* New York, 1911.Vol. 12, p. 73.

[20] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.*

[21] M. E. W. Williams, *Attempts to Measure Annual Stellar Parallax: Hooke to Bessel.* PhD Thesis, University of London, 1981. p. 27.

[22] John Wallis, *Opera Mathematica,* Oxford, 1699 Vol. III, pp 701 – 708. A letter from John Flamsteed to John Wallis, 20th December 1698. The original letter was written in English and appears to have been lost. Wallis translated it into Latin before publication. See M. E. W. Williams, p. 30.

[23] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in "Miscellaneous Works". pp. 1 – 16. Bradley includes some of Flamsteed's data in a postscript to the paper. In the third unpublished version, Bodleian Library, Bradley MS. 20*, fols. 18r – 30v, which I argue is the most authoritative account, edited and included as Appendix 3 of my MSc. Dissertation pp. 101 - 134; the Flamsteed data is included in the body of the work.

[24] Ibid. p 16.

[25] Jacques Cassini, *Réflexions sur une letter 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. pp 177 – 183.

[26] John Flamsteed to Sir Christopher Wren, 19th November 1702. Printed in S. Wren, *Parentalia; or memoirs of the family of the Wrens,* London, 1750. pp 247 – 253.

[27] Ibid. p. 252.

[28] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. Book III, Proposition 14, Theorem 14. London, 1726. Translated by I Bernard Cohen and Anne Whitman, London, 1999, p. 819.

[29] Eustachio Manfredi, *De Annuis Inerrantium Stellarum Aberrationibus,* Bologna, 1729. Manfredi's investigation of aberrations was largely completed by 1721. His treatise was written between then and 1729 when it was finally published. Manfredi refers to the work of Robert Hooke, Jean Picard, Ole Rømer, John Flamsteed, Jacques Cassini and Cassini's cousin Jacques Maraldi, as well as many observations made by him and several other astronomers.

[30] Aristotle, 259b, *Physics,* Book VIII, Chapter VI. Loeb ed. *Physics II,* pp 350 – 351. This is a difficult passage to interpret but the notion of bodies being moved by their souls is suggested.

[31] Including Francesco Algarotti.

[32] Manfredi accepted the veracity of Bradley's observations in declination and the law of the new discovered motion, but did not accept the hypothesis or the generally accepted belief that the new

discovered motion was evidence of the motion of the Earth. Bradley in turn accepted the veracity of Manfredi's observations in right ascension and conceded that the "anti-Copernicans" still had grounds to object to any notion that his observations established the heliocentric theory.

[33] Although Bradley and Manfredi accepted the veracity of their mutually compatible observations of motions in declination and right ascension, their radically opposed conclusions were the product of differing working practices and fundamentally different ways of evaluating evidences.

[34] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in "Miscellaneous Works". p.15. Bradley accepts that the anti-Copernicans possessed sufficient grounds to object to his hypothesis.

[35] Bodleian Library, Bradley MS 20*.

[36] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, 1729.

[37] Manfredi, ibid. The Latin text is, "Sunt tamen nonnulli, qui cum illas Stellarum evagationes statis anni temporibus recurrere animadvertant, minime dubitandum existiment quin ea res ad annuum, quem Tulluri cum Copernico tribuunt, motum pertineat, atque adeo vel hoc ipso indicio detectum, demonstratumque verum Mundi systema depraedicent, in quo tamen decipi proclive est, labante Copernicano Systemate una cum argumentis, quibus illi sussultum arbitrantur, id quod in hoc ipso opere demonstraturum me esse confide." pp. 1 – 2.

[38] Manfredi, ibid. Caput VIII, "De Stellarum aberrationibus observandis, et cum annua parallaxi conferendis; ac primùm in declinationem." pp 57 – 67.

[39] A letter from Flamsteed to Wallis, 20th December 1698, in Wallis, *Opera Mathematica,* Oxford, 1699 Vol. III, pp 701 – 708.

[40] ibid. p. 702.

[41] This was initially the "apparent opinion" of Bradley prior to the work he undertook with Molyneux and Graham at Kew from December 1725. My reconstruction of the Kew experiment suggests that Bradley entertained strong reservations.

[42] Manfredi, ibid. Caput VIII, Section 149, p. 62.

[43] Ibid. p.62, Unde distantia Stellae a Sole, sive sphaerae, in qua ipsa versantur, semidiameter eruetur, si fiat ut sinus scrup. 15 ad radium ita semidiameter orbis annui ad quasitam distantium, quae propteres erit ejusmodi semidiametrorum 13755."

[44] Ibid. Caput VIII, Section 150.

[45] Ibid. "in observatione dici 14, vel 16 Julii ipsa aberrationis plaga parallaxibus annuis repugnat, ab ea siquidem die ad 2 Decembris aberravit Stella ex observatione sec. 48 …… … Alia igitur errorum hujusce Stellae perquirenda videtur caussa, quam annia Telluris circa Solem conversion." p. 64.

[46] Colures are lines of longitude superimposed onto the celestial sphere.

[47] This is Jacques Maraldi who partnered his cousin Jacques Cassini on his geodesic survey of France.

[48] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus". The Latin reads, "Paullo operosior est earum aberrationum contemplatio, quae secundum ascensiones inerrantibus Stellis contingunt, cum scilicet eandem ascensionem rectam, sive arcum distantiae a coluris, minime servant, sed nonnihil ad ortum, vel ad occa sum evagantur, id quod nemini Astronomorum certo animadversum mihi constat, ante celeberrimum Maraldum, qui collatis Sirii, atque Arcturi, temporibus, quibus Meridianum pertranseunt, alium aliio anni diebus inter ea tempora differentiam invenit, deque eo literis suis me admonuit anno 1707; neque in his tantumodo Stellis, sed in aliis praeteria, ejusmodi excursiones ab se deprehensas significavit, quae res me quoque impulit, ut & in Marsiliana Sepecula cum Stancario, & in hoc publico Scientarum Instituti Observatorio cum Sociis, eandem indaginem aggrederer." p. 68.

[49] Manfredi's sustained examination of "aberrations" was very difficult to execute in right ascension, for he required both trustworthy clocks as well as efficient assistants to record the time at the moment the star was centred on the meridian. There was little room for manoeuvre. The accuracy of the clocks at his disposal was critical. Given these limitations his work was highly commendable.

[50] Peter Dear, *Discipline and Experience: The Mathematical Way in the Scientific Revolution,* London, 1995, p. 44.

[51] Ibid. p. 45.

[52] Ibid. p. 45.
[53] At the latitude of Gresham College it passes within 2 arc minutes of the zenith.
[54] Lisa Jardine, *On a Grander Scale: The Outstanding Career of Sir Christopher Wren,* London, 2002. In the preface to Jardine's biography of Christopher Wren she revealed how the Monument had been designed specifically to allow observation in the zenith.
[55]Bodleian Library, *Bradley MS 20*.* fol.18-18v. The third unpublished and final version of Bradley's paper.
[56] Hooke abandoned the claim.
[57] On the 6th and 9th July, 6th August and 21st October, 1669.
[58] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, 1729, p. 62.
[59] It is a problem that becomes ever more evident when Bradley's work at Kew and Wanstead is also examined in the various versions of his account.
[60] Newton, "Philosophiæ Naturalis Principia Mathematica", Book III, Prop 14, Theorem 14, Coro. 2. 3rd ed. London, 1726. Translated by I Bernard Cohen and Anne Whitman, London, 1999.
[61] Molyneux, "A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith". Included in the "Miscellaneous Works" pp 93 – 115.
[62] Bodleian Library, Bradley MS 14, marked K14, *The Kew Observation Book.*
[63] Ibid, fols 1 – 14v.
[64]Molyneux, "A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith". Included in the "Miscellaneous Works" p 99.
[65]Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Caput 1.
[66] Bodleian Library, Bradley MS 14, fol. 2v
[67] Bradley, "Directions for using the common Micrometer", included in "Miscellaneous Works" pp. 70 – 74.
[68] Ibid. pp. 70 – 71.
[69] Richard Hinckley Allen, *Star Names their Lore and Meaning,* (formerly *Star Names and their Meaning*) London, 1899. reprinted Dover Publications, New York, 1963. pp 207 – 209. The name of the star *Eltanin* is derived from Uleg Beg's *Al Rās al Tinnīn* (the Dragon's Head). Riccioli called it *Ras Eltanim*, whilst Bayer referred to *Rastaben.* In the Alphonsine Tables it is called *Rasaben.* It passes within two minutes of the zenith at the latitude of London and is occasionally referred to as the *Zenith Star.* In 2000BC it was the nearest bright star to the pole and was of great significance to the Egyptians and was variously referred to as *Apet, Bast, Mut, Sekhet* or *Taurt.* It was associated with *Isis* and the worship of that deity. It is designated as γ Draconis. It is a double star, a bright component of magnitude 2.4, and a companion of magnitude 13.2. The primary star possesses a distinct orange hue.
[70] Rigaud, "Memoirs", p. xvi.
[71] Ibid. p. xvi.
[72] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in "Miscellaneous Works" p. 2.
[73] Molyneux, "A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith." In "Miscellaneous Works", pp. 109 – 110.
[74] Bodleian Library, Bradley MS 14, K14, *Kew Observation Book,* fol. 10.
[75] Molyneux, Ibid. p. 109.
[76] The significance of this will become evident when James Bradley constructs his account of the work undertaken at Kew and Wanstead on the "new discovered motion".
[77] Rigaud, "Memoirs", p. xxix. Rigaud suggests that Halley lacked Bradley's skills as an observer.
[78] Bradley, "Observations of the Fixed Stars made at Wansted in Essex by J. Bradley." in the "Miscellaneous Works" pp. 201 – 286.
[79] Designated ά Aurigæ. It is a brilliant yellow-white star of magnitude 0.3. It is significant as the brightest near zenith star at the latitude of London. Although much more luminous than the Sun it belongs to the same spectral class on the main sequence. It passes within 6½° of the zenith.
[80] The terms "went out" and "going out" refer to the star moving out of the field of vision.

[81] Bradley, "Observations of the Fixed Stars made at Wansted in Essex by J. Bradley." in the "Miscellaneous Works" p. 208.
[82] Rigaud, "Memoirs", p. xxix.
[83] Shortly after Nevil Maskelyne became the 5th Astronomer Royal in 1764 he tested the zenith sector constructed for Bradley by Graham in 1727 and was confident that it was accurate to half a second of arc. It was used to collimate the instruments of the Royal Observatory before being sent to Cape Town from 1837 to 1839. It is now mounted on the west wall of the Transit House at the Old Royal Observatory at Greenwich.
[84] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works". p.3.
[85] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.* fol. 19.
[86] Bodleian Library, Bradley MS 14, *Kew Observation Book* K14, fol. 10v.
[87] Rigaud believed that this observation was so decisive that he had Bradley's record of it reproduced and included in the "Miscellaneous Works" as a facsimile.
[88] Rigaud, "Memoirs" p. xvii.
[89] K. V. Hewitt, *The Astronomical Work of the Rev. James Bradley,* MSc Dissertation, London, 1953. pp. 87 – 88.
[90] Jean-Baptiste-Joseph Delambre, *Histoire de l'Astronomie au 18e Siècle.* p. 427.
[91] Rigaud, "Memoirs", p. xvii.
[92] This assertion that the observation of the 20th December 1725 was "decisive" has left a permanent legacy, for the memorial to commemorate the "discovery" of the aberration of light was erected at Kew. Recently a small plaque was placed on a building close to where Bradley's instrument was erected at Wanstead, to commemorate the achievement. The Kew observations were only to acquire significance in the light of the later work performed by Bradley at Wanstead.
[93] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works" pp. 1 – 16.
[94] Delambre, "Histoire de l'Astronomie au 18e Siècle". p. 427.
[95] Bodleian Library, Bradley MS 20*, 3rd unpublished version, fol. 18.
[96] Bradley wrote at least four versions of his account. One account was published by the Royal Society, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars", No. 406, Vol. 35, pp. 637 – 661. There are three other versions at the Bodleian Library, Bradley MS 20*, all of which have been transcribed and edited and included as appendices to my MSc dissertation, "James Bradley and the New Discovered Motion: The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars", University of London, 1994. The third version was included as Appendix 3, and is in my considered opinion the intended final account.
[97] Molyneux died in his 39th year.
[98] Fisher, "James Bradley and the New Discovered Motion: The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars", University of London, 1994.
[99] Molyneux was Berkeley's student at Trinity College, Dublin from 1707 to 1710.
[100] George Berkeley, *A Treatise concerning the Principles of Human Knowledge.* London, 1980. Section 117, p. 113.
[101] Molyneux was made a Lord of the Admiralty when the Prince of Wales acceded to the throne as George II in June 1727.
[102] Although he had his income from his Savilian Professorship to support his standing as a gentleman, the outgoings incurred by his astronomical activities and living expenses at Oxford and Wanstead left him partially dependent on the financial support of his uncle's widow Elizabeth Pound.
[103] John Conduitt, King's College Library, *Memorabilia,* Cambridge, Keynes MS 130.5. fol. 3 – 4.
[104] Bodleian Library, Bradley MS 20*, "Drafts of Papers by James Bradley on Aberration" fol. 19
[105] Ibid. fol. 19v.
[106] Ibid. fol. 19v.
[107] Ibid. fol. 20r.

[108] If the Earth is moving in the direction A to B, then the laws of parallax will determine that an object such as a star will appear to progress in the direction B to A against a more distant background of other objects. The phenomenon that Bradley, Molyneux and Graham observed was completely counter-intuitive, for when the Earth moved A to B, the object star (in this case γ Draconis) also progressed A to B. In addition, the apparent motion induced by parallax was 90° out of phase with that displayed by the anomalous motion of the object star.
[109] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works" pp. 1-16.
[110] Ibid. p.3.
[111] If the control star was in the opposite sky, any motion in declination due to nutation would be similar for both the object and control stars. In fact the motion of the control star was only half that of the object star. The hypothesis of annual nutation therefore failed.
[112] Molyneux, "A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith", included in the "Miscellaneous Works" p. 98. There are some formerly loose papers in George Graham's hand, (now in the Kew Observation Book, K14) that were entitled by Samuel Molyneux as "Mr. Graham's dimensions of part of the parallax instrument", which includes the data, Radius = 24 feet 3½ inch, nearly, Screw 42 threads in one inch. Molyneux adds that the head was divided into 17 equal parts and each part was equal to one second on the radius of 24 feet 3.15 inches.
[113] Ibid. p. 98.
[114] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works". p. 11. Also in "Observations on the Fixed Stars made at Wansted in Essex, by J. Bradley", also in the "Miscellaneous Works." p. 212.
[115] Bessel, "Fundamenta Astronomiæ", p. 176.
[116] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works". p. 11.
[117] Bodleian Library, Bradley MS 14, *Kew Observation Book.* fol. 17 passim.
[118] Ibid. fol. 17 passim.
[119] Newton, "Philosophiæ Naturalis Principia Mathematica", Bk. III, Prop. 21, Theorem, 17. 3rd ed. 1726. Translated by I. Bernard Cohen and Anne Whitman, London, 1999. Newton wrote, "The equinoctial points regress, and the world's axis, by a nutation in every annual revolution, inclines twice toward the ecliptic and twice returns to its former position. This is clear by book I, prop 66, corol. 20. *This motion of nutation, however, must be very small – either scarcely or not at all perceptible.*" (My italics).
[120] Conduitt, "Memorabilia", King's College Library, Cambridge, Keynes MS 130.5. This approach must have been some time in January 1726, for by February the hypothesis of the annual nutation was simply unsustainable. The memorandum is dated 13th May 1730, over four years after the incident recalled, and over two years after the death of Samuel Molyneux. Conduitt's motive in recalling Molyneux's approach was to reveal Sir Isaac Newton in a good light. Conduitt recalled that Newton's response was magnanimous and generous, for he is reported to have said, "there is no arguing against facts and experiments".
[121] Rigaud, "Memoirs", p. vi.
[122] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration*, fol. 18.
[123] Bodleian Library, Bradley MS 14, *Kew Observation Book,* K14. Bradley evidently considered a positive result would pose some difficulties for Newtonian theory, and in his copy of the book he makes various calculations attempting to reconcile the possibility with Newtonian physics.
[124] Bodleian Library, Bradley MS 23, *James Pound's Account Book 1715 to 1724.* Pound lists expenses for various visits to Kew in order to confer with Molyneux during much of 1724. The fact that Pound travelled to Kew and not vice versa can be regarded as circumstantial evidence of Molyneux's primacy in the attempt to repeat Robert Hooke's experiment and to achieve Hooke's result.
[125] Bradley made various calculations in the back of his copy of the Kew Observation Book seeking to reconcile Hooke's *result* with Newton's concerns about the stability of the solar system.

[126] The question that must be raised if Molyneux held reservations about Newtonian theory is what was his motivation? He may have been convinced by the opinions of his friend George Berkeley but the truth of the matter is that Molyneux's possible antipathy is conjectural and difficult to verify. Most of the evidence is circumstantial. The question that forces itself on anyone prepared to study the matter in any detail is simply why did Molyneux approach Newton, when it is possible to determine from the observations made through February 1726 that the conjecture of an annual nutation of some 40" (20" in modern practice) was simply unsustainable after a couple of weeks? It surely suggests that Molyneux simply couldn't restrain himself when he recognised the distinct possibility that he could challenge Newtonian theory, especially after Hooke's observations had been so quickly disposed of. He obviously approached Newton precipitously in order to inform him that his system was "destroyed completely". This is hardly the conduct of a man who supported Newton's system.

[127] Rigaud, "Memoirs", p.xx.

[128] There is a rough sketch in Bradley MS. 20* *Drafts of Papers by James Bradley on Aberration*, fol. 70.

[129] Bradley was to suppress all public discussion of the lunar associated nutation for several years, keeping knowledge of it to a few trusted co-workers and friends like Graham and Machin. Though he observed the phenomenon from 1727, he did not publicise it until 1737, and did not publish his paper until 1748. Bradley would surely have disapproved of his senior partner's approach to Newton of all people.

[130] Conduitt, "Memorabilia", King's College Library, Cambridge, Keynes MS 130.5. fol. 3.

[131] Even though Hooke had abandoned his claim, and that it had been effectively countered by Jean Picard's analysis of Hooke's claim. That an astronomer such as James Bradley could seriously have accepted the validity of Hooke's result on the basis of four observations spread over just three and a half months is impossible to reconcile with his practice. This suggests that Bradley's reported acceptance of the Hooke result is not what it seems. Bradley is seeking to justify himself when he has clearly recognised that the whole project is tinged with an anti-Newtonian bias. He seeks to argue that his partnership in the investigation was undertaken in a spirit of pure inquiry, free from bias or prejudice.

[132] Newton, "Philosophiæ Naturalis Principia Mathematica", Book 2, Section 1, Prop. 4, Problem 2, reveals that the amount of resistance encountered in a fluid medium to create such an effect would probably lead to the earth and the other planets gradually spiralling into the Sun.

[133] Rigaud, "Memoirs", p. xxi.

[134] Including archives of the Molyneux family at Southampton Records Office.

[135] There are some notes in Bradley MS20* *Drafts of Papers by James Bradley on Aberration*, fol. 82.

[136] The dates of the two hypotheses according to Rigaud were 18th June and 2nd July 1726. I find both of these dates unsustainable, for they simply cannot be reconciled with the evidences offered by the Kew Observation Book. Molyneux and Bradley had by mid February 1726 been forced to abandon all pretensions to the nutation because by that date the motions of 35 Camelopardalis did not support the conjecture. The hypothesis of the refraction was mere conjecture and would hardly have been entertained seriously by a committed Newtonian like Bradley, whose familiarity with the second book of the *Principia* would have revealed its inadequacy.

[137] Rigaud, "Memoirs", p. xxii.

[138] Ibid. p. xxii.

[139] Ibid. p. xxii.

[140] The incapacity of the experimental set-up to observe other bright stars as controls revealed its obvious shortcomings to an astute observer like Bradley long before the first month of observation was complete.

[141] The function of the zenith sector is to measure the drift in declination of a star as it passes through the meridian close to the zenith. The function of a transit instrument is to observe the passage of a star as it passes through the meridian. The measurement of motion in declination and right ascension is possible, the latter in conjunction with an accurate timepiece.

[142] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works", p. 4.

[143] James Bradley, "Memoranda respecting the instrument at Wanstead" in the "Miscellaneous Works", p. 194.
[144] Ibid. pp. 194 – 195.
[145] Maskelyne, "Astronomical Observations, 1766 – 1774, Explanation and use of tables". London, 1776. Vol. 1. p.ix.
[146] The character of the investigation at Kew, which may be interpreted in anti-Newtonian terms in the sense that both the original objective and the two main hypotheses were overtly directed against the validity of Newtonian theory, was further underlined because the methodology of the experiment was so restricted that after the failure to observe parallax it became little more than a conjectural search for causes, giving the investigation a pre-Newtonian bent with more than a passing nod to some forms of Cartesian enquiry.
[147] Rigaud, "Memoirs", p. xii.
[148] Bradley's procedure is clearly "Newtonian" in seeking to delineate a motion purely in terms of a mathematical analysis, setting aside a search for causes in the pursuit of the determination of the parameters of a natural phenomenon.
[149] The star *Capella*, designated ά Aurigæ, has a magnitude of 0.08.
[150] As I have already asserted, the programme was based upon a practice that was developed under the combined tutelage of James Pound and Edmond Halley.
[151] The motion had been observed by Hooke, Picard, G. D. Cassini, Flamsteed, J. Cassini, Maraldi, Manfredi and others.
[152] As did Robert Hooke in "An Attempt to Prove the Motion of the Earth from Observations". This is contained in R. T. Gunther, *Early Science in Oxford,* Vol. 8, pp. 17 – 25.
[153] Which was the explanation proffered by Jean Picard after observing various "strayings" of *Polaris*. See Jean Picard, "Voyage d'Uranibourg", Paris, 1671.
[154] Jacques Cassini, *Réflexions sur une letter 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. pp 177 – 183.
Cassini effectively countered the claim made by Flamsteed for the discovery of the annual parallax of *Polaris* during 1696 in a letter to Wallis. Neither Cassini nor Flamsteed, having determined that the motion could not possibly have been due to annual parallax then made any known attempt to explain it in terms of some other cause.
[155] The word "aberration" implies a deviation from the norm. In Manfredi's view such aberrations were deviations from the true "fixity" of the stars.
[156] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, 1729.
[157] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works", p. 4.
[158] Bradley did not initially make any claim for an explanation of the phenomenon except to make a conjecture based on his established astronomical practice, which accepted that the planets went around the Sun and that light possessed a finite velocity. He was concerned at first with an attempt to measure the parameters of what he was certain was a general phenomenon that he believed would be observed in all the stars. It was merely "a new discovered motion of the fixed stars".
[159] It was demolished in 1802 – 03. It was known for most of the 18th century as the White House and was regularly used by George III as a residence.
[160] Bradley, "Memoranda respecting the instrument at Wansted." in the "Miscellaneous Works", p. 195.
[161] Molyneux, "A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith." Contained in Bradley, "Miscellaneous Works", p. 93.
[162] These factors included the height of the bench he would lie on as he observed the stars through the suspended telescope, but one of the factors may have been the acquisition of a glass with a focal length of 12½ feet early in 1727. At this time Bradley's finances were such that his decisions were constrained by the need to live as much as possible within his means.
[163] In fact the use of the sector to observe stars as far from the zenith as the 6¼° of *Capella* when Bradley later observed the nutation of the Earth's axis led to some distortion, for Bradley was forced to abandon the results of the observations of stars at the limits of the sector.

[164] It was certainly reliable enough for the observations of aberration. It was when he was observing the much more exacting phenomenon of the nutation that he began to lose faith in the veracity of his results when observing at the angular limits of the sector due to the effects of the weight of the instrument on the integrity of its structure especially when measuring angles as small as one arc second.

[165] Wil Tirion, Barry Rappaport and George Lovi, *Uranometria 2000.0*: Volume I: The Northern Hemisphere to - 6° and Volume II: The Southern Hemisphere to + 6°, Richmond, Virginia, 1987.

[166] I recognise that the positions of the stars listed are all for epoch 2000. In some cases the difference in the positions may differ perceptibly from what they were in 1727. In the terms of this study, I seek only to indicate their identity and their relative distribution in terms of their right ascension and declination, together with their apparent magnitudes.

[167] The limit can vary according to seeing conditions, but an analysis of Bradley's observation books reveals that he was capable of observing stars down to the fourth magnitude during daylight hours. Given that Bradley used a two inch glass, with all but the central inch stopped, this is a remarkable achievement. My own attempts at observation during the day using a small 2 inch refractor lead me to the conclusion that the observation of objects of even the second magnitude is something of an achievement given the amount of pollution in London skies. During the day the faintest objects I have observed were of the first and second magnitude. Bradley's results are in part a measure of his great skills as an observer, and the comparative clarity of the sky in Wanstead during the 1720s.

[168] Bodleian Library, Bradley MS 14, *Kew Observation Book,* K14.

[169] Ibid. fol. 136.

[170] I am convinced that the control star that was referred to as "Anti-Draco" at Kew is the star identified as 35 Camelopardalis in Flamsteed's catalogue. As both Bradley and Molyneux were intending to compare their observations at Wanstead and Kew, one of the stars that Bradley was observing at Wanstead had to be identical to the former control star. This star had to be in the opposite area of the sky to γ Draconis, and also had to be within reach of the zenith sector at Kew, which could only be moved up to 7 or 8 minutes of arc from the zenith. Only one of Bradley's 69 stars fits the criterion and this is 35 Camelopardalis. γ Draconis is at RA 17h 56m 38m / Dec 51° 29' 22" / mag. 3.9 and 35 Camelopardalis RA 06h 04m 35m / Dec 51° 34' / mag. 6.

[171] These were all of γ Draconis at 5.37am 22nd August, 1727, 5.36am 3rd September, 5.36am 4th September, 5.34am 23rd September, 5.33am 27th September, and finally at 5.43am on 29th December, 1727.

[172] Molyneux was appointed on 2nd August, 1727. This is the reason why in his account of the new discovered motion Bradley referred to Molyneux as the "Honorable".

[173] *Dictionary of National Biography,* Vol. 13, London, 1882. pp. 583 – 585. Molyneux collapsed in the chamber of the House of Commons a few days before his death. See S. P. Rigaud, *Memoirs of Bradley,* p. xxviii, who states that he died of a "brain seizure". An 1864 edition of *Kew Past,* 1864: 7747, reveals that during the last few days of his life Molyneux was attended by the anatomist to the Royal Family, Dr. Nathaniel St. Andre who was also qualified as a dancing master. Whether or not he strove to help Molyneux to recover is open to debate, for within a few days of his death, his widow Lady Elizabeth, who was evidently impressed, eloped with St. Andre who was later charged with Molyneux's murder. Although he was acquitted he remained subject to ridicule from many writers, including Samuel Johnson.

[174] Bodleian Library, Bradley MS 14, *Kew Observation Book,* K14. fol. 138.

[175] Rigaud, "Memoirs" p. xxviii. It wasn't repaired partly because Molyneux had become more involved with public affairs, both at the Admiralty and at Parliament, and partly because Bradley's instrument at Wanstead was now receiving the close attention of George Graham.

[176] Bradley, "Zenith Observations at Wanstead", included in the "Miscellaneous Works", reveals visits by Halley on the 1st and 2nd September, 1727, and on 21st December, 1728 (when he was conferring with Bradley on his written account). There are records of visits by Lord C. Cavendish on 25th June, 1728 (when Bradley had already determined the law of the new discovered motion) and by Dr. Benjamin Hoadly (the son of Bishop Hoadly) on 26th June, 1729 and 23rd August, 1729 (after Bradley continued his observations, convinced that there was another phenomenon hidden in the residuals of his observations of aberration).

[177] Bradley, "Memoranda respecting the instrument at Wansted", contained in the "Miscellaneous Works". p. 194. In a perfect glass the optical centre will coincide with the centre of magnitude, at the principle axis of the lens. The path of a ray of light along this axis will be unchanged.
[178] Bradley, "Zenith Observations at Wansted", contained in the "Miscellaneous Works". p. 203.
[179] Bradley, "Observations at Kew", contained in the "Miscellaneous Works", p. 150.
[180] Ibid. p. 151.
[181] Bradley, "Zenith Observations at Wansted", contained in the "Miscellaneous Works". p. 212.
[182] Ibid. p. 219.
[183] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal, and etc., giving an account of a new-discovered Motion of the Fixed Stars", contained in the "Miscellaneous Works". p.6.
[184] Rigaud, "Memoirs", p.xxx.
[185] Thomson, "A History of the Royal Society", London, 1812.
[186] I can imagine Bradley being in despair, but certainly not after such a short period of investigation, and particularly not until he had investigated the phenomenon for a complete year. Both his account and his observation book record his decision to delay any conclusions until he had investigated the phenomenon for the year. It is entirely in accord with the rest of his practice.
[187] The account of Bradley's "discovery of the aberration of light" given by Thomson can characterised as a "romantic" construction in the way that the use of the "imagination" is extolled in a "sudden" insight. The account reveals more about Thomson and his presuppositions, than it does about Bradley and his methods.
[188] The problem is that Bradley's account is not a simple narrative, but a consummate piece of propaganda constructed for an elite that would both support his conclusions and advance his social opportunities. This will be discussed in the next section.
[189] Bradley, "Reduction of the Wansted Observations", in the "Miscellaneous Works", p. 302.
[190] Ibid. p. 303.
[191] Ibid. p. 303.
[192] Ibid. p. 303.
[193] Ibid. p. 303.
[194] Ibid. p. 304.
[195] Bradley, "Observations at made at Kew", in the "Miscellaneous Works". p. 183.
[196] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 5.
[197] Ibid. p. 5.
[198] The term "maxima of aberration" is anachronous. At this stage of Bradley's investigation it can only be termed as "maxima of apparent motion".
[199] Rigaud, "Memoirs", p. xxxiii.
[200] Bradley, "Zenith Observations at Wansted", in the "Miscellaneous Works", pp. 242 – 248.
[201] Bradley, "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in the "Miscellaneous Works", pp. 287 – 301.
[202] Rigaud, "Memoirs", p. xxxiii.
[203] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 15.
[204] Bradley, "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in the "Miscellaneous Works", pp. 287 – 301.
[205] After he had observed the phenomenon in about 40 stars over about six months.
[206] Rigaud, "Memoirs", p. xxxiii.
[207] Bradley, "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in the "Miscellaneous Works", pp. 287 – 301.
[208] Rigaud, "Memoirs", p. xxxiv.
[209] I argue that internal evidence from the various versions of the paper on "the new discovered motion" or "aberration" suggests that they were written either contemporaneously with or just after Bradley had penned his demonstration of the law of the new motion. Given the technical details he presented in the "discovery" paper, written during December 1728, Bradley must have written out a demonstration prior to it.

[210] Bradley, "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in the "Miscellaneous Works". p. 290.
[211] See also my M.Sc. dissertation, Fisher, "James Bradley and the New Discovered Motion: The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars", University of London, 1994, p. 136, "Two Transitional Aberration Documents - Document A", See also Bodleian Library, Bradley MS 20* *Drafts of Papers by James Bradley on Aberration.*
[212] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, 1729.
[213] Bradley, "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in the "Miscellaneous Works", p. 287.
[214] The version of the paper published by Rigaud in the "Miscellaneous Works", is a later re-writing of a rough version found in Bradley MS 20* The version used by Rigaud is probably impossible to date, but may date from the early 1730's when Bradley was revising the theory of aberration at a time when he was beginning to observe the nutation of the earth's axis. It is this which may have misled Rigaud to think that the law of the motion followed the hypothesis.
[215] Bradley, "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in the "Miscellaneous Works", p. 287.
[216] This is a characteristically Newtonian procedure, for Bradley averred from any *ad hoc* solution, convinced that he was seeking the solution to a general law or phenomenon. He had only observed a few stars and yet he induced from these results that he was elucidating a universal phenomenon.
[217] This is the modern value of the constant of aberration.
[218] Unless Thomson and Rigaud still possessed access to a still living oral tradition there was no justification for accepting the story of the pleasure trip on the Thames. The account offered by Bradley appears adequate for his purposes, although as I will discuss, it was not a simple narrative but a well reconstructed account which sought both justification and support. The question remains that if such an oral tradition had survived into the 19th century why it wasn't retained. Both Berry and I have failed to find any documentary evidence in support of the Thomson account.
[219] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", pp. 1 – 16.
[220] The version of the demonstration of the aberration of light published by Rigaud in the "Miscellaneous Works", pp. 287 – 301, was a later version in neat hand. There are various scraps of an earlier version in the aberration papers, Bodleian Library, Bradley MS 20*. The law preceded Bradley's full expression of the hypothesis, though there is every reason to suggest that both the law and the hypothesis developed side by side, for in their fully developed forms they owe much to each other.
[221] Bradley and Halley, "Tables of Jupiter's Satellites", London, 1752.
[222] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", pp. 1 – 16.
[223] Ibid. p. 7.
[224] Ibid. p. 7.
[225] A view almost universally held astronomers by 1730, except for a handful geocentrically committed astronomers such as Eustachio Manfredi.
[226] An assumption that had all but been conceded by all natural philosophers in the west except for a handful of latter day Peripatetics and a few persistent Cartesians.
[227] An almost universally held assumption since before Euclid published his *Optics*.
[228] The notion of the universality of natural phenomena is one of the most fundamental of all assumptions. Its acceptance is necessary if scientific investigation is to be accepted as a rational activity.
[229] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 15.
[230] Bradley, Observations of γ Draconis in "Reduction of the Wansted Observations", in the "Miscellaneous Works", pp. 302 – 309.
[231] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 13.

[232] Bradley, Observations of η Ursæ Majoris in "Reduction of the Wansted Observations", in the "Miscellaneous Works", pp. 334 – 338.
[233] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 14.
[234] For instance γ Draconis on the 5th June 1728, when the observation of the difference in declination was 18½" and the hypothesis predicted a difference of 20".
[235] James Bradley estimated that the velocity of light was (using metric measures) 297,500 km.p.s. Modern estimates give a velocity of 299,792.458 km.p.s.
[236] Rømer's estimate would give a velocity of about 234,000 km.p.s.
[237] Olé Rømer, *Memoirs de l'Accademie des Sciences,* Paris, 1676. Vol. I, p. 214, and Vol. X, p.577.
[238] Rigaud, "Memoirs", p.xxxvi.
[239] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", pp. 1 – 16. That such important items as Bradley's disavowal of Robert Hooke's results, in direct contradiction of his approbation of these results on the first page of his account, and his incorporation of John Flamsteed's data from his observations of the pole star in 1696 as a postscript suggests the account was written hurriedly in order to have it ready for the 9th January meeting of the Royal Society. In the final unpublished version, reproduced as Appendix 3 of my MSc dissertation in 1994 these observations of Flamsteed and work undertaken by Halley on the extinction rates of first magnitude stars in occultations by the moon, is incorporated in the body of the account.
[240] Wallis, "Opera Mathematica", Vol. III, pp 701 – 708, Oxford, 1699. A letter from Flamsteed to Wallis, 20th December 1698. The original letter was written in English and appears to have been lost. Wallis translated it into Latin before publication.
[241] Read at meetings of the Royal Society on 9th and 16th January 1728/29 OS.
[242] Fisher, "James Bradley and the New Discovered Motion : The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars", M.Sc. Dissertation, Imperial College, University of London, 1994.
[243] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 1.
[244] Bradley, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration,* fol. 68, Bradley stated, "According to my Observations the Star would have been 5½" more northerly Oct 21 than 'twas July ye 7th or 8th whereas Dr. Hook make it 23" more Southerly Diff = 28½". And by my Observations twas 2" more Southerly on Oct 21 than on August 6th whereas Dr Hook makes it then 17" more southerly that is 15" Difference from mine."
[245] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 16.
[246] There are strong suspicions that Molyneux may have harboured some strong reservations about the validity of Newtonian theory. This includes the motivation of seeking to verify Hooke's *result*, ie. a large annual parallax that was at odds with Newton's own strictures that the stars were too distant to display sensible annual parallaxes, and the apparent readiness to approach Newton when it appeared that the "discovery" of a large nutation equally invalidated Newtonian theory. It is of course impossible to state why Molyneux may have been ill-disposed to Newtonian theory, though his teacher at Trinity College, George Berkeley, was one of the most incisive critics of some aspects of Newtonian philosophy on theological grounds, and he remained a friend of Molyneux.
[247] Kings College Library, Cambridge. Conduitt, "Memorabilia", Keynes MS. 130.5, fol. 3.
[248] Bradley was able to capitalise on the backing of powerful sources of patronage when he acquired the lectureship in experimental philosophy later in 1729. As he was able to charge his students to attend his lectures, this became a lucrative source of income which gave Bradley greater financial independence. The election will be discussed in Chapter 3 on Bradley's activities at Oxford.
[249] Kings College Library, Cambridge. Conduitt, "Memorabilia", Keynes MS. 130.5, fol. 3.
[250] The search for a control star began early in January 1726. By mid-February 1726 it was already obvious that the control star was not behaving in a way that supported the observation of a nutation.

[251] As well as being the Prince of Wales' private secretary, Molyneux was a member of both the British and Irish Parliaments.
[252] As he had been since Pound had died prematurely and intestate.
[253] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 1.
[254] Hooke, "An Attempt to Prove the Motion of the Earth from Observations", contained in R. T. Gunther, "Early Science in Oxford", Vol. 8, pp 17 – 25. It is also to be found in *Royal Society Classified Papers,* 20, No. 61, ff. 129 – 136.
[255] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works" p. 1.
[256] Bodleian Library, Bradley MS 20*, fol. 68. Bradley records the four observations made by Robert Hooke in pursuit of his measurement of the annual parallax of the circumpolar star, γ Draconis. These were made on the 6th and 9th July, 6th August and 21st October, 1669.
[257] Hooke abandoned his claim in the face of mounting criticism.
[258] More to the point it was well known to Halley the recipient of the paper.
[259] It is inconceivable that Bradley didn't possess doubts about the characteristics of the programme he was engaged in at Kew, even if he maintained equitable relations with his senior partner. It is not possible to ascertain whether he was aware of Molyneux's approach to Conduitt and Newton and if he had been he would have advised caution. If he was apprised of Molyneux's approach later his relations with his friend would certainly have led to an undermining of trust and approbation. There is no evidence that Bradley and Molyneux met between 19th August, 1727 when the Wanstead programme began and 28th April, 1728 when Molyneux died. Molyneux may have been busy as one of the Lords of the Admiralty but surely not so busy not to attend an experiment that must have regarded as an extension of his own. It is instructive to note that although Molyneux offered his continued observations at Kew to Bradley, he did not make any use of them. Bradley used only his Wanstead data to support his hypothesis. All points to a cooling of relations between the former partners.
[260] Bradley wrote of Molyneux in circumspect if respectful terms, whilst his references to Graham are fulsome with praise and admiration. Bradley extols Graham's disinterested curiosity.
[261] With this statement Bradley absolves himself of any responsibility for Molyneux's approach to Newton whilst acknowledging that such a hypothesis had been entertained.
[262] This passage was included for completeness. Bradley's entire methodology involved almost constant rectification and correction, so it is not really a serious consideration at all. It is unlikely that he seriously entertained this as a possible cause of the observed phenomenon in so many different stars.
[263] As with his statement about a nutation, Bradley is now disposing of a conjecture proposed at Kew, without any consideration of how such a conjecture might possibly challenge Newtonian orthodoxy.
[264] Here Bradley gives the reader the impression that the hypothesis came to him quite suddenly, and it may have been this which allowed Rigaud to give credence to Thomson's account of the boating party, but the evidence of his observations suggests that the realization was a gradual process, connected to the development of his discernment of the law of the new discovered motion. The law was based on a developed practice which stemmed from his work on the Jovian ephemerides and the introduction of a variable to Halley's Tables, which was based on the application of the notion that light possessed a finite velocity and that the Earth moved in an annual orbit around the Sun. It is difficult to understand why Rigaud thought that the hypothesis preceded Bradley's apprehension of the law of the new discovered motion.
[265] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 6.
[266] These various conjectures are presented as possibilities that were briefly considered and then rejected or eliminated once the observational evidences failed to support them.
[267] The passage may offer an insight into Bradley's successful pursuit of patronage. Working in partnership with an astute politician it would appear that Bradley was as skilful in the arts of persuasion as his later success in society may suggest.

[268] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal etc., giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", pp. 1 – 16.
[269] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.*
[270] Jacques Cassini, *Réflexions sur une letter 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. pp 177 – 183.
[271] The distance of 400,000 AU was a consequence of Bradley's assertion that the upper limit of any expected parallax was 1 arc second. In modern practice, which utilizes the mean radius of the Earth's orbit as the baseline, this corresponds to an annual parallax of 0.500 arc seconds. α Centauri (the nearest stellar system to the Sun) has an annual parallax of 0.753 arc seconds.
[272] From the inception of the telescopic observation of the stars, it was initially a puzzle why it was that that planetary images increased with diameter with increasing powers of magnification, whilst stellar images decreased their apparent diameter. It was recognised that stellar images were largely a consequence of telescopic artefacts which exaggerated their apparent sizes. Yet it was believed that their diameters were sensible. Bradley's reasoning, however, when combined with the results of his observations, suggested that it was not possible to "see" the stars, for an estimated diameter of 1/200$^{th}$ of an arc second was much too small to be observed by the human eye, even with instrumental aid.
[273] With an upper parallactic displacement of one arc second a stellar diameter would infer a diameter equal to the diameter equal to that of the earth's orbit. Of course Bradley was not to be aware of Giants and Supergiants with diameters in excess of 2 AU.
[274] Bradley, Bodleian Library, Bradley MS 20*, *Drafts of papers by James Bradley on Aberration.* fol. 29v – fol. 30r.
[275] Rigaud, "Memoirs", p. xxix. Halley worked to limits of about 5 arc-seconds He expressly wrote, "ut verum fatear, minuta secunda vel etiam dena secunda instrumentis quantumvis affabre factis certo distinguere vix homini datum est." Rigaud remarked, "Halley never believed a word of the doctrine of nutation, although he lived until 1742, when Bradley had already continued his observations for a series of many years, in which he had found the constant operation of this effect."
[276] Allan Chapman, *The Accuracy of Angular Measuring Instruments used in Astronomy between 1500 and 1850, JHA*, Vol. XIV (1983), pp. 133 – 137. Allan Chapman states, "The micrometer …extended angular resolving power, either in the form of the filar micrometer, set in the field of the telescope or else adapted as a fine adjusting screw onto the limbs of quadrants. In consequence, the one minute of arc ceiling of 1660 was extended to around 15" by 1700, and 8" by 1725, thus making it possible to detect such small quantities as were necessary to discover the aberration of light, and the nutation."
[277] Derek Howse, *Greenwich Observatory: Volume 3: The Buildings and Instruments.* Derek Howse states, "The micrometer on the zenith sector constructed for James Bradley by George Graham possessed graduations a fraction less than 1" apart. A complete revolution of the screw corresponded to an angle of 33.6". p. 61.
[278] Cambridge University Library, Royal Greenwich Observatory Archives, RGO, 2/9, fols 189 – 164 (pages reversed in binding), and fols. 25 -26. Halley also published these observations in T. Street, *Astronomia Carolina* , 3$^{rd}$ ed. "To which is added a series of observations on the planets, chiefly of the Moon, made near London with a sextant of near six foot radius, in order to find out the lunar theory a posteriori. Being a proposal how to find the longitude & etc By Dr. Edmund Halley", London, 1716.
[279] Few if any attempts were made to measure annual stellar parallax during the 18$^{th}$ century after the publication of Bradley's paper on the new discovered motion.
[280] Maupertuis to Bradley, "Miscellaneous Works", pp. 404 – 406, in a letter addressed to Bradley by Maupertuis on 27$^{th}$ September, 1737 he refers to Bradley's "votre belle theorie", which Bradley rendered into English as "your curious theory". The adoption of the term "aberration of light" was a gradual process.
[281] Bodleian Library, Bradley MS 20* *Drafts of Papers by James Bradley on Aberration*, fol. 56. This is the earliest document I have found where Bradley uses the term "aberration". He uses the

term hesitantly, and indeed he used the term "parallax arising from the motion of light" before changing it to "aberration arising from the motion of light".

[282] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration*, fol. 60. "The parallax of the stars on account of the motion of light" was Bradley's transitional term to describe the hypothesis and the law of the new discovered motion prior to describing it as "the aberration or parallax of the stars arising from the motion of light".

[283] Annual parallax on account of distance is "annual parallax".

[284] This refers to "annual aberration".

[285] This is "annual parallax".

[286] This refers to "annual aberration".

[287] This refers to three of the zodiacal constellations. It reveals that annual aberration is 90° out of phase with annual parallax. It shows why Bradley observed a rapid shift in declination of $\gamma$ Draconis in December 1725 at a time of the year when little motion was expected because of annual parallax.

[288] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration* fol. 56v.

[289] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, 1729.

[290] Ibid. pp. 1 – 2. The Latin text reads, "Sunt tamen nonnulli, qui cum illas Stellarum evagationes statis anni temporibus recurrere animadvertant, minime dubitandum existiment quin ea res ad annuum, quem Telluri cum Copernico tribuunt, motum pertineat, atque adeo vel hoc ipso indicio detectum, demonstratumque verum Mundi systema depraedicent, in quo tamen decipi proclive est, labante Copernicano Systemate una cum argumentis, quibus illi sussultem arbitrantur, in quod in hoc ipso opere demonstraturum me esse confide."

[291] Manfredi to Antonio Leprotti, *Comm. Acad. Bononiensis,* Vol. 1, 1748, p.634. Cited in Rigaud, "Memoirs", p.xxxiii.

[292] Rigaud, "Memoirs", p.xxxiii.

[293] Andrea Rusnock, "Correspondence networks and the Royal Society, 1700 – 1750", *BJHS*, (1999) Vol. 32, pp. 155 – 169. Sir Thomas Dereham was a Roman Catholic who lived in Italy and was in regular correspondence with James Jurin, who was Secretary of the Royal Society during the final years of Sir Isaac Newton's presidency. Dereham was in regular correspondence with various Italian natural philosophers, including Eustachio Manfredi. He must not be confused with William Derham, vicar of Upminster in Essex, the author of *Physico-Theology* in 1713 and *Astro-Theology* in 1714.

[294] Bodleian Library, Bradley MS 20*, *Drafts of Papers by James Bradley on Aberration.* fols. 56 – 60.

[295] Ibid. fols. 56 – 66 passim.

[296] Particularly as Manfredi himself admitted that the motions he had observed in right ascension were analogous to the motions that Bradley had observed in declination.

[297] Bradley to Graham, "Miscellaneous Works", pp. 406 – 408. Dated between the letter from Maupertuis to Bradley dated 27th September NS and the letter written in reply dated 27th October OS.

[298] Maupertuis led an expedition to Lapland in order to make observations using a zenith sector designed and constructed by George Graham in order resolve the dispute over the figure of the earth. Those members of the Académie Royale des Sciences who supported the Cartesian position claimed the Earth was a prolate spheroid, whilst those (including Maupertuis, Clairaut and Celsius) who supported the Newtonian position claimed that it was an oblate spheroid. The results of the measurements, including Bradley's adjustments due to aberration and nutation were published in Maupertuis' book, *La Figure de la Terre,* Paris, 1738, published in English as *The Figure of the Earth,* London, 1738. The expedition purported to provide evidence in support of the Newtonian position.

[299] Once a phenomenon has been identified and further, it is used to correct other observations, it may be accepted that it has been established as an objectively identifiable process. In my MSc dissertation, "James Bradley and the New Discovered Motion: The Origins, Development and Reification of the New Discovered Motion of the Fixed Star". I trace the process whereby the new discovered motion was reified until it was accepted as a physical process, with a distinctive theoretical structure that clearly distinguished it from annual parallax.

[300] No one has ever "observed" annual aberration, any more than anyone has ever "observed" annual parallax. Both are concepts that are derived from patterns of observation based on conjectural expectations. The Kew experiment was structured on the expectations of measuring the motion of γ Draconis on the basis of the theory of parallax. The Wanstead programme was structured on the expectation of repeating the observed behaviour of γ Draconis at Kew in all the other stars the instrument was designed to observe. Bradley did not observe aberration at Kew in December 1725, any more than any of his eminent predecessors had, he observed an *anomaly* which did not conform to the theory of parallax.

[301] It is the belief that the aberration of light was "observed" by Bradley in December 1725 that led to the erection of the memorial at Kew to commemorate the "discovery". The phenomenon later identified as the aberration of light required a new theoretical construct in order to transform the observations made in December 1725 into the "discovery" of a newly identified physical phenomenon.

[302] Manfredi's "aberrations" were anomalous because his intention was to demonstrate that these motions did not conform to the theory of parallax. The anomalous character of the aberrations he observed was interpreted in terms of the practice of many contemporary Jesuits which would have regarded them as "monsters". Much as Galileo's reported observations in the *Sidereus Nuncius* were regarded as such monsters before they were assimilated into the practice of astronomers, then so the observations of "aberrations" were regarded as unaccountable exceptions to assimilated doctrine, as "monsters". See Dear, "Discipline and Experience: The Mathematical Way in the Scientific Revolution", p. 14 and passim.

[303] The socialisation of knowledge is here signified by the general acceptance of the validity of such knowledge claims.

[304] It was an idea that conformed to the expectations of the contemporary astronomical community, for whom the Copernican cosmos was accepted as a given. There were still a few latter day geocentric astronomers, but their voice was regarded increasingly as eccentric or as constrained by theological considerations.

[305] And it has to be accepted that "annual parallax" is equally a conceptual construct embedded within a complex of theory and practice. It is no more possible to "observe" annual parallax than annual aberration. They are both concepts or ideas, and ideas cannot be observed.

[306] Bradley's primary purpose as a positional astronomer was the elimination of the degrees of uncertainty in the derivation of the position of astronomical objects. To the instrumental causes of error such as chromatic and spherical aberration and faults in instrumental construction were added the problems associated with atmospheric refraction and precession. Bradley's identification of a regular motion which could account for variations of up to 40 arc seconds a year was part of a process whereby the causes of positional variation could be accounted for and predicted.

[307] A variation of 40 arc seconds accounted for a major proportion of the one minute of arc demanded by the 1714 Longitude Act.

[308] It was the way objects behaved, not the causes of that behaviour, which was becoming increasingly important in the new mathematically based physics that Bradley ascribed to. As such he was in every sense the quintessential Newtonian astronomer.

[309] Many even asserted that the "discovery" of the aberration of light (as opposed to Bradley's construction of the theory which underlay his observations) proved the motion of the Earth.

# Chapter Three.

## Wanstead: The Work on Nutation.[1]

### The Precision and Accuracy of Bradley's Work.

By March or April 1728, at the time when he was beginning to understand the parameters of the law of the new discovered motion, Bradley was also aware that his observations were not in conformity to prediction. The apparent anomalies amounted to no more than a second or a second and a half of arc. Bradley's accurate and precise astronomical work would not have been possible without the attendant craftsmanship of George Graham. Bradley was unstinting in his praise of a man he always regarded as his co-worker. In his paper on the aberration Bradley wrote that the accuracy and precision of his observations were

> owing to our curious member Mr Graham, to whom the lovers of our science are also not a little indebted for several exact and well contrived instruments. The necessity of such will scarcely be disputed by those that have had any experience in making astronomical observations;[2]

Bradley and Graham were partners in every sense of the term, for the precision of Graham's craftsmanship was matched by Bradley's technique. This partnership has been detailed by Allan Chapman, who writes:

> Before its installation at Wanstead, Bradley carefully determined the sector's optical and mechanical characteristics. He realised that accurate measurements were only possible if his observing techniques equalled in rigour the workshop techniques of the instrument maker. After repeated trials on land objects, the object glass of the new sector was found to have a focal length of 12 ft 6.3 in. This meant that when the 12-ft tube was moved in an arc around its pivots, 1" at the eye-piece end would correspond to 1/1375 in. The motion of the telescope was governed by a fine micrometer, of which one revolution corresponded to 0.0246 in. At

the end of the micrometer rod was a circular head, so that to turn the micrometer 1/80$^{th}$ of a turn moved the telescope 1/3252 in.[3] When this linear quantity is applied to the moving of a 12½-ft-radius telescope, it was found to advance the same through an angle of ½".”[4]

Bradley was confident that he could rely on the zenith sector for angles as small as 0.5 arc-seconds because of his trust in the undoubted skills of Graham. Both Bradley and Graham belonged to a close knit group who were associated with Newton’s inner circle. To the end of their days both Graham and Bradley remained committed to the imperatives of Newtonian philosophy. Bradley’s strong Newtonian commitment could not have been more elegantly stated than in the opening paragraph of his account of his work on the nutation of the Earth’s axis.

> The great exactness with which instruments are now constructed hath enabled the astronomers of the present age to discover several changes in the positions of the heavenly bodies, which by reason of their smallness, had escaped the notice of their predecessors. And although the causes of such motions have always subsisted, yet philosophers had not so fully considered what the effects of these known causes would be, as to demonstrate *a priori* the phænomena they might produce; so that theory itself is here, as well as in many other cases, indebted to practice, for the discovery of some of its most elegant deductions. This points out to us the great advantage of cultivating this, as well as every other branch of natural knowledge, by a regular series of observations and experiments.”[5]

Bradley is here claiming that theory is much indebted to practice, indeed that observation or experiment may expose persistent phenomena that theory may have ignored, precisely because it was of such small moment that it was overlooked or ignored. The work that Bradley undertook on the nutation was of considerable duration, the time it took for the nodes of the lunar orbit to revolve through a complete cycle around the Earth, a period of time of over 18½ years. It was also of extraordinary precision, for the angles of deviation induced into the

terrestrial axis amounted to about one second of arc a year. It is somewhat ironic that his mentor Halley thought that the validity of the observations of such small changes in direction as 1 arc-second were without foundation. As we have seen Halley believed the measurement of angles of less than 4 or 5 arc-seconds was impossible to establish. Until he died in 1742 Halley never accepted the validity of Bradley's work on nutation,[6] even though Bradley had by then been observing the phenomenon in several stars over a period of almost 15 years.[7] This must have weighed heavily for Bradley for early in his account of the nutation he wrote,

> When I shall mention the small quantity of the deviation which the stars are subject to from the cause that I have been so long searching after, I am apprehensive that I may incur the censure of some persons, for having spent so much time in the pursuit of such a seeming trifle; but the candid lovers of science will, I hope, make due allowance for that natural ardour with which the mind is urged on towards the discovery of truths, in themselves perhaps of small moment, were it not that they tend to illustrate others of greater use.[8]

**The Identification of the Nutation.**

The observational programme begun on the 19th of August 1726 at Wanstead was predicated on the presumption that the observed behaviour of γ Draconis and 35 Camelopardalis would be repeated in the behaviour of other stars, and that the mutual variation in their motions would be related to some form of universal or uniform cause.[9] I call it a programme and not an experiment because the lines of investigation were structured but open ended. The instrument constructed by Graham to Bradley's specifications was designed specifically to undertake a reiterative investigation that would expose what Bradley expected to be a universal phenomenon. The programme was designed to determine the parameters of a phenomenon that Bradley had already intuited whilst he was still working with Molyneux and Graham at Kew. It led to the construction of the law of the new discovered motion prior to the conjecture that the phenomenon was the product of the relative motion of the Earth and of light.

The continuation of Bradley's observational programme beyond the publication of the paper on the new discovered motion was predicated on the belief that he possessed evidence of some other phenomenon. There was a common presumption amongst astronomers influenced by Newton that there might be a nutation of the Earth's axis, though many were of the opinion that it would remain incapable of being observed or measured.[10] Bradley, in contradiction to this opinion, believed that the instrument he had at his command and his own reiterative and precise techniques had already exposed the nutation in the residuals of his observations of the new discovered motion. Bradley accepted Newton's conjecture and firmly believed he could demonstrate the efficacy of Newtonian theory by carefully isolating the phenomenon and determining its parameters.

A generation earlier Flamsteed had also referred to such an effect.[11] In an account of his use of Abraham Sharp's 7 foot Mural Arc Flamsteed wrote,

> By means of these and other observations, made in the course of the next 12 months, the errors of the instrument were determined for some years to come. Indeed I once used to think that by comparing them to each other, I should be able to find the quantity of the nutation of the Earth's axis, which was propounded by Isaac Newton. But if such a thing were to be granted it would be so minute as to be scarcely perceptible, unless with instruments of far greater than 7 feet and more fixed and solid than mine: otherwise, I am afraid, it could hardly be achieved.[12]

Rigaud stated that in 1693 Rømer had conceived of a nutation as a possible variation in the declination of various stars which could not be accounted for by parallax or refraction,[13] and then added that it was one of the first causes which occurred to Molyneux and Bradley,[14] but a nutation of such dramatic proportions was hardly a motion that conformed to Newton's expectations. A nutation of the magnitude of that reported by Molyneux to Newton[15] if it had been confirmed would indeed have "destroyed completely the Newtonian system".[16] It was simply incompatible with Newtonian theory.

Bradley, aware that there may have been a small but regular variation in the residuals of his observation of the aberration of light, must therefore have begun to conceive that it would be so small as to be almost imperceptible, fully conforming to Newton's expectation of a small nutation. The Newtonian nutation as predicted in the *Principia* was, however, "a very small semi-annual nutation depending on the sun."[17] Given the immense precision of Bradley's work, the epithet "almost imperceptible" was sufficiently encouraging to suggest that he may have acquired evidence for a confirmation of Newton's elusive nutation. When Bradley determined to continue with his observations with the zenith sector at Wanstead, he could not have been aware that he was committing himself to such a sustained programme of observation as he finally undertook. He may have believed that the work isolating and identifying Newton's expected semi-annual nutation would extend his work for a year or two at most. By the time he had been working on the perceived nutation for about five years (from 1727 to 1732) Bradley was becoming aware that the nutation he was now isolating was less associated with the influence of the Sun than with the Moon, and that he was committed to observing a phenomenon through a complete cycle of the Moon's nodes probably up to 1746 or 1747.

When Bradley began the series of observations that would lead to the isolation of the phenomenon later identified as the aberration of light on the 19th of August 1727, the Moon's ascending node was close to the first point of Aries.[18] Given this, the orbit of the Moon was as steeply inclined to the equatorial plane as possible, with the result that the stars near the equinoctial colure changed their declination up to 2 arc-seconds more than would be expected if precession was 50".[19] Bradley's account reveals that the following year's observations exposed the same phenomenon.

> I have already taken notice in what manner this phænomenon discovered itself to me at the end of my first year's observations, viz. by a greater apparent change of declination in the stars near the equinoctial colure, than could arise from a precession of 50" in a year; the mean quantity now usually allowed by astronomers. But there appearing at the same time an

effect of a contrary nature in some stars near the solstitial colure, which seemed to alter their declination less than a precession of 50" required, I was thereby convinced that all the phænomena, in the different stars, could not be accounted for, merely by supposing that I had assumed a wrong quantity for the precession of the equinoctial points.[20]

Characteristically Bradley tested his equipment, with the suspicion that the plumb line was possibly in error, for the wire had broken three times over the course of the first year.[21] After adjusting a particular point of the arc to the plumb-line as accurately as he could, and taking off the old wire and hanging the plumb on another wire he compared his resulting observations with those formerly made and discovered after repeating the procedure three times that no sensible error arose from the use of different plumb-lines.[22] By the end of 1728 he had fully determined the parameters of the phenomenon later called the aberration of light, and after reflecting on the nature of the apparent motions he was observing he was more than suspicious that he had begun to isolate another regular motion contained within the motion he was presently observing.[23] Bradley continued his observations until May 1732 when he changed his chief abode from Wanstead to Oxford. In the account addressed to Macclesfield in 1748 he stated that he had begun to guess about the cause of the motions he had been observing for almost five years.[24]

> It appeared from my observations that, during this interval of time, some of the stars near the solstitial colure had changed their declinations 9" or 10" less than a precession of 50" would have produced; and at the same time, that others near the equinoctial colure had altered theirs about the same quantity more than a like precession would have occasioned; the north pole of the equator seeming to have approached the stars, which come to the meridian with the sun, about the vernal equinox and the winter solstice; and to have receded from those which come to the meridian with the sun about the autumnal equinox and the summer solstice.
>
> When I considered these circumstances, and the situation of the ascending node of the moon's orbit, at the time when I first began my

observations; I suspected that the moon's action on the equatorial parts of the earth might produce these effects:[25] for if the precession of the equinox be, according to sir Isaac Newton's principles, caused by the actions of the sun and moon upon those parts, the plane of the moon's orbit being at one time above ten degrees more inclined to the plane of the equator than at another, it was reasonable to conclude, that the whole part of the annual precession, which arises from her action, would in different years be varied in its quantity;[26]

During the period from 1728 until 1732 Bradley must have seriously considered Newton's discussion of the phenomenon of annual precession in the third book of the *Principia.*[27] Bradley was now convinced that the annual precession of the Earth's axis was being subjected to some inequality that modified the regular motion of this long observed phenomenon. The precession of the equinoxes was first recognised by the Greek astronomer Hipparchus during the second century before the common era, using data from many centuries of empirical observations, mainly from Babylonian sources. In Book 7 of the *Syntaxis Mathematica* (commonly called the *Almagest*) Ptolemy discussed the phenomenon with reference to the work of Hipparchus.[28] On the first page of his translation of the 7th book of the *Almagest,* G. J. Toomer placed a footnote,

> Note that the motion which in modern terminology is "precession of the equinoxes" (i.e. a motion in the direction of decreasing longitudes of the tropical points with respect to the fixed stars) is described by Ptolemy as a motion of the fixed stars with respect to the tropical points in the direction of increasing longitudes. This accords with his taking the tropical points as the primary reference points.[29] Hipparchus, however, seems at times to have adopted the modern convention, to judge from the title of his work *On the displacement of the solsticial*[30] *and equinoctial points.* (III, 1, p132, and VII, 2, pp. 327 and 329).[31]

The concept of there being a regular displacement of the solsticial and equinoctial points was therefore well known to practical astronomers for about two thousand years when Newton incorporated his discussion of the phenomenon in the

*Principia.*[32] At the close of the argument in the third edition, Book 3, Proposition 39, Problem 20, Newton stated that because of the inclination of the plane of the equator to the plane of the ecliptic the motion would be diminished by a ratio equal to the cosine of 23½° (91,706 to 100,000). This, he argued, is the annual precession of the equinoxes due to the force of the Sun. Newton also argued that the force of the Moon to move the sea relative to the force of the Sun was about 4.4815 to 1, and the force of the Moon to move the equinoxes was of exactly the same proportion. Newton calculated that the annual precession of the equinoxes arising from the action of the Moon would be 40 seconds, 52 thirds, 52 fourths, whilst the total annual precession of the combined forces of the Sun and the Moon would be equal to 50 seconds, 0 thirds and 12 fourths. Newton added that from astronomical observations the precession of the equinoxes is more or less 50 seconds per annum.[33] These calculations were all predicated on the earth being a sphere, but Newton clearly understood that the earth was an oblate spheroid. Earlier he calculated that this would affect the motion of objects, for in Book 3, Prop. 9, Theorem 9 he wrote,

> *In going inward from the surfaces of the planets, gravity decreases very nearly in the ratio of the distances from the center.* If the matter of the planets were of uniform density, this proportion would hold true exactly by book 1, prop. 73.[34] Therefore the error is as great as can arise from the nonuniformity of the density.[35]

So when Newton completed his discussion of the precession of the equinoxes he offered a suggestion that the motion may not be uniform due to the effects of the earth being an oblate spheroid and not a perfect sphere. He argued that if the "height of the earth at the equator" exceeded "its height at the poles" by 17.15 miles its matter would be rarer at the circumference than at the centre and the precession of the equinoxes would be increased because of the excess in "height" and diminished because of the lesser density.[36] So when the motion due to the precession of the equinoxes was revealing an apparent inequality, it might be justified to accept that Bradley must have seriously considered the possibility of determining the degree of oblateness of the figure of the earth by joining Graham

in an experiment with a seconds pendulum, comparing results in London with those in the tropics. This presumption is strongly supported in my opinion by the work that Bradley undertook during the 1720s and early 1730s with Graham on isochronal pendulums, long an interest of the great instrument maker.[37]

**The Work with Graham on Isochronal Pendulums.**

It was with particular reference to another proposition in the *Principia* that Bradley directed his immediate attention when examining the possible consequences of the earth being confirmed as an oblate sphere.[38] In the recent translation by Cohen and Whitman[39] Newton states, in the third edition, that the lengths of pendulums which oscillated with equal periods are the gravities, adding that at the latitude of Paris the length of a "seconds pendulum" (a pendulum that oscillates in precisely one second) is 3 Parisian feet and 8½ lines. At the equator the length of a pendulum will be shorter than the length of a pendulum with the same period (one second) to an amount equal to 1.087 lines.[40] Newton computed a table with the following data.

| Latitude in degrees. | Length of pendulum in ft. & lines. | | Length degree of Meridian in toises. |
|---|---|---|---|
| 0 | 3 | 7.468 | 56637 |
| 5 | 3 | 7.482 | 56642 |
| 10 | 3 | 7.526 | 56659 |
| 15 | 3 | 7.596 | 56687 |
| 20 | 3 | 7.692 | 56724 |
| 25 | 3 | 7.812 | 56769 |
| 30 | 3 | 7.948 | 56823 |
| 35 | 3 | 8.099 | 56882 |
| 40 | 3 | 8.261 | 56945 |
| 41 | 3 | 8.294 | 56958 |
| 42 | 3 | 8.327 | 56971 |
| 43 | 3 | 8.361 | 56984 |
| 44 | 3 | 8.394 | 56997 |
| 45 | 3 | 8.428 | 57010 |
| 46 | 3 | 8.461 | 57022 |
| 47 | 3 | 8.494 | 57035 |
| 48 | 3 | 8.528 | 57048 |
| 49 | 3 | 8.561 | 57061 |
| 50 | 3 | 8.594 | 57074 |
| 55 | 3 | 8.756 | 57137 |
| 60 | 3 | 8.907 | 57196 |
| 65 | 3 | 9.044 | 57250 |
| 70 | 3 | 9.162 | 57295 |
| 75 | 3 | 9.258 | 57332 |
| 80 | 3 | 9.329 | 57360 |
| 85 | 3 | 9.372 | 57377 |
| 90 | 3 | 9.387 | 57382[41] |

Bradley certainly knew that Newton's calculations were based on Jean Richer's observations of the 1670s in Cayenne.[42] It was entirely characteristic that Bradley would seek to undertake an experiment in partnership with Graham in order to produce data that was as rigorous, precise and accurate as contemporary instrumentation and practice could determine, rather than rely on earlier derived data no matter how reliable. Bradley began his paper on isochronal pendulums stating that it was a well established truth that pendulums of the same length did not perform their oscillations in equal times in different latitudes as predicted by Newton. Ostensibly, Bradley's purpose was to determine precisely and accurately the difference in the vibration of a pendulum in London and in Jamaica, in order to determine how accurate Newton's calculations were. This was to Bradley very much a means to an end, for if he could demonstrate through precise observations a confirmation of Newton's calculations, then the suggestion that the precession of the equinoxes might be affected by a factor connected with the oblateness of the Earth, would support his current work on what he was now sure was a nutation connected with the motion of the moon's nodes. This was a distinctive Newtonian trait that extremely accurate and precise measurements could help determine the extent of the nutation and the oblateness of the Earth. Bradley set out his objectives in his paper on the isochronal pendulums,

> and though several experiments made since in different parts of the earth concur to prove that pendulums swinging seconds are in general shorter as we approach the equator; yet what the real difference is between the lengths in different latitudes, does not seem to have been determined with sufficient exactness by the observations that have hitherto been communicated to the public; as may be gathered from the twentieth proposition of the third book of sir Isaac Newton's Principia, where they are compared as well with each other, as with the theory of that illustrious author.[43]

What impresses the reader of this paper is yet again the thoroughness of the preparations and the careful planning that went into setting the experiment up.

For Bradley this horological experiment might help to determine the veracity and validity of the work he was now undertaking on the inferred nutation. Bradley evidently believed that he needed this support, for there can be no doubt that he had discussed his work on the nutation with Halley and had been met with discouragement given his scepticism regarding the precise tolerances to which Bradley was then working to with the zenith sector.[44] In addition, although Bradley admired Newton's work he firmly believed that there was no possible substitute for accurate and precise data, no matter how accurate or valid the calculations made by his greatest mentor. It was a Newtonian characteristic, for Newton was reported by Conduitt to have asserted that "there is no arguing against facts and experiments."[45]

Although the construction of the clock was entirely due to Graham's immense skills, Bradley appears to have been the originator of the particular experiment in question, even though Graham had long had an interest in the problem. Graham's skills and achievements had included the invention of the dead-beat escapement, which was a major advance in the accuracy of time-pieces, though it must be accepted that Bradley's faith in Graham's consummate craftsmanship was based on several years of intimate experience of his immense abilities, giving him the confidence that the experiment would provide evidence that would support his work on the nutation, and counter the scepticism of his most notable living mentor. In his account Bradley wrote,

> Among these there is a clock whose pendulum vibrates seconds, made by our ingenious member Mr. George Graham,[46] justly esteemed for his great skill in mechanics; who judging that an opportunity was now offered of trying with the utmost exactness what is the true difference between the lengths of isochronal pendulums at London and Jamaica, readily embraced it; and in framing the parts of the clock, carefully contrived that its pendulum might at pleasure be reduced to the same length, whenever there should be occasion to remove the clock from one place, and set it up in another.[47]

The clock was carefully adapted, thus all the appurtenances associated with a normal clock such as striking parts and so forth were omitted. Everything was directed to the accurate timing of astronomical observations.[48] The clock oscillated seconds of sidereal and not solar time to increase its facility as an astronomical time-piece. Various tests were effected by George Graham whilst the clock was at his premises in London. Through observations made of transits of α *Aquilæ* Graham was able to determine that the clock gained 12 seconds in 10 apparent revolutions of the star.[49] The immense thoroughness of the preparations were, however, demonstrated in the way the pendulum was tested in two entirely different ways, both for the effects of temperature on the length of the pendulum and on the effects that various weights might have on the capacity to keep "true time". To determine how the pendulum might be affected by the greater expected heat of the Jamaican climate a thermometer was also fixed close to it, so whilst Graham observed and timed the passages of the test star through his meridian, he also read off the temperature from the height of the spirits in his thermometer. All of these tests and observations were effected between the 20th and the 30th of August, 1731.[50] After this Graham altered the clock weight from one of 12lb. 10½oz. to one of 6lb. 3oz. (the weight of the pendulum being 17lb.) and noted that the pendulum now vibrated only 1° 15' each way compared with its former 1° 45'.[51] There was, however, no noticeable difference in the accuracy of the timekeeping of the clock. Bradley concluded,

> This experiment shews, that a small difference in the arcs described by the pendulum, or a small alteration in the weight that keeps it in motion, will cause no great difference in the duration of the vibrations; and therefore a little alteration in the tenacity of the oil upon the pivots, or in the foulness of the clock, will not cause it to accelerate or retard its motion sensibly; from whence we may conclude, *that whatever difference there shall appear to be, between the going of the clock at London and in Jamaica, it must wholly proceed from the lengthening of the pendulum by heat, and the diminution of the force of gravity upon it.* (My italics) [52]

For a mind as perceptive as Bradley's, this must be accepted as an unusual aberration, for on reflection such an experimental set up must have been subject to many other sources of error than those mentioned.

After obtaining the results of the observations in Jamaica[53] and comparing the thermometer readings, Bradley and Graham calculated the effects that the lengthening of the pendulum might have on the slowing of its oscillations. From the careful records of the performance of the clock in London during from the 20th to the 30th August, 1731, it had been concluded that the clock was one second a day slower when the thermometer was two divisions higher. From the Jamaican records the thermometer varied from between 15 and 20 divisions higher than those recorded in London, and this led to the conclusion that on average the clock went some 8½ seconds slower each day on average due to the greater air temperatures in Jamaica than when it was situated in London.[54] The clock lost 2 minutes and 6½ seconds a day in Jamaica compared to its performance in London.[55] Thus taking away the effect due to the expansion of the pendulum Bradley deduced that the clock lost 1 minute and 58 seconds a day due to the diminution of gravity. Bradley's account continued,

> the increase of gravity, as we recede from the equator, is nearly as the square of the sine of the latitude; and that the difference in the length of pendulums is proportional to the augmentation or diminution of gravity. Upon these suppositions, I collect from the forementioned observations, that if the length of a simple pendulum (that swings seconds at London) be 39.126 English inches, the length of one at the equator would be 39.00, and at the poles 39.206.[56]

When the clock was sent to Colin Campbell in Jamaica great care was taken about the effects of bias. Bradley reported that Graham sent very full directions describing how the clock was to be set up and how the pendulum might be reduced exactly to the state as when it had been situated in England. Immense care was taken in not revealing how the clock had performed in London. Bradley and Graham wanted to avoid all possible bias or prejudice in favour of any hypothesis or any former observations that Campbell may have been aware of.[57]

After such due care and attention to every possible detail, Bradley finally came to a remarkable conclusion which must have exceeded every expectation, for he was aware of the capacity of even the most diligent of scientific investigators to be misled by favourable results. He wrote,

> the difference of the squares of the sines of 51½ ° and 18°, the latitudes of London and Black-River being to the square of the radius as 118 to 228¼, the clock will go 1' 58" in a day slower at Black-River than at London, *as was found by observation.* (My italics)[58]

A careful and diligent worker like Bradley must surely have believed that such a result, in which the result of the observations exactly equalled the prediction of theory, given the many different factors that could affect the final result was almost too much to be hoped for. Whilst Graham gave explicit instructions to Campbell on how to reduce the pendulum to the state it was in at London, there must have been many other factors which involved various forms of tacit knowledge and common practice that came into play. This suspicion is further increased because there is a significant variation between the results of this experiment with the predictions contained in the *Principia*,[59] even though the postulation that the Earth was an oblate spheroid was fully supported by the experiment. Bradley often expressed concern about the problems associated with observational uncertainty, which is one of the primary reasons why his mature work is always characterized by his practice of recording the sources of error. If this was true of his own diligent practice, then how much more it must have been so when he dealt with the observations of partners, no matter how well attested. Surely a result such as that which he derived from Campbell's observations must have led to Bradley entertaining some misgivings about a result that was so much in agreement with prediction, even if it appeared to support Newtonian theory, no matter how careful and accurate the set up was. He did, however, have great faith in the accuracy of Graham's handiwork,[60] and this may have alleviated some of the reservations he may have entertained.

Bradley's reservations were not expressed directly, but he hoped that the example of his experiment with Graham and Campbell, to whom he gives all due

respect as regards the success of the observations, (with undoubtedly more than a passing hope of future support and patronage),[61] might be repeated by others.[62] Like his greatest mentor, Bradley believed that experimental results required constant confirmation or possible refutation. His own reiterative methodology was such, that although he must have felt relieved and vindicated that the results of the pendulum experiment supported his continuing work on the nutation, he was also perceptive enough to recognise the effects of observational bias, particularly as the phenomenon he was observing was of such small proportions, particularly when he was so aware of Halley's continued scepticism.[63] Whilst concluding his account of the pendulum experiment, Bradley referred in passing to a controversy that was beginning to gain the attention of the scientific community.

**The Debate over the Figure of the Earth.**

The publication of Bradley's paper on isochronal pendulums in 1732 coincided with the beginnings of what was to become a lively debate over "the figure of the earth". Although Bradley's work with Graham and Campbell appeared to support Newton's theory, most French astronomers did not at first accept the result. Throughout the latter decades of the 17th century and into the early years of the 18th century various French astronomers had performed extensive geodesic surveys which appeared to conform to the Cartesian theory that the Earth was a prolate sphere. In 1728 Pierre Louis Moreau de Maupertuis visited London and like his compatriot Voltaire, who lived in London from 1726 to 1729, discovered he had moved into a different world. Voltaire famously summed up the contrast.

> A Frenchman arriving in London finds things very different, in natural science as in everything else. He has left a world full, he finds it empty. In Paris they see the universe as composed of vortices of subtle matter. In London they see nothing of the kind. For us it is the pressure of the moon that causes the tides of the sea; for the English it is the sea that gravitates to the moon,[64]

To many French astronomers and natural philosophers Newton's *Principia* was admired as a work of mathematical theory which, however well argued, failed to account for the "real" world. To Cartesian leaders of scientific opinion in France and within the Académie Royale des Sciences in particular, Newtonian theory represented a retrograde step from the rationalistic and materialistic physics of Descartes to a formerly discarded acceptance of occult influences and affinities. To French astronomers and natural philosophers inclined to Newtonian solutions it was Cartesian physics and its theory of vortices which was becoming more questionable. It was Voltaire who best expressed the stated position of the French Newtonian school. He wrote, "It is the vortices that might be called an occult quality, since nobody has proven their existence. Attraction on the contrary is a real thing, since its effects can be demonstrated and its proportions calculated."[65] In 1732 Maupertuis revealed his own Newtonian pretensions with the publication of his *Discours sur les differentes figures des astres.*[66] The ambitious academician developed a subtle mathematical and philosophical argument which was perceived by some Academicians as a critique of the work of the highly respected Jacques Cassini (Cassini II). Taking his lead from Newton's *Principia,* Maupertuis demonstrated that not only Newton's theory but that

> all *hypotheses* of central forces of attraction whose magnitudes vary like $r_n$, where $r$ is the distance from the centre of force situated at the centre of a figure of revolution, make *all* homogenous figures of revolution come to be flattened.[67]

One of the major reasons why Newtonian theory had not been generally accepted in France was because Cassini II, together with various colleagues, including his very able cousin Jacques Maraldi, had deduced from various measured variations of degrees of latitude in France, along the meridian through Paris, that the Earth was shaped, in accordance with Cartesian theory, like a prolate spheroid. Indeed, Cassini's observations suggested an elongation of the shape of the earth of about 7.5%, and as these results were widely regarded as the most accurate ever made, they were accepted as firm empirical evidence in support of Cartesian theory.[68]

The great panjandrum of French science, Bernard le Bouvier de Fontenelle published a hostile review of Maupertuis' *Discours* in which he asserted that *measurements* made by reliable geodesists such as Cassini II were much to be preferred to any theoretical demonstrations of geometry.[69] The Academy had already planned an expedition to Peru, or what is presently Equador, and in a *mémoire* written in 1735 Maupertuis argued that a second expedition was required to travel to Lapland, in present day Finland.[70] Various authors have argued that these expeditions took on the character of an *experimentum crucis,*[71] but the research and writings of Eric Aiton,[72] John Greenberg,[73] Mary Terrall,[74] and Rob Iliffe[75]suggest that the resolution of the "conflict"[76] between the Cartesian and Newtonian systems was not achieved by these experiments and observations. Rather the resolution came when Newton's inverse-square law was verified by solving analytical problems in the field of celestial mechanics. The expeditions were inconclusive. As Terrall states,

> The interchange of results and ideas focused a substantial group of academicians on interlocking mathematical, theoretical, and empirical problems. One paper or solution sparked another, whether a challenge or a clarification. This kind of scientific sociability was not always collaborative or even friendly, but it gave the subject a momentum that continued to grow with the departure of the first expedition.[77]

Greenberg goes further and asserts that the conflict between the Cartesian and Newtonian world views would never have been resolved by the geodesic expeditions, for both schools in the main supported a flattening of the poles, although the Cassinis adamantly refused to accept this view.[78] Much of the rivalry was undoubtedly engendered by the reception of the *Principia* in France, which was delayed by its impenetrability and the development of a different version of the calculus on the continent, which was exacerbated by the conflict that took place between the Leibniz and the supporters of Newton. The *Opticks* received a slightly more sympathetic treatment, but in France the work of Newton faced a continuing resistance well beyond any clearly discernible causes. In some circles there was a clearly maintained distinction between the Cartesian and Newtonian

natural philosophies in France and Great Britain, such that Voltaire was able to characterise them as a French physics and an English physics.[79]

Bradley appeared to express a degree of reticence in getting entangled in the debate when he stated that he would prefer not to get involved with it.[80] He must have regarded the dispute with a degree of detachment, and certainly with more than a measure of amusement. He recognised the intractability of the world and was highly critical of the observational methods practised by Maupertuis and his colleagues. Nevertheless, it was a subject which would involve him more than once. When the account of his experiment on isochronal pendulums came to the notice of French natural philosophers and astronomers, French "Newtonians" saw Bradley as a possible supporter.

Although it was sometimes contended that natural philosophy was divided into two warring camps called the "Cartesians" and the "Newtonians", with diametrically opposed views about the very nature of scientific research and how knowledge claims were recognised and applied,[81] the divisions were often more apparent than real. The differences that divided the Bernoullis were certainly maintained with a degree of sincerity that suggests that the *perceived* differences were very real so our opinions must remain both guarded and cautious. Whilst some scientific workers such as Cassini II abandoned entering into the debates that were engendered during the late 1730s,[82] and some thinkers such as Fontenelle, remained committed to what were increasingly regarded as outmoded patterns of thought,[83] the truth of the matter is that whether men regarded themselves as "Newtonians" or as "Cartesians", their practice converged.[84] What was so important in the debate was the commitment to what was regarded as "reasonable", and to analysis, which in the rapid development of Leibnizian calculus and mathematical analysis became increasingly quantitative.[85] The "Newton" of the *Principia* became the model of emulation, not so much because the search for "causes" was necessarily abandoned, but merely deferred.[86] There is a shift in emphasis and an abandonment of a search for grandiose world systems by some scientific workers during the first half of the 18th century. The adoption of a more open ended, less openly doctrinaire approach allowed the

acceptance of Newtonian methods even by workers who had long maintained the Cartesian search for causes. This was to become a pattern for the development of modern science. Bradley certainly reflected this trend for he remained sceptical about grand schemes and favoured a "hard won" empirical approach to the acquisition of natural knowledge.

The debate about the nature and reality of a "scientific revolution" has tended to concentrate on the important changes in perception and practice that took place during the 16th and the 17th centuries, and these should not be made light of, for whether this may be regarded as a revolution or not, the search for knowledge was irretrievably altered by the coming together of various hitherto separate traditions, opening up different possibilities, in which the "learned" scholar and the "unlearned" craftsman found mutual benefit in common ends.[87] The changes which took place in scientific practice during the 18th century, however, were far-reaching in their consequences. Although Bradley belonged to a circle of men who inhabited Newton's immediate entourage, he also helped to create an intellectual *construct* that was identified as "Newtonian".[88] For much of his life Newton was a restless philosopher of nature whose curiosity encompassed patterns of experience and knowledge which we now variously describe as natural philosophy, theology and alchemy. To be sure, Newton made distinctions between them, but they were not mutually exclusive in his own mind,[89] and many studies now reveal how these other "Newtons" helped to form the figure that became the apotheosis of scientific endeavour during the 18th century.

The "Newton" of the 18th century was of import because increasingly his work was identified as the foundation of a new way of conducting research. If Newton laid some of the foundations of this new science, then it too created the "Newton" that until the 20th century was regarded almost without question as the very epitome of scientific rectitude. But this Newton was the creation of the Enlightenment.[90] Indeed in some quarters almost any knowledge claim that was accepted or regarded as being "reasonable"[91] was subsumed under the epithet of "Newtonian". When discussing the "conflict" between the Newtonians and the Cartesians during the 1730s and 1740s, what must not be lost sight of is that the

meaning of these terms became increasingly malleable. Cartesian physics was never entirely abandoned.[92] As well as the work of John Bernoulli, that of Leonhard Euler, who maintained that the goal of natural philosophy was to provide causal explanations in terms of micro-structures, various workers maintained a strong propensity in support of Cartesian procedures. Some of these elements remained and were subsumed under the heading of Newtonianism.

The work of Bradley is of interest to the modern historian because his work characterised the quest that was identified by his scientific contemporaries as Newtonian. Indeed his work so exemplified the "Newtonian" model that it may be accurate to assert that he, along with others dedicated to precision and accuracy, became the "creators" of the Newton of the Enlightenment, the *construct* that was so loathed by later radical romantics as the quintessence of the mechanistic world view they rejected. There is in Bradley's work an attention to detail and a perceptive grasp of the absolute essentials of a problem. This, when combined with his characteristic, and undoubtedly self-fashioning modesty, was to become increasingly identified with the highest ideals of natural philosophy. It is a commonly expressed cliché that "the devil lies in the detail", and Bradley was amongst those who diligently modified the world by a conscientious pursuit of the detail that some considered trivial. Bradley's precision was a powerful tool of practical and theoretical analysis, and whilst he modestly conducted much of his most important research in the shadows he nevertheless remained steadfast in the pursuit of his aims. One of his critics in his pursuit of such apparently trivial detail, Bradley's own mentor Halley, must have been very much in mind when he remarked, "I am apprehensive that I may incur the censure of some persons, for having spent so much time in the pursuit of such a seeming trifle."[93]

In the pursuit of such "trifles" Bradley established a nutation of the Earth's axis associated with the motion of the moon's nodes, and to his contemporaries confirmed an important aspect of Newton's own theoretical analysis at the very time when "Cartesian" arguments were being vanquished by the auspices of a mathematical approach that was identified as "Newtonian". The eventual

publication of Bradley's paper on the nutation in 1748 came at the very moment when the recognition of its import was at its greatest.

**Bradley's Continuing Work on the Nutation.**

After observing the phenomenon which Bradley was now sure was a nutation for about half the revolution period of the moon's nodes, from 1727 to 1736, he must have been confident of observing the same phenomenon in reverse until the cycle had been completed, provided he lived to observe it.[94] Rigaud rightly stated that Bradley's endemic caution would have precluded him from announcing his discovery of the nutation until he had observed the entire cycle.[95] However, Bradley was contacted by Maupertuis on his return from the expedition to Lapland.[96] He had earlier approached Bradley requesting that Graham construct a zenith sector for use on the expedition,[97] so Bradley translated Maupertuis' letter into English for Graham in London.[98] At the end of 1737 Bradley told Graham that,

> Mr. Maupertuis having done me the honour to communicate to the Royal Society, through my hands, the observations which the French gentlemen made in the north, as I apprehend his account can be to none more agreeable than to you, who had so much trouble in the affair, and to whom we are principally indebted for the accuracy of the experiment. I shall therefore take the liberty of sending you the contents of his letter, as nearly as my small skill in French enables me to guess at it, imagining it may afford you some satisfaction, at least, till you can hear a more exact translation of it read before the Society.[99]

However, Bradley was extremely critical of Maupertuis' methods. This critique did not any way express overt antipathy, but implied how an experienced and skilful observer might have reversed the instrument to ensure the accuracy of the observations. Bradley believed that Maupertuis hadn't fully utilised the accuracy of the instrument designed by his friend Graham. Bradley expressed his view to Graham, that there might be doubts about the veracity of Maupertuis' results, and that the instrument should be utilised on the Paris meridian which had already

been accurately surveyed, in order to determine the accuracy of the method used in Lapland.[100] He asked to Graham whether he should tell Maupertuis about the nutation. He wrote,

> As I must soon send Mr. Maupertuis an answer to his request about the quantity of aberration on account of light in those stars which they observed, (which indeed they have already rightly computed,) I should be glad to hear from you before I write to him, and have your opinion as to such points as you may think proper to be suggested to them relating to the future use of their sector, and whether I should not at the same time intimate to him the *other irregularities* which I have observed in the situation of the earth's axis to the plane of the ecliptic, and of the unequal precession of the equinoctial points.[101]

The letter reveals the respect that Bradley had for Graham, and it also shows that Graham was fully apprised of the work that Bradley was undertaking on the nutation of the earth's axis. The advice that Bradley sought from Graham on the advisability of revealing the work on the nutation to Maupertuis, whilst motivated by his wish to increase the accuracy of the work of the Lapland expedition, may also have been prompted by his wish that he not get too closely entangled with a debate that appeared to have generated so much emotion. In his reply to Maupertuis he wrote, "I beg leave to do myself the honour to tell you what I have farther discovered, relating to some other changes that happen in the apparent situation of the fixed stars."[102] After giving an account of some of his observations from 1727 to 1737, Bradley continued to inform Maupertuis of his observations of a phenomenon arising from the unequal action of the Moon on the equatorial regions of the Earth, due to the inclination of the lunar orbit to the plane of the terrestrial equator. Bradley argued that this would induce a small nutation in the Earth's axis, together with an associated acceleration and retardation in the precession of the equinoxes amounting to about 9 arc-seconds from the mean.[103] This was the first time that Bradley's work on the nutation had ever been made public beyond his own circle of immediate associates which also included John Machin, the secretary of the Royal Society. In response Maupertuis

acknowledged the information, together with the revised reduction of the Lapland result.[104] It has to be accepted that Maupertuis gratefully accepted Bradley's amendments because in his disputes with colleagues in the Academy it served to advance his claims. Whether or not Bradley wanted to be associated with the debate over the figure of the earth, knowledge of his work on the nutation irrevocably associated him with some of the leading figures. Over the following years Bradley was to be approached by various figures in French astronomy and natural philosophy.

Eventually Bradley's work on the nutation was utilised by several astronomers in France. In a memoir on the on the altitude of the pole at Paris the nutation was alluded to by Pierre Charles Le Monnier.[105] There is no doubt that various French astronomers, including Lacaille and De l'Isle, held very high opinions of Bradley's work, for both warned him about the activities of Le Monnier, whom they stated often claimed more than he was entitled to, and who was attempting to pass off some of the work on the nutation as his own.[106] At some time during 1738 Le Monnier took possession of Graham's sector from Maupertuis. He began to undertake a series of observations of the nutation which largely conformed to the pattern already determined by Bradley. In 1745 Le Monnier forwarded some of his results to Bradley at Greenwich.[107] As Rigaud stated, this might be have been regarded as a commendable action, but the paper that Le Monnier wrote which was published with the results he sent to Bradley suggests otherwise, for the French astronomer gave an account of the nutation and of his part in the resolution of the problem.[108] Rigaud expressed his concern with Le Monnier's overt opportunism.

> This was not handsome, after the freedom of communication which he had enjoyed, and Bradley, when he had made such a fine discovery, and had pursued the inquiry for so many years, ought to have been allowed in common courtesy (to say nothing of justice) the privilege of being the first to publish the details of it to the world.[109]

It appears, however, that Bradley did not pay any attention to this.[110] It was only after he had confirmed his own hypothesis that he wrote the letter to the

2nd Earl of Macclesfield which was read at the Royal Society on the 7th and 14th January, 1748.[111] It was awaited with equal anticipation in France.

**The Paper on the Nutation.**

Bradley made hundreds of observations of many bright near zenith stars and gave several tables of the observations of six chosen stars.[112] Bradley was very like his most eminent mentor in that he would never accept the veracity of a phenomenon unless he possessed a sufficient explanation or cause.[113] Thus when he established the phenomenon later called the aberration of light he did so initially by applying it to particular instances. Likewise, Bradley revealed the motion due to the nutation by reference to these six chosen stars. None of these tables are included in the two still extant earlier versions of the paper.[114] He calculated the precession, aberration and the nutation of these stars and then determined how close his observations fitted these calculations. As such, although there were some apparent variations which fitted within the bounds of observational uncertainty, his observation books[115] show that there were some stars which were seriously out of step with expectations, including *Capella*,[116] which showed results far from the expected mean. This star was at the extreme angular range of the sector, and so the accuracy of his observations might have been prejudiced. It was by observing how the phenomenon affected the motions of the stars that promised better results because they passed closer to the zenith that Bradley felt confident about the veracity of the nutation. Nevertheless in presenting his results he judiciously selected those stars which best represented his claim to their validity and veracity. After working on the phenomenon for two decades, Bradley evidently believed that the results supported his initial conjecture that the Earth was subject to a minute nutation associated with the precession of the nodes of the lunar orbit. Yet following the experience of his paper on the aberration Bradley was acutely aware of the importance of presenting evidences that reinforced his argument.

Many of Bradley's later observations were not undertaken in the best possible conditions. After Elizabeth Pound gave up her house in Wanstead when

she moved to Oxford with Bradley, the house was let to a tenant, Mrs Elizabeth Williams. Bradley made arrangements for the sector to remain in its original situation so he could visit the house on the occasions he was able to travel to Wanstead, apparently staying with Matthew Wymondesolde and his family who owned a fine house and an extensive estate close by. Bradley agreed that he would never make observations at times that were inconvenient to Mrs Williams. This meant that he rarely observed after midnight. This was another factor which may have led to some degree of inaccuracy in some of Bradley's later observations,[117] which were also undertaken when his work at Oxford, where he was still presenting three lecture courses a year, and at Greenwich where he was dealing with inherited instrumental problems at the Royal Observatory, demanded time and intensity of effort. He did, however, possess sufficiently good observations of several stars to enable him to demonstrate the phenomenon of the nutation. From the many thousands of observations that Bradley made of dozens of stars he made up tables which analysed the motions of these stars due to nutation, aberration and precession. From these he produced tables which he introduced into the published version of his account. From the tables that Bradley produced of six stars, I have produced a brief summary of the results with the stars ordered in right ascension in which the basic pattern may be discerned.

| **Star** | **RA** | **Dec** | **Pattern of Nutation.** | |
|---|---|---|---|---|
| α Cassiopeiæ | **00h 40m 40s** | 56° 33' 23" | 8 Jan 1732 +8.9" | 2 June 1740 -8.9" |
| τ Persei | **02h 54m 28s** | 52° 46' 35" | 2 Dec 1729 +9.0" | 23 Dec 1738 -9.0" |
| α Persei | **03h 24m 32s** | 49° 52' 23" | 3 Dec 1729 +8.9" | 24 Dec 1738 -9.0" |
| 35 Camelopardalis | **06h 04m 35s** | 51° 34' | 20 Oct 1727 +8.9" | |
| η Ursæ Majoris | **13h 47m 37s** | 49° 17' 50" | 19 Jul 1730 -9.0" | 3 June 1740 +8.9" |
| γ Draconis | **17h 56m 38s** | 51° 29' 22" | 3 Sept 1727 -8.9" | 9 Sept 1736 +8.9" |

What is immediately obvious is that the stars situated near the solstitial colures, such as γ Draconis and 35 Camelopardalis reached the greatest deviation from the mean in 1727 and 1736, though being in opposition to each other one was north and one south of the mean. Stars near the equinoctial colure such as α Cassiopeiæ reached greatest deviation from the mean in 1732 and 1740.

It is when all of the observations of all the stars he observed are examined and analysed that the patterns become convincing, and my own brief summary can only point in the direction that Bradley's results indicated, which revealed a

motion that was evidently connected with the motion of the moon's nodes. The patterns imposed by the nutation on the annual precession of the equinoctial points are easier to discern than a few isolated observations made of a handful of stars. The mean precession is near 50" per annum, but the effect of nutation can lead to a variation from about 42.7" to about 58.0". Bradley produced a table in his account of the nutation.[118] I have reproduced this table.

**The Annual Precession of the Equinoctial Points.**[119]

| **Distance from 1st point of Aries .** | **Sign 0**[120] | **Sign I** | **Sign II** | **Sign III** | **Sign IV** | **Sign V** | |
|---|---|---|---|---|---|---|---|
| 0° | 58.0" | 57.0" | 54.2" | 50.3" | 46.5" | 43.7" | 30° |
| 5° | 57.9" | 56.6" | 53.6" | 49.7" | 46.0" | 43.4" | 25° |
| 10° | 57.9" | 56.2" | 53.0" | 49.0" | 45.5" | 43.2" | 20° |
| 15° | 57.7" | 55.7" | 52.3" | 48.4" | 45.0" | 43.0" | 15° |
| 20° | 57.5" | 55.2" | 51.7" | 47.7" | 44.5" | 42.8" | 10° |
| 25° | 57.3" | 54.7" | 51.0" | 47.1" | 44.1" | 42.8" | 5° |
| 30° | 57.0" | 54.2" | 50.3" | 46.5" | 43.7" | 42.7" | 0° |
| | **Sign XI** | **Sign X** | **Sign IX** | **SignVIII** | **Sign VII** | **Sign VI** | |

Thus a star whose right ascension is close to the first point of Aries, such as α Cassiopeiæ is at its greatest elongation from mean precession when the phenomenon of nutation is also at it's greatest, whilst coincidentally a star which is at quadrature to the first point of Aries, such as 35 Camelopardalis, is at its least elongation from mean precession when the nutation is also at its minimum. The variation of annual precession from the mean is thus correlated to the variation in terrestrial nutation. Although mean annual precession is close to 50 arc seconds the nutation imposes a variation so that over a period of just over 18.65 years any object may appear to precess by any arc between 42.7 and 58 arc seconds.

Machin suggested to Bradley that tables might be calculated by presuming the moon's attraction caused the terrestrial pole to describe a small circle around its mean place, arguing that the determination of the magnitude of this circle was one of the major objects of the inquiry.[121] The diameter of the circle was initially determined to be about 18", but Bradley believed there would be a closer agreement with his observations if the curve was presumed to be an ellipse with its major and minor axes to be about 18" and 16".[122] Nevil Maskelyne wrote that he had determined his own tables, "from a scrupulous calculation of all of Dr.

Bradley's observations of γ Draconis, which he was pleased to lend me,"[123] and from this it followed,

> that the nutation of the earth's axis is performed in an ellipse, whose greatest axis lying in the solstitial colure is 19.1", and lesser lying in the equinoctial colure 14.2", namely in proportion to one another as the cosine of the obliquity of the ecliptic to the cosine of double the obliquity, as given by theory.[124]

The theory of nutation had not been completely developed when Bradley wrote his paper on the work he had undertaken from the 19th August 1727 to the 3rd September 1747.[125] Although Newton had demonstrated the principles on which the theory of nutation depended, and Machin had been employed on the theory of gravity,[126] Bradley did not concern himself with the theory, even though he possessed the requisite skills required to undertake the task. Following his work on the observation of the nutation, Bradley's teaching commitments at Oxford,[127] and his work in London with the Royal Society and the Board of Longitude,[128] together with the heavy workload involved in the planning of new instruments and the construction of a new transit house at the Royal Observatory at Greenwich[129] and his ongoing observational work meant he had little enough time for the purpose. There were also observations of some stars which he was sceptical of, including those of *Capella*,[130] which showed results far from the expected mean. This star was at the extreme range of the sector and Bradley was sure that such results did not achieve the reliability he required to support his claims for such a minute phenomenon as the nutation.

**The Award of the Copley Medal.**

The paper on the nutation was read at the Royal Society on the 7th and the 14th of January 1748 (OS). The vote of thanks was worded in an unusually informal manner to the 2nd Earl of Macclesfield who had read the paper to the gathered assembly, for Bradley was in Oxford. They

> were likewise ordered to Dr. Bradley,[131] for his great pains and unwearied diligence in carrying on, through a course of so many years, so noble a

> series of the most accurate observations; which, joined to his great knowledge and sagacity, have enabled him to make such discoveries as will be a lasting honour to himself, to his country, and the present age.[132]

Under the direction of the president of the society Martin Foulkes, who had also long been Bradley's friend and patron,[133] the Royal Society determined to offer him, the Copley Medal. Bradley had become acquainted with Foulkes through the offices of his uncle's circle in Wanstead. Foulkes became an important intermediary when he represented Bradley's claims for the Savilian professorship in 1721, writing in support to his uncle William Wake, the Archbishop of Canterbury, who was one of the principle electors. The Copley Medal remains the highest scientific award of the society, having been instituted in accordance with the provisions of the will of Sir Godfrey Copley[134] and offered for the first time in 1731, when the recipient was Stephen Gray. When the award was made by the society, it was received on his behalf by Lord Macclesfield because of Bradley's continuing teaching commitments at Oxford.[135]

Bradley's work on the nutation had become widely known in the scientific community from 1737 onwards, following the letter he addressed to Maupertuis.[136] His work continued against the background of the development of Newtonian theory in France. Arguably the final identifiably Cartesian treatise published in France was Molière's *Leçons de physique*[137] which was an attempt to effect a rapprochement between Cartesian and Newtonian physics.[138] The decline in the vitality of the Cartesian tradition was indicated by the division of the prize awarded by the Academie Royale des Sciences in 1740. Of the four competitors for the prize for an essay on the tides, three were identifiably Newtonian.[139] Of these, that by Daniel Bernoulli was based on

> the attraction of mutual gravitation of the heavenly bodies and the earth; this incomprehensible and incontestable principle that the great Newton has so well established and that we can no longer hold in doubt.[140]

When Bradley was awarded the Copley Medal, it was given in the light of the knowledge that his work confirmed Newtonian theory in a way that was not obvious when he began his observations in 1727, but which became obvious only

as the theory was applied in the light of his observations. Although Bradley's observations confirmed Newtonian theory as expressed in the *Principia,*[141] it did so in an entirely unexpected way. It should have been obvious that the motion of the moon's nodes, with an associated variation in the obliquity of the angle between the plane of the terrestrial equator and the place of the lunar orbit, when combined with the oblateness of the figure of the earth, would lead to an effect on the mean precession of the equinoctial points. Bradley's work demonstrated how unexpected but revealing consequences may follow from theory, and which may only be revealed through experiment or observation. Newton had predicted the existence of a solar nutation but had not calculated its parameters. Prior to Bradley's work on this unlooked for lunar induced nutation it was generally believed any such motion would be too insignificant to observe. Bradley's paper indicated its parameters and encouraged work on the theory of nutation.

In July 1748 Bradley became a foreign associate of the Académie Royale des Sciences at Paris.[142] He acknowledged the honour when he wrote to the Comte de Maurepas on the 24th August, 1748.[143] Other honours were to follow. In 1750 he became a corresponding member of the Imperial Academy of Sciences at St. Petersburg.[144] In 1757 he was elected a member of the Institute of Bologna.[145] There is no doubt that the work on aberration and nutation had given Bradley a European reputation. His work on these two phenomena represented a major advance, not merely in the science of positional astronomy, but in a minor way with a firmer acceptance of Newtonian theory in France and much of the rest of Europe. Certainly in terms of the rigour of his scientific practice his work was to impact on the development of astronomy and astrometry throughout much of Europe.

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[1] This work was continued until 1747 whilst he was also teaching at Oxford, and observing at Oxford, Shirburne Castle and Greenwich.
[2] Bodleian Library, Bradley MS 20* *Drafts of papers by James Bradley on Aberration,* fol. 18v.
[3] Bradley, "Memoranda respecting the instrument at Wansted." in the "Miscellaneous Works", pp. 194 – 195.
[4] Chapman, *Dividing the Circle: The Development of Critical Angular Measurement in Astronomy 1500 – 1800.* 2nd ed. Chichester, 1995.
[5] Bodleian Library, Bradley MS 40, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works", p. 17.

[6] Rigaud, "Memoirs", p.xxix.
[7] There is no documentary evidence to suggest that Halley and Bradley had nothing but the highest regard for each other. Bradley witnessed Halley's last will and testament, and Halley earnestly sought to resign from his office as Astronomer Royal in favour of his protégé, but had to accept that his office was for life. Bradley worked with and for Halley right up until his mentor entered his final decline. It must have grieved Bradley that the one person whose approval he sought above all others considered his work on the nutation to be little more than worthless.
[8] Bodleian Library, Bradley MS 40, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "*Miscellaneous Works"*, p. 18.
[9] Bodleian Library, Bradley MS 20*. "The Third Unpublished Version of the paper on the New Discovered Motion", fol. 20. Bradley wrote, "The great regularity of the observations left no room to doubt, but that there was some regular cause that produced this unexpected motion, which did not depend on the uncertainty and variety of the seasons of the year."
[10] Newton, "Philosophiæ Naturalis Principia Mathematica", Bk. III, Prop. 21, Theorem, 17. 3rd ed. London, 1726. Translated by I. Bernard Cohen and Anne Whitman, London, 1999. Newton wrote, "The equinoctial points regress, and the world's axis, by a nutation in every annual revolution, inclines twice toward the ecliptic and twice returns to its former position. This is clear by book I, prop 66, corol. 20. *This motion of nutation, however, must be very small – either scarcely or not at all perceptible.*" (My italics)
[11] John Flamsteed, *The Preface to John Flamsteed's Historia Coelestis Britannica,* edited and introduced by Allan Chapman, based on a translation by Alison Dione Johnson, Maritime Monographs and Reports, No. 52, London, 1982.
[12] Ibid. p. 126. See also Wallis, "Opera", Vol. III. p. 706.
[13] Rigaud, "Memoirs", p. lxiv.
[14] Bradley, "A Letter to Dr. Edmond Halley, Astronomer Royal and etc. giving an account of a new-discovered Motion of the Fixed Stars" in the "Miscellaneous Works", p. 3.
[15] Kings College Library, Cambridge, Conduitt, "Memorabilia", Keynes MS. 130.5, fol. 3.
[16] Molyneux to Conduitt, as reported by Conduitt in his "Memorabilia", Keynes MS. 130.5 fol.3.
[17] Rigaud, "Memoirs", p. lxiv.
[18] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works", p. 23.
[19] Ibid. p. 23.
[20] Ibid. pp. 21 – 22.
[21] Bodleian Library, Bradley MS 40, *Papers related to Nutation. Observations, calculations and drafts of the letter to Lord Macclesfield.* fol. 23v.
[22] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "*Miscellaneous Works"*, p. 22.
[23] Ibid. p. 22.
[24] Ibid. p. 23.
[25] Bradley early suspected a lunar induced nutation after observing the phenomenon for two or three years. This was not in any way the same as Newton's small solar induced nutation. Bradley's phenomenon, though in conformity with Newtonian theory was not in any way predicted by Newton except as a possibility in connection with his final qualitative remarks in Book 3, Prop 39, Prob. 20.
[26] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works", p. 23.
[27] Newton, "Philosophiæ Naturalis Principia Mathematica", Book 3, Prop. 39, Prob. 20. 3rd ed., London, 1726. Translated by I. Bernard Cohen and Anne Whitman, *The Principia: Mathematical Principles of Natural Philosophy,* University of California Press, London, 1999. pp. 886 – 887.
[28] Claudius Ptolemaeus, *Syntaxis Mathematica,* Book VII. Translated by G. J. Toomer, and published as *Ptolemy's Almagest,* London, 1984.
[29] Ibid. p132.
[30] This is Toomer's rendition. Many modern lexicons also use "solstitial".
[31] Ibid. p. 321, n. 2.

[32] Newton, "Philosophiæ Naturalis Principia Mathematica", Book 3, Prop. 39, Prob. 20. Translated by I. Bernard Cohen and Anne Whitman, "The Principia: Mathematical Principles of Natural Philosophy", London, 1999. pp. 886 – 887.
[33] Ibid. pp. 886 – 887.
[34] Newton, "Philosophiæ Naturalis Principia Mathematica", Book I, Section 12, Prop. 73, Theorem 33. Translated by I. Bernard Cohen and Anne Whitman, *The Principia: Mathematical Principles of Natural Philosophy,* University of California Press, London, 1999, p. 593, reads, "If toward each of the separate points of any given sphere there tend equal centripetal forces decreasing in the squared ratio of the distances from those points, I say that a corpuscle placed inside the sphere is attracted by a force proportional to the distance of the corpuscle from the center of the sphere."
[35] Newton, "Philosophiæ Naturalis Principia Mathematica", Book 3, Prop. 9 Theorem 9. Translated by I. Bernard Cohen and Anne Whitman, "The Principia: Mathematical Principles of Natural Philosophy", London, 1999, p. 815.
[36] Newton, Philosophiæ Naturalis Principia Mathematica, Book 3, Prop. 39, Prob. 20. Translated by I. Bernard Cohen and Anne Whitman, The Principia: Mathematical Principles of Natural Philosophy, London, 1999. pp. 887 – 888.
[37] Bradley, "An Account of some Observations made in London by Mr. George Graham, F.R.S.; and at Black-River in Jamaica, by Colin Campbell, Esq. F.R.S. concerning the Going of a Clock, in order to determine the Difference between the Lengths of Isochronal Pendulums in those places", in the "Miscellaneous Works", Oxford, 1832, pp. 62 – 69.
[38] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed., London, 1726, Book 3, Prop. 20, Prob. 4. "To find and compare with one another the weights of bodies in different regions of our earth". Translation by I. Bernard Cohen and Anne Whitman, London, 1999, p. 826.
[39] Ibid. p.828.
[40] Ibid. p.828.
[41] Ibid. p.828.
[42] Newton, "Philosophiæ Naturalis Principia Mathematica", London, 1687, 1st ed., Book 1, Prop. 39, Prob.19, "Invenire Præcessionem Æquinoctiorum", pp.472 – 473.
[43] Bradley, "An Account of some Observations made in London, by Mr. George Graham, F.R.S.; and at Black-River in Jamaica, by Colin Campbell, Esq. F.R.S. concerning the Going of a Clock; in order to determine the Difference between the Lengths of Isochronal Pendulums in those places", contained in the "Miscellaneous Works", Oxford, 1832, p.62.
[44] Rigaud, "Memoirs", p. xxix.
[45] King's College Library, Cambridge, Conduitt, "Memorabilia", Keynes MS 130.5, fol. 3r.
[46] George Graham was an FRS.
[47] Bradley, "An Account of some Observations made in London, by Mr. George Graham, F.R.S.; and at Black-River in Jamaica, by Colin Campbell, Esq. F.R.S. concerning the Going of a Clock; in order to determine the Difference between the Lengths of Isochronal Pendulums in those places", contained in the "Miscellaneous Works" pp. 62 – 63.
[48] Ibid. p. 63.
[49] Ibid. p. 63.
[50] Ibid. p. 63.
[51] Ibid. p. 64.
[52] Ibid. p. 64.
[53] Campbell observed the passage of α Canis Majoris (Sirius) and β Canis Majoris.
[54] Ibid. p. 67.
[55] Ibid. p. 67.
[56] Ibid. p. 68.
[57] Ibid. p. 64.
[58] Ibid. p. 68.
[59] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed., London, 1726, Book 3, Prop. 20, Prob. 4. "To find and compare with one another the weights of bodies in different regions of our earth." Translation by I. Bernard Cohen and Anne Whitman, London, 1999, p. 826.
[60] Bodleian Library, Bradley MS 20* *Drafts of papers by James Bradley on Aberration,* fol. 18v.

[61] Colin Campbell was a cousin to the incumbent Earl of Argyle.
[62] Bradley, "An Account of some Observations made in London, by Mr. George Graham, F.R.S.; and at Black-River in Jamaica, by Colin Campbell, Esq. F.R.S. concerning the Going of a Clock; in order to determine the Difference between the Lengths of Isochronal Pendulums in those places", contained in the "Miscellaneous Works", pp. 68 – 69.
[63] Rigaud, "Memoirs", p.xxix.
[64] Voltaire, *Lettres Philosophique,* Paris, 1730. Translated by Leonard Tancock as *Letters on England,* Penguin Books, London, 1980. p. 68.
[65] Ibid. p.81.
[66] P. L. M de Maupertuis, *Discours sur les differentes des astres: d'oú l'on tire des conjectures sur les étoiles qui paraissent changer de grandeur et sur l'anneau de Saturne avec une exposition abbrégée des Systémes de Monsieur Descartes et de Monsieur Newton,* Paris, 1732.
[67] John L. Greenberg, *The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science,* Cambridge, 1995, p. 80.
[68] Eric Aiton, *The Cartesian Theories of the Planetary Motions,* Ph.D. Thesis, University of London, 1958.
[69] Bernard le Bouvier de Fontenelle, Review of Maupertuis's "Discours sur les differentes des astre", contained in *Histoire de l'Académie Royale des Sciences, 1732.* Paris, 1735.
[70] Greenberg, "The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science". p. 81.
[71] Amongst the various authors who have favoured the interpretation that the Peruvian and Lapland expeditions resolved the conflict between the Newtonians and Cartesians are Rene Taton, "Sur la diffusion des théories Newtoniennes en France. Clairaut et le problème de la figure de la terre." *Vistas in Astronomy,* Vol. 22, pp. 485 – 509. 1978, Valentin Boss, *Newton and Russia. The Early Influence, 1698 – 1796.* London, 1972, and David Beeson, *Maupertuis: An Intellectual Biography,* Oxford, 1992.
[72] Eric Aiton, *The Vortex Theory of Planetary Motions,* Macdonald, London, 1972. Eric Aiton very kindly lent me a copy of his doctoral thesis, *The Cartesian Theories of the Planetary Motions,* University of London, 1958, which he allowed me to copy and which I still find a useful source. Aiton favoured the view that many aspects of Cartesian theory were eventually subsumed into Newtonian theory.
[73] Greenberg, "The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science", Cambridge, 1995.
[74] Mary Terrall, *The Man Who Flattened the Earth: Maupertuis and the Sciences in the Enlightenment,* London, 2002.
[75] Rob Iliffe, "'Aplatisseur du Monde et de Cassini': Maupertuis, Precision Measurement, and the Shape of the Earth in the 1730s" in *History of Science,* Vol. 31 (1993), pp. 335 – 375.
[76] It was often conceived as a conflict, but although there were distinctive methodological approaches which divided the two traditions, there was sufficient overlap between them to allow discourse between the natural philosophers who favoured one above the other. The Lapland and the Peruvian Expeditions were largely inconclusive because the results were open to many challenges that created further debate.
[77] Terrall, "The Man Who Flattened the Earth: Maupertuis and the Sciences in the Enlightenment", p. 94.
[78] Greenberg, "The Problem of the Earth's Shape from Newton to Clairaut: The Rise of Mathematical Science in Eighteenth-Century Paris and the Fall of "Normal" Science", pp. 83 – 88.
[79] Voltaire, "Lettres Philosophique". Paris, 1733.
[80] Bradley, "An Account of some Observations made in London, by Mr. George Graham, F.R.S.; and at Black-River in Jamaica, by Colin Campbell, Esq. F.R.S. concerning the Going of a Clock; in order to determine the Difference between the Lengths of Isochronal Pendulums in those places", contained in the "Miscellaneous Works", p.67.

[81] Taton, "Sur la diffusion des théories Newtoniennes en France. Clairaut et le problème de la figure de la terre." *Vistas in Astronomy*, Vol. 22, pp. 485 – 509, 1978, and Beeson, "Maupertuis: An Intellectual Biography", 1992.
[82] Rene Taton, "Jacques Cassini", in the *Dictionary of Scientific Biography,* Vol. 3, pp. 104 – 106. One of Cassini's last important papers was a defence of Cartesian physics published in 1735 entitled, "De la revolution du soleil et des planétes autour de leur axe, et de la manière que l'on peut concilier, dans le sisteme des tourbillons, la vitesse avec laquelle les planets se meuvent á leur surface, avec celle que l'ether ou le fluide qui les environne doit avoir suivant la régle de Képler", in *Memoires de l'Academie Royale des Science.*, Paris, 1735.
[83] Fontenelle, *Théorie des Tourbillons,* Paris, 1752.
[84] Eric Aiton, "The Cartesian Theories of the Planetary Motions", PhD Thesis, University of London, 1958. Chapter 13, "The Rapprochement between the Cartesian and Newtonian Systems in the prize essays of John and Daniel Bernoulli", pp. 224 – 240.
[85] This quantification became one of the defining characteristics of what was regarded as Newtonian science.
[86] One of Bradley's defining procedures was to set aside the search for causes in favour of a search for some pattern or law of behaviour whereby the parameters of a phenomenon might be clearly apprehended first.
[87] It is not my intention to enter into the lively historiographical debate concerning the reality or otherwise of a scientific revolution. This is debated in Margaret J. Osler (ed.), *Rethinking the Scientific Revolution,* Cambridge, 2000.
[88] I am not suggesting that James Bradley did this consciously, or even that he was aware of any change in the general perception of his most eminent mentor, merely that in his practice he came to epitomise the "Newtonian" approach to scientific knowledge.
[89] B. J. T. Dobbs, *The Foundations of Newton's Alchemy, or "the Hunting of the Greene Lyon",* Cambridge, 1975, reveals some of the influences that Newton's alchemy had on his ideas in natural philosophy, whilst an important paper (written in part by my MSc tutor Piyo Rattansi) in studies of Newton is P. M. Rattansi and J. E. McGuire, "Newton and the 'Pipes of Pan'", in *Notes and Records of the Royal Society of London,* Vol. 21 (1966), pp. 108 – 143, which explores the connections between Newton's religious beliefs and his natural philosophy.
[90] This is the "Newton" that the poet William Blake so hated. Given the restless character of the Newton now being revealed by modern historiography, sexual attitudes aside, Blake may yet have found some affinities between this "Newton" and some of his own unorthodox beliefs.
[91] Blake and his fellow radical Joseph Priestley often argued. One of Priestley's favourite rejoinders when in debate with Blake was "give me *your reason*". Blake epitomised Priestley in the character of "Urizen" in his prophetic writings. To Blake the imagination was liberation, whilst reason was an enslaver. I am unable to give a reference for this but I read the source at least 40 years before writing this thesis.
[92] John Bernoulli, *Essai d'une nouvelle physique céleste,* Paris, 1742, draws on ideas expressed in Maupertuis' "Discours sur les differentes des astres" of some ten years earlier. Bernoulli consciously remained a "Cartesian" and yet he developed an analysis that was identifiably "Newtonian".
[93] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works", p. 18.
[94] Rigaud, "Memoirs", p. lxiv.
[95] Ibid. p. lxv.
[96] Maupertuis to Bradley, 27th September, 1737, "Miscellaneous Works", pp. 404 – 406.
[97] There is no trace of an earlier letter from Maupertuis. We only know of its existence from the contents of the letter from Maupertuis to Bradley in September 1737.
[98] Maupertuis evidently respected Graham's skills and felt obliged to approach the instrument maker through Bradley, probably because his English was inadequate to express his requirements and because he was fairly certain that Graham's French was as equally inadequate. It may also be presumed that Maupertuis had recognised that Bradley was a potential ally in his dispute with Jacques Cassini, and may have regarded the approach to Graham as an opportunity to make his acquaintance.

[99] Bradley to Graham in December 1737, "Miscellaneous Works", pp. 406 – 407.
[100] Ibid. p. 407.
[101] Ibid. p. 408.
[102] Ibid. p. 409.
[103] Ibid. p. 409.
[104] Académie Royal de Sciences, *Memoires de l'Académie des Sciences,* Paris, 1737, p. 411.
[105] Académie Royal de Sciences, *Memoires de l'Académie des Sciences,* Paris, 1738, p. 220.
[106] Bradley, "Miscellaneous Works and Correspondence of James Bradley", Oxford, 1832. See letters to Bradley from De l'Isle, p. 433 – 438, particularly on p. 434, and from Lacaille, pp. 438 – 442, particularly on p. 440, where they accuse Le Monnier of plagiarism.
[107] Académie Royal de Sciences, *Memoires de l'Académie des Sciences,* Paris, 1745, pp. 522 – 524.
[108] Ibid. pp. 512 – 521.
[109] Rigaud, "*Memoirs*", p. xliv.
[110] Bradley to Le Monnier, "Miscellaneous Works", pp. 473 – 475.
[111] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "*Miscellaneous Works*" pp. 17 – 41.
[112] Ibid. pp. 31 – 36.
[113] Bodleian Library, Bradley MS. 19, *Latin Lectures delivered at Oxford by James Bradley.* 128v. Here Bradley wrote "et veræ sint et phænomenis explicandis sufficiant." fol. 128v.
[114] Bodleian Library, Bradley MS. 40, *Papers related to Nutation. Observations, calculations and drafts of the letter to Lord Macclesfield.* fols. passim.
[115] Ibid. fols passim.
[116] Rigaud, "Memoirs", p. lxviii.
[117] Bradley's observational practice was usually capable of securing motions as small as 0.5" but when working at the limits of the sector when the supporting tube was some 4 or 5 arc seconds from the zenith his observations were (according to the theory of nutation) some 1.5 to 2 arc seconds from prediction. Bradley rightly suspected that the weight of the tube was probably causing some distortion once the instrument was moved too far from vertical. This led to Bradley abandoning any reference to these observations in his paper.
[118] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works", p. 37.
[119] Ibid. p. 37.
[120] Each "sign" refers to the nominal 30° covering each sign of the zodiac. The 12 signs make up the 360° of the entire plane of the ecliptic. Thus 4 signs and 20° refers to a point on the ecliptic of $4\times30° (120°) + 20° = 140°$.
[121] Ibid. pp. 25 – 26.
[122] Ibid. p. 36.
[123] Maskelyne, "Greenwich Observations, Vol. I, Explanation and use of tables", p. viii.
[124] Ibid. p.viii.
[125] Bradley, "Observations on the Fixed Stars made at Wansted in Essex by J. Bradley", contained in the "*Miscellaneous Works*", pp. 201 – 285.
[126] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. London, 1726, Book III, Prop. 33, Prob. 14, is followed by a short section written by John Machin, "On the Motion of the Nodes of the Moon", translated by I. Bernard Cohen and Anne Whitman, University of California Press, London, 1999, pp. 861 – 864.
[127] Bradley gave three courses of lectures a year from 1729 to 1749 and two courses a year from 1750 to 1760 at Oxford on experimental philosophy and astronomy.
[128] Bradley was a member of the Council of the Royal Society in various years up to the mid-1740s and continuously from 1749 to 1760.
[129] This will be discussed in Chapter 5 on the work Bradley undertook at Greenwich.
[130] Rigaud, "Memoirs", p. lxviii.
[131] Bradley was in Oxford when his paper was read.
[132] Ibid. p. lxx.

[133] Foulkes was instrumental in the election of Bradley to the Savilian Professorship in Astronomy at Oxford in 1721. This will be discussed in Chapter 4 on Bradley's work at Oxford.
[134] Ibid. p. lxx.
[135] Ibid. p. lxx.
[136] Bradley to Maupertuis, "Miscellaneous Works", pp. 404 – 406.
[137] J. P. de Moliéres, *Leçons de physique*, Paris, 4 vols. 1734 – 1739.
[138] Eric Aiton, "The Cartesian Theories of the Planetary Motions", PhD Thesis, University of London, 1958. p. 248.
[139] The Cartesian essay was written by Antoine Cavalleri, and was a Cartesian argument in the style of Moliéres, of Newton's calculations. The three Newtonian essays, which shared the prize, were written by Colin Maclaurin, the Professor of Mathematics at Edinburgh University, Daniel Bernoulli, who won ten prizes between 1725 and 1749, and Leonhard Euler, who in 1748 was to publish his *Introductio in analysin infinitorum*, which developed the concept of function in mathematical analysis, which was to become a powerful tool in the development of celestial mechanics, founded on largely Newtonian principles.
[140] *Recueil des pieces,* Vol. 4, Paris, 1740. p. 56.
[141] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. Book III, Prop. 33, Problem 14. translated by I. Bernard Cohen and Anne Whitman, University of California Press, London, 1999, pp. 861 – 864.
[142] Rigaud, "Memoirs", p. lxxi.
[143] Bradley, "Miscellaneous Works and Correspondence of James Bradley", p. 455. Jean-Frédéric Phélypeaux Maurepas, was Louis XV's secretary of state. His finest achievement was the reform of the French navy. He lost office the following year in 1749 as a result of a personal quarrel with Madame de Pompadour, Louis XV's mistress. He later achieved high office under Louis XVI.
[144] Ibid. p. 465. Bradley received a letter from Schumacher dated 20th July 1750. In 1754 Bradley was made into a full member of the Imperial Academy of Sciences.
[145] Académie Royal de Sciences, *Histoire de l'Academie Royale des Sciences*, Paris, 1762, p. 241.

# Chapter Four.

# Oxford and Shirburne: Lectures and Observations.

## Introduction.

Although Bradley was elected Savilian Professor of Astronomy in October 1721 he remained living in Wanstead until May 1732. The demands of the professorship were not onerous and until he became Lecturer in Experimental Philosophy at Oxford he preferred to continue living with Elizabeth Pound, his uncle's widow, while he concentrated on his observations of annual aberration at Wanstead.[1] His work with the zenith sector continued as he began his observations of the phenomenon later identified as a nutation. By 1732 his increasing commitments at Oxford demanded that he move from Wanstead to take up residence in the house in New College Lane which came with the professorship. With the help of his aunt, who remained living with him,[2] he soon settled in what was to remain his main abode for the next ten years.[3] Until 1747 Bradley continued his observations of nutation, staying with Elizabeth Pound's brother Matthew Wymondesolde at his estate in Wanstead. Matthew regarded Bradley as a member of his own family.[4]

Bradley's association with Oxford was maintained for a longer period than with any other location in his life. He lived for over 69 years, but maintained some connection almost continuously from when he went up to Balliol College as a commoner on 15th March 1711 until his death on 13th July 1762, and except for a brief interlude from 1717 to 1721 maintained a connection for a period of over 50 years.[5] As a student his intention had been to prepare for the cloth, but as a teacher he was associated with astronomy and experimental philosophy for almost 40 years from the 31st October 1721. His work at Oxford involved three distinct activities. First his work as the Savilian Professor of Astronomy, second his lectureship and later his readership in experimental philosophy, and third his astronomical work at Oxford and at nearby Shirburne Castle, the estate of his patron and friend, George Parker, 2nd Earl of Macclesfield.

It was during his brief career with the church that Bradley began to capitalise on his uncle's connections. These connections were to stand him in good stead, for initially, following the demise of John Keill,[6] it had been Pound who had been proposed for the vacant professorship. It was Thomas Parker the Lord Chancellor, later the 1st Earl of Macclesfield, one of the electors, who strongly proposed Pound for the office, arguing that he was "the fittest man perhaps in Europe,"[7] in a letter to one of the other electors, William Wake the Archbishop of Canterbury. It was generally accepted that Pound would have been elected,[8] if he had not been unwilling to give up his lucrative church preferments in order to conform to the statutes that had been laid down by the founder Sir Henry Savile in 1619.

According to Rigaud the office was briefly considered for the Rev. Joseph Atwell. Fellow of Exeter College, while much interest was directed on behalf of the Rev. John Whiteside of Christ Church, who had formerly been a candidate at the time of John Keill's election in 1712.[9] Almost no one seriously considered Bradley who was little known outside his small circle at Wanstead.

**Bradley's election as Savilian Professor of Astronomy.**

After Pound politely declined consideration for the office, the Lord Chancellor placed his support behind Bradley following prompting by his uncle. From this date, Bradley was to enjoy the patronage and friendship of the Parker family who would soon be raised to the Earldom of Macclesfield.[10] Bradley acquired further supporters. On the 4th of September 1721 a personal friend of his, Martin Foulkes, the future PRS, wrote on Bradley's behalf to his uncle William Wake. Foulkes described Bradley as,

> a gentleman who is thoroughly qualified for that part, and will be, I make no doubt, every way recommended to your grace's satisfaction. ……he has lived for some years with his uncle, Mr. Pound of Wansted, where he has had great opportunities of joining to his theory the practical part of astronomy, in which he has made himself very eminent, having prepared for the press accurate Tables of the satellites of Jupiter, with some other

> curious pieces; and I am satisfied his being professor will do honour and service to the science. I shall only take the liberty of adding, that he is perfectly approved, and will be entirely recommended by Sir Isaac Newton.[11]

With William Wake, the Archbishop of Canterbury and Thomas Parker, the Lord Chancellor of England in his favour it is hardly surprising that Bradley was duly elected to the office on the 31st October, 1721, just two months after the demise of his predecessor.[12] One of these powerful patrons came through his uncle's connections and influence, and the other through Bradley's connections in the Royal Society, which in turn was dependent on the connections made through Pound. As in his short ecclesiastical career the intervention of powerful sources of patronage determined the direction of Bradley's life. Although Bradley had demonstrated something of the skills and aptitude that was to mark his astronomical career, he was almost unknown outside his immediate circle of acquaintances.[13] Other candidates such as John Whiteside could call on powerful sources of patronage from Christ Church. He also had an established reputation as a lecturer in experimental philosophy. The election of Bradley as Savilian Professor of Astronomy was resented by many voices within the university, less out of any antipathy towards Bradley in person, than towards the powerful sources of Whig patronage based in London.[14]

On acceptance of his election, Bradley gave up his church preferments. It is worthwhile reflecting on what he gave up. Rigaud is certainly convinced that Bradley was almost certainly giving up an lucrative career in the church, writing

> The immediate loss could not have been very great, but the sacrifice of future prospects must have been considerable. With such friends as Molyneux and lord Macclesfield, and such a patron as Hoadly, to whom he was now chaplain, there could be no doubt of great and rapid advance in his profession.[15]

Yet to Bradley, astronomy had become in the words of Rigaud, "an all absorbing passion".[16] Although he was elected on the 31st of October, Bradley did not take up the office until the 18th of December 1721. He read his inaugural lecture on the

26th of April 1722.[17] According to Thomas Hearne it did not add to his reputation.[18] Hearne was close to Whiteside who for the second time in barely nine years had been disappointed in his candidacy for the chair in astronomy. Yet over the next few years Bradley was to impress Hearne sufficiently for the non-juror to regard the young astronomer with sufficient approbation to support him in an unsuccessful bid for election to the Curatorship of the Ashmolean Museum.

**Experimental Philosophy in Oxford before Bradley.**

According to John Theophilus Desaguliers it was John Keill who, "was the first who publicly taught natural philosophy by experiments in a mathematical manner",[19] apparently some time in 1704 or 1705.[20] Very much in line with Newtonian practice Keill had argued for the necessity of experimental evidence.[21] Larry Stewart attests to the work undertaken by Desaguliers, as well as the work on chemical forces by Keill, and on attraction and repulsion by Desaguliers, and Francis Hauksbee the Younger on pneumatic chemistry. All owed much to the Queries Newton was to add to succeeding editions of the *Opticks*.[22] A few years later in 1709 Keill left for New York in order to act as treasurer to the Palatines.[23] Following this Desaguliers, "began to teach experimental philosophy after the same method."[24] Desaguliers appears to have begun teaching experimental philosophy in 1710, the same year that he was appointed chaplain to James Brydges, the Earl of Carnarvon, later the Duke of Chandos.[25] Desaguliers continued teaching at Oxford, probably at Hart Hall, until 1713 when he left the university to live in London.

Some time after Desaguliers discontinued his lectures it appears that John Whiteside took up the baton and began courses of lectures in experimental philosophy at the Old Ashmolean Museum in Broad Street. In a letter addressed to Dr. Charlett from John Richardson, dated the 11th of June 1716 he referred to Whiteside's "designe of experimental philosophick lectures."[26] Rigaud suggested that Whiteside gave courses of lectures from the following year 1717 up to and including 1728. By this time Whiteside was the Keeper of the Ashmolean Museum.[27] Whiteside divided the entrance room into two, giving a lecture in one

room before leading his students to the next where a servant then exhibited the experiments that illustrated the subject of the foregoing lecture.[28]

Experimental philosophy was a term that in England was often synonymous with the term "Newtonian philosophy", and all three of the lecturers so far discussed were avowed "Newtonians". Yet politically they appear to have been quite diverse, either in their stated opinions or in the associations they kept. Keill, for instance, was rather disapproving of the Whig influence in the Church,[29] so he would not have approved of most of the patronage enjoyed by his successor in the chair of astronomy. Desaguliers, on the other hand, became very much a man of practical affairs who associated himself with many in the Whig ascendancy.[30] Whiteside, a chaplain at Christ Church, displayed High Church and Tory sympathies[31] and was also a friend of Thomas Hearne who had strong Jacobite leanings.[32] Given the diversity of political and religious opinions held by the various holders of the Savilian Professorship of Astronomy and the Lectureship in Experimental Philosophy it can be cautiously asserted that they were largely in agreement with a Newtonian "methodology".[33] Larry Stewart has asserted, judiciously in my opinion, that this was largely the product of the post-Restoration polemic adopted by Sprat and others in their defence of the aims and objectives of the Royal Society, when all political and religious discussion was proscribed.

The eighteenth century historian of the Royal Society, Thomas Birch who wrote in the context of a triumphal Newtonianism, was to uphold the view that Newtonian Philosophy was "nothing but Observations & Experiments, which are evident to the Sense of all Mankind, & From thence he deduces demonstrative Conclusions; & then again, by ye Assistance of these Conclusions, he explains the Causes of many Phaenomena in Nature."[34] This is rather disingenuous for an entirely different "style" of scientific investigation was in vogue in France throughout much of the early 18th century, the practitioners of which would have argued equally forcefully in favour of the use of the senses and philosophical demonstration. In this milieu the "English" tradition was regarded as a retrogression to the very occult qualities which the practitioners of the mechanical

philosophy regarded as so reprehensible. The Swiss mathematician Johann Bernoulli, though not fully in accord with the leading lights of the Paris Academy of Sciences revealed a common response to this "English" tradition when writing to Maupertuis after reading the French philosopher's *The Shape of Celestial Bodies* in 1733.

> If you appear English to me, Monsieur, you appear to be a reasonable Englishman, who does not push the principle of attraction beyond geometry into physics, as the rigid followers *(sectateurs)* do, more than Mr. Newton himself did."[35]

Bernoulli had long opposed Newtonian philosophy and had been Maupertuis's mentor for several years, but even when he recognised that his protégé was surreptitiously entering the "Newtonian" camp, he still chose to regard Newton's *Principia* as an ingenious geometrical treatise, which unfortunately did not accord with the physical world. However, this was certainly not the view of the overwhelming number of natural philosophers in England, who regarded it as virtually self evident that Newton's *System of the World* was confirmed explicitly by observation, experiment and demonstration and often regarded the "French" national philosophical tradition with a traditional national contempt.

**Bradley's succession to the Lectureship in Experimental Philosophy.**

Throughout much of the 1720s Bradley's sojourns in Oxford were minimal, being little more than to present the small number of lectures required by the statutes of the office.[36] He barely made use of the house in New College Lane that came with the Savilian Professorship, living for most of this time at his aunt's house in Wanstead as determined by the Wanstead Observation Book,[37] which records all of his observations there from 1718 to 1740, and the record of his astronomical lectures at Oxford from 1722 to 1733.[38] After the publication of the paper on the new discovered motion in January 1729,[39] Bradley's profile moved into greater prominence.

Yet in spite of this growing eminence, his financial position if not parlous was nevertheless insufficient for his needs if he was to maintain his life as a

gentleman.[40] Since the untimely death intestate of his uncle Bradley had depended inordinately on the goodwill and support of Pound's widow.[41] It is difficult therefore, to understand how Bradley was able to purchase the scientific apparatus that had belonged to Whiteside in 1729, unless he received some degree of financial support. Hearne reports on the 24th of January, 1729, and this was only eight days after Bradley's paper on the new discovered motion had been read at the Royal Society,[42] that Whiteside had "sold his apparatus, the best that is known, for more than four hundred pounds to Mr. Bradley."[43] However, some three weeks after this memorandum Hearne states that he had been informed that Bradley, "gave only an hundred and seventy pounds, tho' I had been before told, that he gave four hundred pounds for them. Mr. Whiteside, several years ago, valued them at five hundred pounds, and at last at eight hundred pounds."[44] Soon after this Bradley took over the rooms at the Ashmolean Museum that Whiteside had used.[45] It appears that Bradley had purchased Whiteside's instruments for a comparatively small sum in comparison to his predecessor's own valuation. Hearne suggests that Whiteside had been reluctant to continue giving lectures in experimental philosophy, perhaps because the attendance was low and subsequently the income was too.[46]

It can be assumed that as Bradley moved to the conclusion of his work on the new discovered motion at Wanstead, he gave serious thought to how he might increase his income, and subsequently effect his liberation from the obligations he undoubtedly felt to Pound's widow and possibly his friends. He must have been aware that Whiteside wanted to sell his instruments for as soon as his paper had been read he had entered into negotiations to purchase them.[47] The price may have been dictated in part by Whiteside's anxiety to be rid of them, and in part by the comparative paucity of Bradley's resources. This must remain conjectural. Rigaud wrote,

> Bradley's character was established by the discovery of aberration, and he probably was successful from the beginning of his new engagement, but we have no document to give us information on this point at so early a period.[48]

Rigaud also had to resort to conjecture on this point, for there are simply no documents which can be referred to over this important transition in Bradley's scientific and financial affairs.

There can be little doubt that Bradley kept careful records of his financial affairs during this period. They are no longer extant.[49] We can estimate that Bradley's income during the 1730's was probably in the order of £725 per annum. There is a record of attendance at Bradley's lectures from 1746 until 1760,[50] so it may be possible to infer what sort of income he derived from his lectures in experimental philosophy prior to 1746. Bradley gave 79 presentations of his course (usually of 20 lectures), from the spring of 1729 onwards. Of these there exist records of 33 presentations. He charged 3 guineas (£3.15) to register for his course, or 1 guinea for a second attendance. The average attendance for each course was 57, which included not just students but also some masters. The number of students attending twice is very low so it can be calculated that his average income for each course was about £175, and as he offered three courses a year up to 1749 (two a year thereafter) it can be estimated that his annual income from his lectureship was of the order of £525 per annum, (£350 after 1749). With the inception of the readership he acquired another £30pa from 1749. Added to the stipend from his professorship which after various fees and charges had been paid amounted to about £200, it can be estimated that during the 1730s his income was of the order of £725 per annum, well in excess of what was required for his needs, particularly as his lodgings at Oxford were almost ex-gratia except for a small annual fee. By the mid 1730s Bradley's finances were secure. After 1742 he was able to add his income as the Astronomer Royal to this (some £100 pa), which from 1751, due to the intervention of Henry Pelham, he received a pension of £250 pa and income from investments (a substantial sum after he inherited from Elizabeth Pound), so it can be estimated that even without his investments he would have a commanded an income of just under £1,000 pa. J. L. Heilbron discussed professorial finances in the 18th century, remarking that the inability to exact fees for statutory lectures encouraged the development of additional services and courses for which fees could be demanded.[51] Heilbron states that

salaries varied according the wishes and the wealth of the founder of the chair, the prestige of the subject, and the date of the foundation. The Lady Margaret Chair of Divinity at Cambridge brought some £1,000 during the 18th century, while law and medicine still brought the £40 per annum set during the reign of Henry VIII. Heilbron adds that with £100 a don would be comfortable (given that most of his daily needs would be met by his college), though with £40 he would be near destitute. Bradley's professorial income of some £200 would therefore, according to Heilbron, be regarded as comfortable, but it must be remembered that Bradley was not supported in the same way, and had also to finance a more expensive life-style due to the cost of scientific instruments and other appurtenances in both Oxford and Wanstead. His professorial salary alone was barely adequate and not sufficient to maintaining his independence as a gentleman.

In 1721, Nathaniel Crew, the Bishop of Durham left £200 a year to the University of Oxford, and in 1731 a decree of the Convocation of the University set aside £30 for "a readership in experimental philosophy."[52] However, Bradley saw nothing of this for several years for Lady Stanwell had first claim on the bishop's estates, a rent charge for the duration of her life. It was not until 1749 that Bradley derived any benefit, after which he was also styled as "Reader in Experimental Philosophy".[53]

When Bradley took up his office as the Savilian Professor of Astronomy in 1721 his appointment had been resented, as is obvious from the remarks of Hearne.[54] Although Bradley was a man with strong Whig associations, who moreover had served as chaplain and private secretary to Benjamin Hoadly, one of the most controversial churchmen of his day, a man implacably opposed to the Tories and the High Church, Hearne a man with strong Jacobite sympathies nevertheless strongly supported Bradley's claim in 1731 to become the Keeper of the Ashmolean Museum in succession to his friend Whiteside. Hearne wrote,

> it was expected at London, and by the most understanding men of the university and much wished too,[55] that Mr. Bradley, Savilian Professor of Astronomy, should have had this post, being a person every way qualified with respect to skill in mathematics, (though he is no antiquary.)

and being a man that performs courses of experiments at the Museum in the great lower room.[56]

If Bradley was able to so impress a man with Hearne's prejudices and sensibilities and overcome his initial resentment and even elicit his support in an election, in spite of the candidate's obviously different associations and beliefs, then it offers an insight into Bradley's ability to attract so much support and patronage during his life. Given the rather raucous life of many at Oxford University,[57] and the outright debauchery that was widely observed by Hearne, a man of Bradley's modest habits would have been noted by the university proctor.[58] Thomas Hornsby, who knew both Bradley and Rigaud, remarked to his first biographer that Bradley was, "peculiarly kind and gentle in his manners;"[59] This genteel demeanour is another key to understanding why, in addition to his undoubted astronomical skills and his depth of experimental and natural philosophical knowledge, Bradley was able to elicit so much support from so many quarters throughout his life.

**From Wanstead to Oxford.**

After the publication of the paper on the new discovered motion, Bradley's observations at Wanstead continued intensively whilst he sought the causes for the residuals between the law of this motion and the results of his observations. Bradley's commitments at Oxford had increased, as he initiated his first courses in experimental philosophy at the Ashmolean Museum.[60] By early 1732 his observations of the nutation had settled down to a routine[61] and he recognised that he was committed to a programme of observation that would last for the full cycle of the moon's nodes, a period of over 18 years. In the meantime his lecture commitments at Oxford had become more demanding. By May 1732 Bradley decided that had to move his main abode from Wanstead to Oxford, moving from the house owned by Matthew Wymondesolde and lived in by his sister Elizabeth Pound, taking up permanent residence at the house in New College Lane. Mrs Pound moved with Bradley to Oxford and her house was let to Mrs Elizabeth Williams. Bradley made a friendly arrangement with Mrs Williams

in order to maintain his observations of the nutation whilst visiting and staying with the Wymondesolde family in Wanstead.[62] Bradley's activities at Oxford, which became the centre of his life from 1732 until he took up his post in Greenwich in 1742, were centred on his teaching duties. As already discussed in Chapter 3 of this thesis, his work on the nutation continued. His contemporaneous work on isochronal pendulums was discussed in connection with the work at Wanstead. He did not abandon his observational work, however, for he took most of his instruments with him from Wanstead, including the transit that had belonged to Pound[63] and his 15 foot telescope.[64] The transit instrument was set up in one of the windows of the (Old) Ashmolean Museum.[65] Neither Rigaud nor Hornsby were able to determine where Bradley set up the 15 foot instrument, as neither the yard attached to his house in Oxford nor the museum had sufficient room for the telescope to be used. Wherever the 15 foot telescope was situated he continued his observational work at Oxford and later in the 1730's he planned and helped construct the finest observatory in England at Shirburne Castle, the country seat of his friend and patron, the 2nd Earl of Macclesfield, only a few miles from Oxford.[66]

**The work on John Hadley's nautical octant.**

Although Bradley had moved his main abode to Oxford he maintained his wide interests and remained in touch with most of the group that he had met so regularly in Wanstead and London. One of this select group was John Hadley who published a paper on what was really a mariner's quadrant or a double reflecting octant, a viable forerunner of the modern nautical sextant.[67] Many claims have been made on behalf of Thomas Godfrey of Philadelphia as the effective inventor of the mariner's quadrant, and there is no doubt that the claim of the American glazer is a good and valid one. Benjamin Franklin forcibly promoted Godfrey's claim after his early demise, partly because his son (also Thomas Godfrey) was a member of his Junto Club in Philadelphia. Yet Hadley invented a similar instrument independently and possessed the advantage of ready access to the centres of power in the Admiralty.

Rigaud also rightfully accepted Godfrey's claim and proffered the view that such simultaneous inventions are made in response to common problems;[68] although he also asserted that the instrument was really constructed on principles laid down by Newton.[69] The Admiralty ordered sea trials of Hadley's instrument,[70] and although these would normally have fallen to the interest of the Astronomer Royal Edmond Halley, his failing health led him to propose Bradley, especially as he had already worked closely with Hadley on his earlier reflector. Bradley boarded the Chatham yacht and took part in trials in the Thames estuary and further out in the North Sea during the late summer of 1732.[71] There is a letter to Hadley in Bradley's correspondence which dates from the end of 1732, after the trials had been completed. The potential of the instrument was obvious to Bradley. The double reflecting octant was designed to locate a ship's latitude with unprecedented accuracy even in poor seeing conditions. With the 1714 Longitude Act in mind Bradley wrote,

> I am now convinced, that by means of this contrivance, we may observe at sea with a degree of exactness that 'twas before vain to expect, and which will undoubtedly contribute very much towards the perfection of navigation, not only in determining the latitude to great exactness, and thereby correcting the course as usually measured by the log; but if ever the moon's theory is completed, 'twil be of utmost consequence in determining the longitude likewise. … and the observations made the next day, when under sail, make me inclined to think that a little practice will be sufficient to enable any one to take the latitude in tolerable weather, without error of a single minute.[72]

The astronomical route to the determination of the longitude at sea was to remain of central importance to Bradley, and very specifically after he became Astronomer Royal in 1742. Later the double reflecting octant would be superseded by the sextant with an arc of 60° instead of 45°, which in conjunction with lunar tables enabled mariners to calculate longitude from observations with the sextant. In 1755 Johann Tobias Mayer published very accurate tables of the motion of the Moon.[73] Bradley supported Mayer's claim for a share of the

Longitude Prize. From 1766 the fifth Astronomer Royal, Nevil Maskelyne supervised the annual publication of the *Nautical Almanac,* adapted for the use of mariners from Mayer's tables. Later in the century chronometers based on John Harrison's H4 were preferred by mariners for their convenience and ease of use, and were commonly in use in ships of the Royal Navy. Even so astronomical methods utilising the *Nautical Almanac* that required painstaking mathematical skills were utilised to cross check the accuracy of the chronometers.

**The Astronomical Observations at Oxford and Shirburne Castle.**

Bradley formed a lifelong friendship with George Parker during the 1720s following the intercession on his behalf by his father Thomas Parker, the 1st Earl of Macclesfield, Lord Chancellor of England, during his election as the Savilian Professor of Astronomy. George Parker succeeded to his father's title as the 2nd Earl of Macclesfield in 1732,[74] the year that Bradley moved his residence to Oxford, when he took up the house in New College Lane that came with the professorship. The friendship was important not only for Bradley's advancement in society, but also for the continued development of his astronomical skills. The work that he undertook with Macclesfield at Shirburne Castle, the country seat of the Parker family, only 17 miles from Oxford, was to be of paramount importance after Bradley succeeded Halley as the 3rd Astronomer Royal, especially in connection with his development of the new observatory at Greenwich constructed from 1749 to 1750.

The 2nd Earl of Macclesfield possessed both an observatory and a laboratory in the grounds of his estate. Throughout much of 1738 Bradley and Parker began to plan the development of a fine new observatory at Shirburne Castle.[75] On completion it would be unrivalled as the finest observatory in England, a status it would arguably retain until Bradley developed the new observatory at Greenwich. The observatory at Shirburne was situated some 100 yards to the south west of the main gate of the Castle.[76] The first transit observations are dated from December 1739 and are almost entirely by Bradley; Rigaud conjectured that Bradley was seeking to bring the instrument into proper

adjustment.[77] On the 4th of June 1740 Macclesfield began his regular observations which were to continue until his death in 1764. There can be little doubt that astronomy was as much the ruling passion of the 2nd Earl's life as it was Bradley's, but he was compelled by his social position and political connections to contain his scientific pursuits to his leisure periods, unlike his friend, who was able to devote the greater part of his life to his observations. Throughout much of the period between 1740 and 1760 entries in Bradley's hand appear regularly alongside the 2nd Earl's, for they maintained a close working relationship which continued after Bradley became the 3rd Astronomer Royal. During the winter of 1740 – 41 Bradley and Macclesfield regularly observed ζ Ursæ Majoris and *Alcor*.[78]

The Shirburne Observatory contained three rooms.[79] One was fitted as a bedroom for the Earl, which he sometimes occupied when he passed the night during observations. The second room contained what was then commonly agreed to be the finest transit instrument in England. The third room contained a mural quadrant where an assistant also had a bed. Both instruments were designed and constructed by Jonathan Sisson,[80] whom Bradley was again to employ extensively during his early years at Greenwich. Initially Macclesfield made observations on his own, occasionally with help or assistance from Bradley, but soon he employed the family servant Thomas Phelps to work with him. Later, another servant John Bartlett also assisted.[81] According to Rigaud there was an engraving of Bartlett preparing to write down an observation made by Phelps at the transit.[82]

The focal length of the transit instrument at Shirburne was 5 feet, though the instrument was some 5 feet and about 3 inches long. The instrument possessed by Halley at Greenwich was about 5 feet and 6½ inches long but this instrument too possessed a focal length of some 5 feet. At Shirburne the arms of the axis were conical, being supported by two stone pillars. It originally possessed three wires, but it was eventually increased to five. The quadrant had Graham's division of 96 on the limb as well as that of 90°. During the summer of 1745 a new set of divisions were made on the quadrant by Bird when he demonstrated that Sisson's quadrant was 10" in error.[83] After it was put up again the quadrant was again

tested, "It appeared upon a medium of several trials made by his Lordship. Dr. Bradley, and Mr. Bliss"[84] In a letter to the French astronomer De l'Isle Bradley explained that that between the 18th and the 28th of September 1745, he

> made several experiments in order to discover the true quantity of the arc of the outer divisions of the mural quadrant, as hung on a wall, by means of a curious[85] level and apparatus contrived by Graham for that purpose.[86]

The Shirburne observations were recorded in a series of registers, the transit instrument dated from June 1740 to June 1787, and the quadrant from January 1743 to March 1793. Over this period the adjustments to the instruments were made by Bradley, Nathaniel Bliss and Thomas Hornsby. In his paper on the nutation published in 1748 Bradley referred to the use he made of the Shirburne observations.[87]

The transit instrument at Shirburne possessed many advantages over the transit designed for Halley at Greenwich. By way of example there is a letter from Nathaniel Bliss to the Earl of Macclesfield in which wrote,

> Dr. Bradley is pleased that the comet has been seen on the meridian at Shirburn (sic), because his nephew could not find it at Greenwich;[88] the reason of which he supposes is, because they have no tube on the end of the telescope to screen the object-glass from the sun.[89]

This and other factors clearly motivated Bradley to improve and then replace the various instruments he inherited from Halley at Greenwich.

Bradley's first recorded observation after moving to Oxford was on the 13th of June 1732. On the 6th of December 1732 he prepared to observe an eclipse of the sun. His notes began with an observation so familiar to English astronomers, "An eclipse of the sun observed at Oxford. Clouds prevented my seeing the beginning."[90] He observed the eclipse from 9h 6m 0s carefully measuring the size of the chord from then until 9h 27m 15s/30s[91] when the eclipse ended.

Bradley was not best situated to observe this eclipse for it was only partial. Oxford was well away from the centre of the phenomenon, but he used the event

in order to clarify the accuracy of his instruments. Even though this eclipse of the sun was not well placed for observers in England, British astronomers were singularly fortunate during this period, for total eclipses of the sun were observed on the 22nd of April 1715, the 11th of May 1724 and the 14th of July 1748, and annular eclipses of the sun were observed on the 18th of February 1737 and the 1st of April 1764.[92] On the 23rd of September, 1736 Bradley observed a glimpse of another partial solar eclipse, this time with his 7 foot object glass. He wrote,

> 4h 39m 5s app. time, I observed the beginning of an eclipse of the sun in a seven foot glass, and though I could not (by reason of clouds) make any farther observations, yet I take this observation to be pretty exact, so as to be depended upon within 5".[93]

On the 18th of February 1737, Bradley observed the annular eclipse of the sun. This began at 2h 34m 30s. His observations of the eclipse were maintained until it ended at 5h 12m 15s or 17s.[94] In both of the eclipses that he had observed for an extended period Bradley used his timings of the eclipse to cross check the accuracy of his clocks. Even though the centre of this annular solar eclipse passed through the British Isles, from the west of Ireland through Ulster, Galloway and Edinburgh, Bradley situated in Oxford only observed the partial phenomenon.

The most numerous of Bradley's observations at Oxford were of the Jovian system in pursuit of the rectification of the ephemerides of Jupiter's satellites. These observations were made with the 15 foot telescope.[95] On the 30th of June 1732 an entry in Bradley's Oxford Observation Book reads, "9 h 32m 0s m. t. the 1st sat. of Jupiter began to emerge in the 15f. tube."[96] On the 2nd of August 1733 the 15 foot telescope was again in evidence,

> 8h 41m 15s app. time, the 3d sat. of Jupiter immerged (at Oxford) in my 15f.tube: Jupiter was low, but appeared pretty well, and the observation, I think, pretty good, and may at least be depended upon within the minute.[97]

There are many other similar observations in his observation book, which were in continuation of the extended series of observations he had undertaken on behalf of Halley.

Another common observation was of planetary and stellar occultations. A fine example is of an occultation of Mars by the Moon on the 7th of October, 1736 recorded in the Oxford Observation Book.[98] A small comet appeared during 1737 and in 1738 Bradley published the observations he had undertaken together with the elements of the comet's parabolic trajectory which he calculated from these using the methods developed by Newton and Halley. These elements, based entirely on his own observations, differed by less than a minute of arc from later calculated positions. It was a truly remarkable achievement given that the comet was excessively faint, even in a moonless sky. Bradley stated,

> I first saw the comet on the 15th of February 1737, between six and seven in the evening, when its nucleus appeared small and indistinct, and its tail (extending above a degree from the body) pointed towards the star in Lino Austral. Piscium, marked ξ by Bayer. Applying my micrometer to a good seven foot tube, I observed, that at 7h. 32' temp. æquat. The comet preceded the said star 1° 1' 40" in right ascension, and was 20' 20" more southerly than the star.
>
> Assuming the place of this star as it is settled in the British Catalogue, (as I shall likewise the places of others hereafter mentioned,) it follows, that the comet's right ascension was 23 ° 58' 0" and its declination was 1° 31' 55" north.[99]

Bradley observed it closely night after night carefully measuring its position relative to the stars of the British Catalogue, usually with his 7 foot, but sometimes with his 15 foot telescope. The last observation was made on the 22nd of March, 1737.[100] Thereafter the light of the moon was such that the faint comet, which had become increasingly difficult to observe, became impossible, even for a skilled astronomer.

Bradley produced a table which compared the observed positions of the comet with those which he calculated from Newtonian theory.

| | A | B | C | D | E |
|---|---|---|---|---|---|
| | Oxon. 1737. Temp. Æquat. Lat. | Com. Longit. Observat. | Lat. Aust. Observat. | Com. Longit. Computat. | Lat. Aust. Comput. |
| | D. h. ‘ | o ‘ “ | o ‘ “ | o ‘ “ | o ‘ “ |
| Feb. | 15 7 32 | 22 45 7 | 7 53 27 | 22 45 0 | 7 53 1 |
| | 17 7 33 | 26 30 30 | 8 27 21 | 26 30 44 | 8 28 6 |
| | 18 7 14 | 28 18 14 | 8 44 20 | 28 17 46 | 8 43 57 |
| | 21 7 25 | 33 26 34 | 9 26 50 | 33 26 53 | 9 26 46 |
| | 22 7 45 | 35 4 53 | 9 40 00 | 35 5 28 | 9 39 27 |
| | 25 7 45 | 39 42 18 | 10 12 21 | 39 41 19 | 10 12 22 |
| | 27 8 45 | 42 36 43 | 10 31 42 | 42 36 16 | 10 31 13 |
| Mar. | 4 8.00 | 49 3 0 | 11 6 46 | 49 3 5 | 11 7 8 |
| | 12 8 25 | 57 49 58 | 11 43 3 | 57 49 53 | 11 43 19 |
| | 14 9 00 | 59 47 42 | 11 49 59 | 59 47 19 | 11 49 26 |
| | 17 8 40 | 62 30 57 | 11 56 31 | 62 30 50 | 11 56 49 |
| | 19 7 50 | 64 12 36 | 12 0 19 | 64 12 45 | 12 0 47 |
| | 9 00 | 64 15 11 | 12 1 12 | 64 15 13 | 12 0 52 |
| | 20 8 05 | 65 3 10 | 12 3 5 | 65 3 32 | 12 2 33 |
| | 22 8 15 | 66 41 30 | 12 6 15 | 66 41 19 | 12 5 42 |

101

Bradley concluded his paper in his characteristically modest tone, asserting that the close agreement between his repeated observations of this faint comet and the computations he derived from Newtonian theory,[102] suggest that neither his observations nor his calculations can be “far from the truth”,[103] and therefore can be of use to future astronomers in being able to distinguish one comet from another on its return, settling its period,

> which I cannot at present pretend to do, not having met with an account of any former comet that seems likely to have been the same with this, whereof a description has been given particular enough to determine this point.[104]

In 1743, the French astronomer Pierre Le Monnier published his *Théorie des Comètes.*[105] In this case, unlike with Bradley’s work on nutation, Le Monnier openly acknowledged his debt. Later the French astronomer Jean-Baptiste-Joseph Delambre, who possessed a low opinion of Le Monnier’s work complained in his *Histoire de l’Astronomie au xviii siècle* of the obscurity of Bradley’s work,[106] suggesting that Bradley, Halley and Le Monnier had all attempted to keep the method of calculating cometary orbits to themselves. This notwithstanding the fact that Bradley clearly and openly expressed the view that he had used the method of calculating parabolas using Newton’s method.[107] Rigaud in turn was highly critical of Delambre.[108]

**An Introduction to the Experimental Philosophy Lectures.**

Although Bradley had moved his abode from Wanstead to Oxford, his activities, although based primarily in the university town, were not necessarily confined to Oxford.[109] His work on the nutation necessitated regular journeys to Wanstead, and his work on the octant and other matters took him to Chatham and London. Nevertheless, his life was increasingly centred on his university activities. Of these activities the most demanding were his lectures in natural and experimental philosophy. Although Bradley did not fully write out his lectures, he sketched them out,[110] and one or two student books have been preserved which record the lectures. On the very first page of his notebook Bradley wrote out the purpose of his lectures: viz.[111]

> The design of this Course[112] being to explain several of [the] principle Phenomena of Nature and to give some Account of their Causes as far as they depend upon the Situation. [The] motion of Bodies and the general laws of Nature according to which the motion of Bodies are performed. It may be proper to begin by considering Body or matter in general with respect to its most obvious Properties[113] and then to proceed to inquire into the Laws of Nature by Examining the Natural Phenomena from whence they must be deduced.[114]

Even a brief examination of Bradley's notes suggests that he conceived of natural and experimental philosophy as a rational activity reducible to mathematical analysis. In this Bradley might be characterised in terms of what most educated people in the later 18th century conceived as "Newtonian". It was an activity in which observations are analysed so that the "laws of nature" may be deduced. From these, predictions can be made and then tested against further observations. These are the procedures which Bradley applied when he isolated both the aberration of light and the nutation of the terrestrial axis. As in his astronomical activities Bradley's lectures reveal a distinctive practical aspect that was so often characteristic of many of Newton's disciples. Constantly his lectures are illustrated by practical demonstrations that appealed to a society where the applications of Newtonian mechanics often created substantial fortunes.[115] Larry

Stewart astutely recognises that it was the assertive proselytisation of aggressive "apostles" such as Desaguliers and John Harris that placed Newtonian philosophy before a public to whom the *Principia* would forever remain an arcane mystery. Stewart writes, "In order to appreciate the social impact of natural philosophy, at any point, it is necessary to admit a wider society exists – in this case, beyond the Royal Society."[116] It would be all too easy to underestimate the impact of Bradley's lectures in experimental philosophy, but it must be appreciated that his audiences were made up largely of members of Oxford University, who on graduation would often enter society in privileged positions leading to the holding of important public offices. Bradley's notes reveal a sound introduction to the fundamentals of Newtonian natural philosophy and experimental physics.

The authenticity of Bradley's sketched out lecture notes is verified by reference to lecture notes written by at least one of his students. Of particular interest is the notebook written by R. Heber in 1754.[117] Unfortunately, there is no record of a student of that name[118] so whether they were notes taken by a student on another's behalf, or whether they were written down by another unrecorded attendee I have not yet been able to ascertain. There can be little doubt that they were a record of Bradley's lectures for they faithfully follow the pattern of Bradley's own notes, upon which he must have expounded freely.[119] They are not in Bradley's hand.

At the opening of the first lecture of the course in March or May 1754,[120] Heber records the following,

> The purpose of this course of Lectures is First to prove by Experiment what properties belong to matter or Body – Secondly to show how the Phenomena or Appearances of Nature, that is, the Constant settled Rules according to which Effects are produced in the Material World.
>
> That these are such a Law is sufficiently apparent from a survey of Nature, either in the Animate or inanimate World: Similar causes still producing similar Effects, according to what one may call stated Laws, to which by the Creator's appointment they conform to themselves.[121]

What is clear from this account is that as well as being rationally apprehensible the entire material universe is entirely dependent on God's volition. This voluntaristic theology is highly characteristic of Newton and some of his followers.[122] That the material world is "rational" is really an expression of God's loving providence. Newtonian natural philosophy, was distinct from its natural theology,[123] but created many of the grounds on which such a voluntaristic theology could be constructed.

As I have already argued, Bradley's earliest notebook for lectures was penned sometime during the early 1730s when many of the issues dividing the Newton's followers and the "Cartesians" had not been resolved. I think it is important to clarify what these issues were. In Mary Terrall's recent study of the life and work of Maupertuis she succinctly set out the main characteristics subsumed under the term "Cartesian". She writes,

> When someone was called a Cartesian in early eighteenth-century France, it might imply any or all of the following: belief in a material plenum with vortices of subtle matter carrying the celestial bodies; commitment to "system", which usually involved hypothesizing about causal mechanisms beyond the reach of observation; matter defined by extension, with no added fundamental properties; "rationalism" rather than "empiricism"; conservation of quantity of motion ($mv$) in the universe; measuring the force of bodies by their quantity of motion rather than by their "living force" ($mv^2$); refusal to accept forces acting across empty space; equating such forces with "occult qualities" of scholastic philosophy; observable effects of gravity caused by pressure of matter in celestial vortices. Many of these terms were rhetorically laden, and many of the concepts were conflated together.[124]

René Descartes, as one of the founders of the mechanical philosophy had explicitly denied that there were any powers or forces in nature. Matter, according to his belief was entirely passive and inert, set in motion by God at the creation of the universe in a perfect clockwork mechanism without any need of intervention.[125] However, it was Nicolas Malebranche who took the denial of the

existence of force to its extreme.[126] Any power or force in nature he argued would be power removed from God. When two bodies collide no force is exerted between them. This gives rise to his doctrine of "occasionalism". The collision is merely an "occasion" for God to act. As there can be no activity in matter it is God who is responsible for changing the motion of bodies.[127] To Malebranche the search for causality within a material universe was vain. Yet even though the French philosopher was a Catholic priest and Newton was acerbic in his denunciations of "popery", in their various syntheses of mechanism, neo-Platonism and theology Malebranche and Newton were not so far removed from each other. Later in the eighteenth century David Hume was to expose the metaphysical basis of the new physics, and reduce "causality" to little more than a psychological expectation. Hume denied that a theory of reality was possible and that it was useless to base knowledge on anything but experience. In this Hume had taken what he conceived was the Newtonian model of scientific method.[128]

I think, therefore, that terms such as "Newtonian" and "Cartesian" should be treated somewhat guardedly and cautiously, and never without specific reason, clearly defining what the issues are when a conflict between the two contending traditions or styles raises its head. Certainly, an examination of Bradley's papers and correspondence reveals his evident distaste for such philosophical point scoring,[129] preferring to dedicate his work to observations and experiments to demonstrate, at least inductively, the principles of natural philosophy within the style or tradition that was so often described by the epithet "Newtonian".[130] Bradley's lectures were presented within an ethos in which such observations and experiments were "self-evidently" demonstrative of the laws of nature.[131] These principles and laws were presented and demonstrated in a course of twenty lectures. By 1754, when R. Heber recorded Bradley's lectures, the Cartesian world view had all but been abandoned in France except for one or two inactive authorities such as Fontenelle.[132] The early lectures, written during the 1730s when much of the distinctive and contrasting styles of "French" and "English" physics remained unresolved, contrasted the two systems whilst asserting the validity and veracity of the world system set out in the *Principia*.[133] It would be

incorrect to assume, however, that the Newtonian style entirely superseded the Cartesian, for various habits of thought were in fact retained from the French or Cartesian approach and were quietly and surreptitiously adopted within the Newtonian tradition.[134]

**The Opening Lectures: An Introduction to the Laws of Motion.**

The first and second lectures of Bradley's course were an introduction to the laws of motion.[135] However, Bradley first deals with the Peripatetic philosophy and states,

> The Ancient Philosophers dealt little in Experiments in order to find out the properties and Laws of Matter and to explain the appearances that happen in it. And yet they were as forward to undertake the Explanation of Phenomena, as the Moderns are. This they did by making abundance of suppositions or Hypotheses, which, without proving them, they laid down as principles, which they required to be admitted for certain truths, and these being admitted they could readily assign the Reason of any Phenomenon that happened. Thus if you asked them why water rises in a Pump upon your working it, they would first make you swallow an Hypothesis. Nature they would say abhors a Vacuum, and this being granted the cause of the waters rising in the Pump appeared plainly to be Nature's abhorrence of a Vacuum, to prevent which she sent up the Water into the Pump all along.[136]

This assertive assault on Peripatetic philosophy is, however, a preparation for a much more vigorous denial of many of the presuppositions of modern non-Newtonian natural philosophy.[137] In early 18th century England the term "experimental philosophy" was virtually a synonym for Newtonian philosophy and a course in experimental philosophy was usually a presentation of the central concepts of Newtonian physics, as developed from both the *Principia* and the *Opticks*. In the preliminary to his dismissal of the claims of Cartesian natural philosophy Bradley asserted,

> To know anything of the Nature of Body we must not guess and suppose, but make trials and Experiments which diligently pursued have led Men to discover many Articles of Natural Knowledge, which the most ingenious Hypothesis could never have brought to Light.[138]

It was Bradley's contention that Cartesian natural philosophy was based on ingenious but misleading hypotheses, which were not supported by experiment or common experience. In a section of the first lecture on the properties of matter, Bradley contrasted the nature of matter as apparently revealed by experimental philosophy with the assertions of what he conceived to be Cartesian natural philosophy.

Accordingly, in Heber's notebook under the heading "Of the Properties of Matter" Bradley stated,

> The Universal Properties of Matter are 1st Extension which is threefold, magnitude of length, breadth and thickness. 2nd solidity or that Property by means of which any Body resists any other Body from coming into its place while it keeps its Place. This Property is prov'd to belong to Air and Water as much as to Adamant.
>
> The Cartesians maintaining that Nature is full of Matter make Space to be matter and consequently solid contrary to common sense and Experience.[139]

Thus initially Bradley contrasted the Newtonian concept of matter with the Cartesian view. The French philosopher had conceived of the world being divided into mind and matter. Thought constituted the nature of mind, and thought did not possess the property of spatial extension. In contrast Descartes repeatedly asserted that matter was identical to spatial extension.[140] Just as thought was inconceivable without mind, then extension was impossible without matter.[141] However, as Daniel Garber reveals, Descartes' assertions were perhaps less settled than they have usually been represented, for in the *Meditations* and the *Principles of Philosophy* he sometimes reverted to scholastic terminology such as "ratio", "accident", "quality", "attribute", "property" and "mode" to represent his views about matter and extension.[142] Bradley presented the accepted Newtonian

viewpoint because his presentation of the laws of motion was also a polemic directed against a style of natural philosophy that was represented as being little better than the Peripatetic philosophy it purported to replace. Bradley, as a painstakingly diligent empiricist whose contingent natural philosophy also supported a voluntaristic natural theology, was directing his ire against any notion of "system", ancient or modern. Bradley cited experimental evidence and intuitive insights to contrast the Newtonian concepts of matter, prior to his presentation of the laws of motion. The concept of extension includes the notions of length, breadth and height or thickness, but these properties are contrasted with that of solidity. In Bradley's presentation the Newtonian conception of matter makes a radical distinction between matter and void. Matter is not identified with the concept of extension.[143]

According to the Heber memorandum Bradley then discussed the properties of divisibility and mobility.[144] When he alluded to the property of mobility Bradley discussed the distinctions between velocity and momentum. He referred to the analogy of a horse and a dog moving with an equal velocity, but emphasised that the consequences of a collision with a horse were likely to be more far reaching than colliding with a dog. Bradley referred to the momentum or the quantity of motion as the velocity combined with the "quantity of matter".[145] The first lecture concluded with a brief introduction to the fifth property of matter, the "vis inertiæ" of a body, its "propensity to continue in its state of Rest till acted upon by some outward force".[146]

After elucidating the nature of matter, with an emphasis on an implied demonstration of the passivity of matter, and the implications of this passivity, Bradley continued the themes in the second lecture when introducing his students to the laws of motion. Bradley opened his lecture, as reported by Heber,

> We are next to set forth and explain the Laws or Rules which Matter is found to observe whether it is at rest or in Motion, or suffers a Change from the one to the other, or from one line of Motion to another Line of Motion.[147]

Bradley next went on to discuss the first law of motion as expounded in the *Principia,*[148] and defined it as, "Every body perseveres in its state of being at rest or of moving 'uniformly straight forward,' except insofar as it is compelled to change its state by forces impressed."[149] After stating that all bodies possessed no ability to change rest into motion or vice versa, so that a body at rest will remain at rest whilst a body in motion will continue in motion, Bradley asserted that this was all based on the supposition of their being no "external impediments". This, concluded Bradley, implied it could not be proved by experiment, quite simply because no body in the observable universe was ever free of such external impediments.[150] Bradley then ended his discussion of the first law by introducing the central ideas of the second law. Bradley is reported by Heber to have stated that

> The Change that happens in the Motion of a Moving Body, is proportionally to the Quantity of new force impressed, and this force tends to Urge the Body towards that right line in which it acts upon the Body.[151]

This paraphrase of the second law of motion was used by Bradley as an introduction to a discussion of the parallelogram of forces that Newton discussed as the first corollary of the third law of motion.[152] In Heber's notebook vector motion is illustrated by the motions of billiard balls before and after contact on a flat billiard table.[153] Indeed, it appears that a practical demonstration of this was given, though whether by Bradley or an assistant is not stated.

With reference to Heber's notebook and to Bradley's outline[154] he concluded the second lecture with a discussion of the nature of the circular motion of bodies.[155] This led to a discussion of the second proposition and the second theorem of the first book of the *Principia.*[156] Circular motion is also expressed as the product of two motions, an application of the parallelogram of forces, one rectilinear and tangential to the centre of motion, and one directed to the centre of the motion. Bradley attempted to demonstrate this experimentally. Heber reported,

> for two Impulses Acting together and at the same time, as it hath been proved, make the Body move in the middle path in a Curved Line. This is

shewn experimentally by a Ball being whirled round a round Table where the vis inertio keeps it some time from motion. But motion being at last communicated to the Ball, the Ball keeps its Motion for some time after the Table is stopped. The Ball being tied with a string to the Center of the Table.[157]

Throughout his lectures to the student body Bradley's lectures appear to have been illustrated by experiments, performed in conjunction with the lecture itself. Few of Bradley's lectures in experimental philosophy were formally written out, often being in sketch form, an outline which could be used to construct and present the lecture. Some of the experiments were direct demonstrations, and some were analogies. Indeed some appear to have been "thought experiments". It is very difficult to be sure of the exact nature of the practical demonstrations that Bradley used during his lectures but as they were presented during the lecture there is every reason to believe that he probably used an assistant who performed an experiment as Bradley discussed the full implications of what was being observed. Thus when discussing the parallelogram of forces it is possible to perceive that Bradley used collisions between billiard balls on what must have been a small, easily transportable table that was sufficiently large to show the effects before an audience of some fifty or sixty.

**The Extent of the Lectures in Experimental Philosophy.**

I have hitherto briefly examined the content and the presentation of Bradley's first two lectures, which largely discussed the nature of matter and introduced the laws of motion, but it is not possible within the confines of this thesis to examine his other lectures with the same degree of detail. It is certainly possible to suggest that Bradley's lectures in experimental philosophy could sustain much future research. Nevertheless, there is no reason why the outline of Bradley's programme of lectures shouldn't be examined in order to evaluate their extent. After dealing with the nature of matter and the laws of motion the discussion in the third lecture began to deal with the action of fluids, arguing from the content of the second book of Newton's *Principia*, before discussing the

concepts of attraction, repulsion, and cohesion and then moving onto magnetism and electricity. Bradley applied much of the reasoning he used in his discussion of gravity and the motion of bodies. With respect to the phenomena of electricity and magnetism, which he distinguished from gravity, he wrote,

> The foregoing experiments [are] sufficient to prove that there are such Laws as Attraction and etc., and if the Effects are in the same circumstances always produced in the same manner, but the causes not known, we do not reason about occult Qualities. The difference between our present method and that which ascribes particular occult Qualities, is that we by experiment find some general cause that is certain though by us inexplicable, yet because tis evident we may properly be said to know the cause of an effect when we have traced its dependence on some known law of nature or the motion of Bodies.[158]

This distinctive Newtonian methodology of reducing general natural phenomena through a process of observation and mathematical analysis, to "laws of nature" which may enable the observer to make mathematically reducible projections, as opposed to seeking causal explanations for occult or inexplicable processes, was characteristic of the practice of Bradley's mature astronomical work, and is reflected in his experimental philosophy and his lectures at Oxford. Some of Bradley's importance to historians lies precisely in the characterisation of his work as one of the earliest examples of a thoroughly worked out Newtonian practice.

Bradley's next subject is the science of "mechanicks". In Heber's notebook Bradley stated,

> In general the Application of Geometrical Reasoning concerning the motion of bodies and makes up that mixed part of mathematics which is called Mechanicks. The name seems to confine it to the Effects of Powers and Motion produced by machines.[159]

Bradley's lectures then discussed simple and compound machines. An interesting discussion involved what he called "perfect machines", in which the effects of "matter" are ignored and the behaviour of machines is first discussed as a branch

of pure geometry.[160] The philosophical implications of this discussion demonstrated just how far experimental philosophy under the auspices of a Newtonian paradigm was connected to an increased mathematisation of nature. The contending forces were applied in theoretical terms and the nature of the machine was explained in geometrical terms of reference, particularly in the discussion of the equilibrium of forces. Although much of the analysis is presented in Bradley's notebooks, Heber's memorandum makes explicit the lines of Bradley's presentation and their implications.

The final discussion of the principles of mechanics in Bradley's lectures is an examination of how even the smallest body in motion or force can give motion to the most massive bodies. The investigation then moves onto an extended study of the lever. The discussion of the lever is preceded by a consideration of the concept of weight, both in the sense of lifting and carrying, and in a theoretical mathematical sense.[161] At the end of the presentation of the theory of the lever Bradley, in an aside, discussed the practical problems of the requirements necessary to make scales exact. In itself it was not of the greatest importance, but it does throw light on Bradley's appreciation of the principles that underlay the suspension of the zenith sector he was then using in his continued observations at Wanstead.[162] The principle of the lever then led into an examination of inclined planes, prior to dealing with the mechanical principles of the wedge and the screw, conceived as an inclined plane around a cylinder. By this point of the course all five simple machines would have been presented to the students, the lever, the wheel and axis, the wedge, the pulley and the screw, and revealed how they can transform various forms of energy or power into mechanical energy or power. Although not sufficient, the connections between a mastery of the principles of Newtonian mechanics and their applications was a prerequisite of the later industrial revolution, when these principles enabled projectors and entrepreneurs to apply various sources of energy to new methods of production. As Larry Stewart states, "in the natural philosophy of Newtonians like Desaguliers, there was a residual Baconianism hidden and submerged by the contemporary reputation of Newton himself".[163]

As I have already stated many of Bradley's lectures were interspersed with practical applications and with experiments that often illustrated these applications. Thus there was an examination of the strongest beams and joists obtainable with the least amount of wood. This is precisely the sort of problem that any prospective projector might consider in the practical consideration of the economic factors involved with an investment in some new scheme. In Heber's memorandum it is stated that,

> A beam cut out of a given tree will be strongest with the least Timber when the Sides are to one another as $\sqrt{}$ 2/e : $\sqrt{}$ τ/e on the longest side = d$\sqrt{}$ 2/3 when d = the diameter. That is, the sides are to one another as 5 to 7 or rather 12 to 17.[164]

The symbol τ appears to be a variable signifying different parameters. Building on the introduction to simple machines there followed a section on how the force of compound machines can be estimated. The purpose of this section of the course was to contrast machines as mathematical constructs with machines in practice.

The sixth lecture of the course *On Friction* underlines this shift from the theoretical to the practical. Heber stated that,

> Machines in fact never answer what they ought to do in Theory. This drag back arises from the Friction of the parts of the Machine. Friction proceeds from the Roughness of the Surfaces which rub upon Each other in the Machine, and is generally equal to one third of the weight, whence the power must be increased to a force beyond what is sufficient in theory. The force requir'd to draw a Body along an Horizontal plane is wholly owing to Friction, without which the least force imaginable would draw it along as we see in a hanging body.[165]

The course of lectures at this point were illustrated by recourse to various simple practical experiments. These included experiments on friction with blocks of wood and various rollers, some experiments of the different effects of equal power variously applied and experiments of two or more power sources. For instance the motion of carriages on different surfaces was illustrated by the use of wheelbarrows. Bradley had recourse to timbers, ropes, twisted ropes, crossbows,

laces and fibres as well as glass, all to illustrate the different effects of friction of different surfaces and different agents.

The next lecture, the seventh, O*n Accelerated and Retarded Motion*, was initially related to the earlier discussion of the second law of motion.[166] Bradley was reported by Heber to state that "The Motion of a Body is said to be Uniformly accelerated when a fresh impulse is impress'd Upon it in every Instant of time."[167] Bradley therefore concluded that a body's velocity increased proportionately to the time in which it is moving, as for instance under the force of gravity. The implication of this statement if it was faithfully recorded by Heber was that Bradley's was an impulse model of gravity rather than a continuous force model. Various mathematical analyses are offered of the rate of fall of a body through time as it accelerates constantly, in which the analysis is dealt with incrementally so it may be that Bradley was inferring nothing more than this in his discussion. The topic is then developed as an introduction to the discussion of the retardation of motion. Again, according to Heber, Bradley stated that

> A heavy Body when thrown perpendicularly upwards (sic) ascends to such a height that when it has fallen back again to the Point from whence it was thrown, it will have acquir'd a Velocity equal to that with which it was thrown.[168]

The lecture continued with an extrapolation of this topic with an extended discussion of the oscillation of pendulums, an expansion of the examination of *constantly* accelerated and retarded motion.[169] At the time when Bradley developed this part of his course he was working on the isochronal pendulum experiment with Graham and Campbell and the lecture examined a few of the considerations met with during that work. Various experiments were presented and discussed. The method of determining the length of a pendulum is demonstrated as is the manner by which the centre of oscillation is determined. Bradley presented the problems associated with the coefficient of expansion of different metals and how these affected the oscillations of a pendulum as the metal expanded or contracted with changing temperatures. He revealed that the alteration due to expansion and contraction between summer and winter in

England for a pendulum of 30 inches is about a fortieth part of an inch. Next he then discussed how clocks slowed as they approached the equator.[170] All of these problems were very much at the centre of his attention when he first developed his programme of lectures during the early 1730s, although by the time Heber recorded this lecture Bradley had already published the paper on the nutation, for which he was awarded the Copley Medal in 1748.

The next lecture examined "the general motion of the heavenly bodies".[171] Beginning with a brief examination of Kepler's second law of planetary motion with planets sweeping out equal areas in equal times[172] Bradley stated,

> Hence Sir Isaac Newton demonstrates that if a Body be acted upon by a force tending to a point or a centre, and the force decreases as the square of the Distance of the Body increases, in that case the Body must describe one of the conic sections and the centre of the force will be the focus of the section.[173] Now there is but one Conic Section which returns into itself which is the Elipsis (under which the Circle is a particular form)[174]

Bradley stated that the primary planets do in fact describe ellipses about the Sun whose foci are in the Sun's centre,[175] and consequently they are acted upon by a force tending to the Sun's centre which decreases as the squares of the distance of the planets from the Sun. He does not conjecture about the nature of this force and in the lecture he is reported by Heber to have stated,

> There is therefore a known power in nature sufficient to retain all the several Heavenly bodies in their respective orbites we ought therefore conclude that they are retained by this power till we meet with Phenomena that contradict these deductions.[176]

The avoidance of the propagation of occult causes to "explain" natural phenomena is one of the "lietmotifs" of Newtonian natural philosophy, and it is entirely characteristic of a practitioner of Newtonian experimental philosophy to assert that the search for causes should be set aside in favour of determining the behaviour of bodies and forces in space, described analytically and in mathematical terms.

**The "Newtonianism" of Bradley's Scientific Practice and Lectures.**

When Bradley's opening lectures are examined the immediate impression given by them is of their thoroughgoing commitment to Newtonian concepts and procedures as presented in the *Principia*. There is yet little attention paid to the Queries of the *Opticks*. Yet equally, the second impression is a distaste for hypothesising in what he perceived to be the "Cartesian" approach together with a respect for the work of Newton.[177] There is a comparative absence of philosophical "axe-grinding" or of any concerted attempt to court controversy. There is positive evidence in favour of the view that Bradley would do almost anything to avoid overt or unnecessary controversy, as my account of the construction of his aberration paper attests. At this point Bradley was asserting his Newtonian credentials in a community in which such connections were an important key to advancement.

It may be justifiably asserted that Bradley never really possessed an alternative. I think the evidence suggested by his practice reveals that his commitment was a conscious decision, for it was a central support for his astronomical practice. When he wrote the paper on the "new discovered motion", the phenomenon later described as "the aberration of light", he surely became aware that Molyneux's commitment to Newton's "system" was at best questionable. In producing his paper Bradley was forced to consider carefully where his interests lay. His commitment to Newtonian method was motivated in part by his acceptance of an acquired and productive practice developed under the tutelage of Pound and Halley, and also by his clear recognition that his advancement within the natural philosophical community centred on his membership of the Royal Society which would be facilitated by his support of the Newtonian "cause". I do not believe he was acting in an entirely mercenary manner, for as with all men his motives were mixed. Bradley also capitalised on his Whig connections in society, and he gained increasing approbation from his personal associations with the Newton circle. As with his Whig associations Bradley recognised that his Newtonian credentials were not universally admired for even in England there were strains of thought that were opposed to the

Newtonian synthesis. Bradley's own association with Molyneux surely brought to his attention the opposition created by an antipathy directed to what were perceived as the atheistic propensities of Newtonian philosophy.

One such movement that was close at hand in Oxford was propagated by the followers of John Hutchinson, which became an important focus of resistance to Newtonianism amongst High Churchmen and many Tories.

"Hutchinsonianism" developed into a notable school in 18th century Anglican thought.[178] Hutchinson published *Moses' Principia* in 1724[179] asserting that the Hebrew scriptures projected a sound natural philosophy which was opposed not just to Newton's system of universal gravitation but also to the whole heliocentric cosmology. What is so interesting about this opposition to the otherwise overwhelming ascendancy of Newtonian thought in England is the way that Hutchinson's writings focussed attention on the misgivings that many held about the whole thrust and tenor of contemporary natural philosophy. It brought about a rapprochement between many High Church Anglicans and certain elements in the more evangelical and non-conformist confessions of the faith. John Wesley the founder of the Methodist movement within the Church of England confessed that the "new ideas" tended the believer "toward infidelity". Amongst the polite such misgivings must have been experienced but the approbation accorded to the rise of Newtonian ideas ensured that such movements remained muted amongst the leaders of social opinion.

Bradley was a product of a particular milieu and it would be misleading to make overassertive claims on his behalf,[180] but his work reveals a real shift in the culture and practice of science. How far it is possible to state that this shift was typical of his age is difficult to evaluate, for in some respects Bradley was a rarity in the 18th century in that he was a man who earned his living from his science, both in terms of his research and his teaching. He was the inheritor of a tradition that was to form the basis of so much of modern natural science, an approach developed by Newton and his followers, an approach that Bradley was to develop in his practice as an astronomer and a lecturer of experimental philosophy. His significance is lies in the totality of his commitment to Newtonianism, for he was

a Newtonian astronomer and experimental philosopher. Additionally Bradley's work was given high value in his day because it reflected the values of an expansive and acquisitional society which denigrated "enthusiasm"[181] and invested in "reason".[182]

Bradley's work appears "modern" because his practice shares so many of the values of modern science. It was constructed on extraordinarily precise observational and experimental practice which sought regularities. It was predicated on the presumption that the world was rational and predictable, which enabled him to develop explanatory schemes, theories and hypotheses. But the real mark of Bradley's "modernity"[183] lay in his appreciation of the importance of recording the sources of error in his observations.[184] This characteristic gave Bradley's mature work at Greenwich a permanent worth as later astronomers were able to reduce his observations with ever greater assurance.[185] As his work on the law and the hypothesis of the "new discovered motion" also reveals, he possessed a perceptive appreciation of the distinctions between the determination of natural law and the schemes, theories or hypotheses which may be developed to account for natural law. It was this clearly apprehended distinction, perceptible to anyone who carefully analyses Bradley's papers, which reveals the extent of Bradley's Newtonian commitment and the reasons why he was reticent about entering into philosophical contention. Because alternative theoretical frameworks were capable of "explaining" empirical data, Bradley tended to base any hypothetical conclusions on what was apparently evident in his own practice or what his audiences were ready to accept. As a result his papers tend to possess a modesty that sometimes overshadows his astute understanding of the epistemological and ontological issues.

The precision of Bradley's work, though based on Newton's own style, was developed in the context of a partnership[186] with two particularly outstanding instrument makers, George Graham and John Bird. These close partnerships and his acknowledged debt to them, readily obvious in the opening sentences of his paper on the nutation in 1748, was also indicative of Bradley's other work, for he also trained assistants who were to work to the same rigorous standards as

Bradley himself. In analysing and then transmitting many of these tacit skills to his partners and assistants, Bradley helped to transform the practice of contemporary positional astronomy. Contrary to Halley's scepticism Bradley was able to demonstrate and disseminate the tacit skills that were to enable astronomers to observe to very high standards of accuracy and precision. Bradley perceived of astronomy as a cooperative venture. His work with his assistants at Greenwich, with partners such as Molyneux, Halley and the 2nd Earl of Macclesfield George Parker, as well as John Machin and John Hadley, and his practice of always recording as much data as possible to leave a resource for later astronomers, reveals his insight that his task was greater than that which he alone could achieve. In a sense this reflects the man's natural reticence and modesty, but at a more fundamental level it reveals a strong Baconian commitment, which in its Newtonian guise was to direct so much of his work.

There was an oral tradition which was recorded by Rigaud that Bradley had a reputation of always seeking to explain things to people in terms they could understand without "talking down" to them.[187] There was "apparently" an extraordinary absence of malice in Bradley.[188] Although Hornsby's testimony to Rigaud confirming Bradley's immense kindness and gentility cannot be lightly dismissed, there must be a suspicion that this was a personal trait he consciously and deliberately inculcated because in the milieu within which he worked such characteristics were highly efficacious in achieving his own ends. This "self-fashioning" was very successful for he was able to bring round a sceptical Jacobite like Hearne to the point of receiving his highest acclamation, and when the French astronomer Le Monnier was claiming credit for much of his work on the nutation, and was warned of it by Lacaille;[189] he remained "magnanimous" until he published his paper on the nutation, whereby Le Monnier's reputation was largely destroyed in any case. He evidently believed he had prior claim and that this was widely recognised. Much of his work was paradigmatic of Newtonian scientific practice, and did much to demonstrate its value throughout Europe. This is probably one of the most important aspects of Bradley's scientific practice, for as opposed to many now better known "Newtonians" who argued

vociferously in support of Newton in his various claims and disputes; Bradley demonstrated the value of Newtonian science and allowed his results to speak on their behalf. The award of the Copley Medal in 1748 was a clear recognition of this by his scientific contemporaries.

**An Outline of the Concluding Lectures.**

I will conclude discussion of Bradley's lectures in experimental philosophy in only the broadest terms, for much of the purpose of this brief examination of Bradley's lectures was to demonstrate their connection with his astronomical practice and his commitment to Newtonian natural philosophy. What is notable is that he was not apparently interested in the metaphysical or theological aspects of Newtonian philosophy. There is not a word or phrase in the Bradley archive that so much as mentions God in any meaningful sense,[190] nor is there any discussion of any metaphysical consequences or religious implications of his work. In comparison to the earlier lectures of William Whiston and John Desaguliers his lectures were almost entirely free of controversy. Whiston's lectures acquired audiences attracted by his notoriety.[191] As Larry Stewart has stated, Whiston claimed that his intention was to show that mathematics, reason and philosophy all attested to Revelation.[192] There is not scintilla of evidence to suggest that Bradley held such an opinion, for he recognised the insufficiency of empirical evidence to establish natural theory,[193] let alone a theological or a religious position. Margaret C. Jacob argued that the Boyle Lectures had been used as a platform for the establishment of the design argument of Newtonian natural philosophy,[194] an opinion largely supported by James E. Force when discussing Whiston's Newtonian argument from prophecy.[195] But Bradley rarely, if ever, alludes to such theological conclusions.

Nor it seems was Bradley concerned with the overt popularisation of Newtonian philosophy. He appears to have regarded this method in utilitarian terms, as a means of achieving sound results in the most efficient manner possible. In a sense it marks the growing maturity of his practice, for Bradley did not believe that it required justification. There appears to be a diversion of interest

from his greatest mentor Newton such that Iliffe,[196] believes it justifiable to use the epithet "Bradleian" to describe his practice. In Bradley's work the utilitarian imperative far transcends any lingering interest in any possible concern with any vague religious or philosophical implications. This approach to natural philosophy by such a leading practitioner of "Newtonian method"[197] undoubtedly reinforced the Enlightenment construction of Newton as the apotheosis of reason.

Bradley's lectures were open to the public but with the exception of a few visitors[198] his audience was almost wholly drawn from the membership of the university. In comparison, most of the itinerant lecturers in London and countrywide were more concerned with acquisition of an income by entertaining largely middle class audiences with presentations that often included items that bore little connection to the work of Isaac Newton. Whether presenting such ideas to "learned" or to "popular" audiences these lectures did much to propagate the new ideas to a public often eager for new knowledge. As Larry Stewart states, "public lectures represented the mediation between the experimentalist and his audience, which might grant assent and authority."[199] Figures such as Whiston, Desaguliers and others represented an important constituency in the propagation of the new experimental physics to new audiences, moving from a largely university centred background to an environment where entertainment was as important as content in attracting paying audiences.

The work of lecturers such as Keill, Whiteside, Desaguliers, Whiston and later Benjamin Martin[200] and many others was critical in the development of a general consensus about the validity not merely of the Newtonian and Baconian traditions, but of the very validity of experimental philosophy.[201] As the important studies of Steven Shapin and Simon Schaffer have revealed, the results of experiments are not self evident for they are invariably presented within procedures that needed to have gained the assent of the various audiences to whom the results were directed. As they assert in *Leviathan and the Air Pump*:

> we discussed Boyle's proposal to erect a procedural boundary between speech of matters of fact (as experimentally produced and manifested) and speech of the physical causes of these facts. In this practice one recognised

> that God might produce the same effect by a number of different causes, and one professed the appropriately nescient attitude towards the search for real causes.[202]

Bradley was acutely aware of this incapacity of experimental and observational evidences to account for "real" causes, and like his greatest mentor, he effects a similar causal nescience. In this it is possible to elucidate the continuation of a Galilean tradition in natural philosophy, though the division between mathematical astronomy and natural philosophy can trace its antecedents to the ontological and metaphysical distinctions made by Aristotle. Bradley's observational programme at Wanstead was specifically directed to resolving the parameters of a phenomenon he perceived from the start as universal. His entire experimental procedure at Wanstead was designed for this one singular objective. The hypothesis which presented the discussion of the "causes" of the phenomenon was based almost entirely on acquired practice, on his observations of the Jovian system on behalf of Halley. However, Bradley was assertive in denying that his hypothesis necessarily ruled out the anti-Copernican case, for he clearly recognised that his hypothesis was not evidence, merely conjecture. The wide acceptance of Bradley's hypothesis as evidence of the motion of the Earth was effected because of the process of its acceptance by a wide audience. As Larry Stewart has written, "the refraction of Newton's sunbeams revealed that written reports alone could not guarantee assent. It was not necessarily the experiment that was crucial of itself but the audience at the event or at its replication."[203] Likewise, Bradley's audience was largely predisposed to accept his hypothesis of the aberration of light as "evidence" of the motion of the Earth in its orbit around the Sun.

**The Concluding Lectures of the Course.**

The fifteen folios of the ninth lecture[204] in Bradley's later notebook[205] range from his discussion of the figure of the earth, in which he contrasts the approach chosen by the Cartesians with that adopted by Newton. In his discussion of Cartesian physics Bradley examined the laws of percussion and its effects,

again contrasting it with Newton's general approach to the topic in the second book of the *Principia*. The lecture is concluded with a presentation of the motion of projectiles. Notes for the tenth lecture are written on just six folios,[206] being an introduction to Newton's *Opticks*[207] and the fundamental principles underlying Newton's optical philosophy. There is a demonstration of Newton's experiments with prisms and glasses. This lecture was an introduction to the next two, the eleventh on the principles of catoptricks or reflection,[208] and the twelfth on the principles of dioptricks or refraction.[209] These three lectures are connected in that the subject matter is often self referential, discussing some of the presentations and demonstrations in earlier lectures. There is a discussion of the construction and theory of microscopes and telescopes, both refractors and reflectors. The various problems associated with chromatic and spherical aberration are discussed before discussing the theory of colours.

The next three lectures are very largely an exposition of the second book of the *Principia*. The thirteenth lecture,[210] was on "hydrostaticks or the doctrine of fluids",[211] and this was followed by lecture fourteen,[212] "of solids immers'd in fluids" in which he gave a fairly full disquisition on the purpose and use of the hydrostatic balance.[213] The final lecture of the three, lecture fifteen,[214] was "of the motion of fluids". This was an exposition of the content of the second book of the *Principia*, with an implied criticism of the Cartesian system, building on the two previous lectures. When Bradley wrote his initial notes during the early 1730s the status of Cartesian physics was still being debated in France, though Newtonian models were becoming more influential in Paris and in the Low Countries, where a distinctive Newtonian school developed.

The next four lectures were concerned with an examination of the pneumatic sciences. The sixteenth lecture[215] is an introduction to pneumatics, to the science of the air and of gases. The seventeenth lecture[216] is more practical with various experimental demonstrations. Using pumps and syphons Bradley demonstrated the effect of the weight and spring of the air before discussing and demonstrating the barometer, an instrument that he regarded as an important adjunct to his own astronomical work. This lecture introduced the following, for

the eighteenth lecture, "of the causes of the rising and sinking of the mercury in the barometer"[217] concludes the discussion begun in the seventeenth. The lecture is both an exposition of the air pump and a demonstration of Boyle's Law that at a constant temperature the volume of a gas is inversely proportional to the pressure. The nineteenth and penultimate lecture is an exposition of several experiments using the air pump.[218] The final lecture in the course is really a summary of the course, giving an overview of Newtonian natural and experimental philosophy.[219]

Bradley offered his lectures in experimental philosophy at Oxford, at the Old Ashmolean Museum from 1729 to 1760 when declining health led him to resign the Readership in Experimental Philosophy. There was undoubtedly a record of the students who enrolled on Bradley's course from 1729 to 1745, but unfortunately this does not appear to be extant. The registers containing the record of his students by college from 1746 to 1760 is located at the back of his lectures on the air pump.[220]

**The Award of the Degree of Doctor of Divinity by Diploma.**

After Bradley was appointed as Astronomer Royal on the 3rd of February 1742 his responsibilities had become time consuming and very demanding. As well as giving lectures in astronomy and experimental philosophy at Oxford he was undertaking a busy observational programme. He was undertaking various planetary and stellar observations at Oxford and Shirburne Castle, continuing his observations of nutation at Wanstead and making preparations for the Royal Observatory. Nothing was further from his mind than the award of a degree. The "discoverer" of aberration and nutation, Savilian Professor of Astronomy and celebrated lecturer in experimental philosophy was far too busy to think of anything other than the demands of each of his public or academic offices.

Although Bradley had been associated with the University of Oxford since the 15th of March 1711 when he went up to Balliol College as a commoner and had been the Savilian Professor of Astronomy since the 31st of October 1721, and had lectured in experimental philosophy since the autumn of 1729, he had not obtained any higher academic degree than the MA he acquired on the 21st of June

1717. After he was appointed Astronomer Royal by Sir Robert Walpole in February 1742 and his national reputation had been firmly established, the Convocation of the University received the assent of the Chancellor, the Earl of Arran, to confer the degree of Doctor of Divinity by Diploma. Rigaud informs us that this was a rare distinction for honorary degrees or degrees by diploma to be offered to sitting members of the university,[221] but such was the respect and approbation that Bradley had achieved that Convocation was moved to present him the higher degree as a distinctive recognition of his work for the university as well as the nation.

As I have already suggested when Bradley was elected Savilian Professor of Astronomy largely by a Whig elite based mainly in London, the appointment had been resented by many in the university which still maintained strong Royalist and Tory associations dating from the time of the Civil Wars and the Restoration.[222] Yet within ten years Bradley had acquired the evident friendship and admiration of the non-juror Thomas Hearne, and the approbation of many within the university for the diligent way he undertook his various commitments. His lectures were well attended and admired, such that many casual visitors, including a party of Russian natural philosophers and various French visitors attended them for their content and for the celebrity of the presenter. The award of the degree of Doctor of Divinity was probably accepted gratefully quite simply because it was never sought. The general characterisation of Bradley's personality that was most often voiced was of his evident modesty.

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[1] The site of Elizabeth Pound's house is now occupied by a small supermarket. When the London Borough of Redbridge mounted a plaque to commemorate Bradley's work on aberration it was placed on a nearby council owned property.
[2] Initially with two of Elizabeth's nephews, the sons of her brother Matthew.
[3] When he took up residence at Greenwich after being appointed astronomer royal in 1742.
[4] Bradley continued to take vacations with the Wymondesoldes at their property in Dorset.
[5] Bradley graduated MA from Balliol on 21st June 1717 and renewed his connection with the University of Oxford on 31st October 1721 when he was elected Savilian Professor of Astronomy.
[6] John Keill died 31st August 1721. He had been elected to the professorship in 1712.
[7] Christ Church Library, Oxford, MS 234, *William Wake Letters* 22, Sir Thomas Parker to William Wake, 1st September 1721.
[8] Bodleian Library, *Thomas Hearne Papers*, No. 92, p. 146.
[9] Rigaud, "Memoirs", p. vii.

[10] Sir Thomas Parker had been Lord Chancellor from 1718 and within a few weeks would be raised to the newly created Earldom of Macclesfield. He had been knighted in 1705, and served as the Whig MP for Derby during the same year. In 1709 he had been on the committee which drew up the articles of impeachment against Dr. Henry Sacheverell. He was an FRS, being elected in 1713, and filled several offices being one of George I's favourites.

[11] Rigaud, "Memoirs", p. vii.

[12] Bodleian Library, Bradley MS. 23, *James Pound's Account Book 1715 to 1724, with some receipts and bills.* fol. 72, reads, "1721, Sept. 2. By coach hire, pocket expenses &c. about the Oxford professorship, £2 12s 0d. Oct. 31. By cousin Bradley lent him £4 4s 0d. By pocket expenses in London, &c. £0 11s 3d. On fol. 73v it is possible to discover that the four guineas lent to James Bradley by his uncle was "to give to the door-keepers of the house of lords". Thus on the day of his election James Bradley, accompanied by Pound, attended both Wake and Parker in the House of Lords.

[13] The Rev. John Whiteside of Christ Church, who had been a candidate in 1712, and was a lecturer in experimental philosophy, had as strong a claim as anyone, particularly as he possessed many powerful connections within the University of Oxford. Bradley was a graduate of Balliol, but he was largely anonymous.

[14] Bradley's changing reputation at Oxford can be no better illustrated than by reference to the Thomas Hearne papers. At first he is resented and dismissed, but over the years he achieves acceptance and finally admiration. Although his career was advanced by patronage, he also possessed the scientific and the social skills which led to an admiration of his person and achievements.

[15] Rigaud, "Memoirs", p. viii.

[16] Ibid. p. viii.

[17] Bodleian Library, Bradley MS 19, *Latin Lectures delivered at Oxford by James Bradley.* These include Bradley's *Oratio inaugurales* of 26th April, 1722.

[18] Bodleian Library, *Thomas Hearne Papers,* No. 94, p. 61.

[19] J. T. Desaguliers, *A Course of Experimental Philosophy,* Vol. 1, Preface, London, 1734.

[20] Rigaud, "Memoirs", p. xxxvii.

[21] John Keill, *An Introduction to Natural Philosophy: or, Philosophical Lectures Read in the University of Oxford, Anno Dom. 1700*, London, 1720. p.16. Cited in Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660 – 1750.* Cambridge, 1992. p.20 and footnote 66.

[22] Larry Stewart, *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660 – 1750.* Cambridge, 1992. p.20.

[23] At the end of the 17th and the beginning of the 18th centuries the two main groups of refugees who fled to England were the Huguenots from France, and the Palatines from Germany. Both were the victims of Louis XIV's political and religious policies, the former dating from the Revocation of the Edict of Nantes in 1689, the second from the Wars of the Grand Alliance (1689 – 1697) and the War of the Spanish Succession (1701 – 1714). In 1707 a French army under the command of Marshall Villars entered the Palatinate laying waste to many areas and imposing a rule of unprecedented oppression. In 1708 an Act of Parliament was passed for the naturalisation of all foreign Protestants. Transports were laid on to bring the Palatines from Rotterdam and a large tented city of some 13,000 refugees appeared outside the City of London. Although many stayed and settled in England, over half were given entitlement to lands in New England. In 1709 John Keill was appointed as the Treasurer to support those arriving in New York.

[24] Desaguliers, "A Course of Experimental Philosophy", Vol. 1, Preface, London, 1734.

[25] Stewart, "The Rise of Public Science", p. 26.

[26] Rigaud, "Memoirs", p. xxxvii.

[27] The building where Whiteside gave his lectures on experimental philosophy is now the Museum of the History of Science in Broad Street. It is now referred to as the "Old Ashmolean Museum".

[28] Ibid. p. xxxviii.

[29] John Keill, *An Examination of Dr. Burnet's Theory of the Earth, together with some remarks on Mr. Whiston's New Theory of the Earth,* Oxford, 1698, pp. 177-178. See Larry Stewart, *The Rise*

*of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660 – 1750,* p. 69.

[30] Stewart, "The Rise of Public Science", pp. 20 passim.

[31] Rigaud, "Memoirs", p. vii.

[32] Graham Midgley, *University Life in Eighteenth-Century Oxford,* Yale University Press, London, 1996.

[33] This "methodology" was often so diverse as to be almost impossible to identify.

[34] Thomas Birch, British Library, Add. MSS. 4478D, *Dr Birch's Common Place Book*, f. 107r. Quoted in Stewart, ibid. p.21.

[35] Johann Bernoulli to Maupertuis, 9th August, 1733. This is quoted in Terrall's "The Man Who Flattened the Earth: Maupertuis and the Sciences in the Enlightenment". p.68 and footnote 123.

[36] Bradley's income from the professorship is difficult to evaluate because only the income of the year 1724 is available when Bradley appraised his financial position after the death intestate of his uncle. It appears that his net annual income after the payment of various statutory fees and charges amounted to little over £200, rather less than the £300 his lifestyle as a gentleman normally demanded at this time. See Bodleian Library, Bradley MS 18, *Notes by James Bradley about annuities, interest and income from professorship in 1724.*

[37] Bodleian Library, Bradley MS 21, *A book of observations made at Wanstead by James Pound and James Bradley.*

[38] Bodleian Library, Bradley MS 19, *Latin Lectures delivered at Oxford by James Bradley including his "Oratio inaugurales", 26th April, 1722.*

[39] Bradley, "A letter to Dr. Edmond Halley, giving an account of a New Discovered Motion in the Fixed Stars, in the Miscellaneous Works and Correspondence of James Bradley", pp. 1-16. It was read at meetings of the Royal Society on the 9th and 16th January 1728/29 OS.

[40] Bodleian Library, Bradley MS 18, *Notes by James Bradley about annuities, interest and income from professorship in 1724,* fols. 1 – 2. Bradley's annual income was under £200 in 1724. Over the next few years this increased slightly, but the estimate of the requirement in order to maintain all of the appurtenances of a gentleman was approximately £300 per annum. The zenith sector may have come from savings but there is every likelihood that Elizabeth Pound gave some degree of financial support during the difficult years from 1724 to 1729.

[41] Bradley took possession of a house in New College Lane with the Savilian professorship. He lived here when he visited Oxford, but his duties as Savilian Professor of Astronomy were not onerous, and it is possible to determine from his observation books that Bradley spent most of his time in Wanstead.

[42] Bradley's paper had been read on 9th and 16th January 1729.

[43] Bodleian Library, *Thomas Hearne Papers,* No. 124, p. 121, 24th January, 1729.

[44] Ibid, No. 125, p. 9, 13th February, 1729.

[45] Rigaud, "Memoirs", p. xxxviii.

[46] Bodleian Library, *Thomas Hearne Papers,* No. 124, p122, 24th January, 1729. The lectureship in experimental philosophy was not a permanent foundation, or a part of the curriculum, so the lecturers were expected to raise income by charging fees. After Bradley's rise to prominence after the publication of the paper on the aberration of light it appears that he attracted greater numbers of students than Whiteside. Unfortunately this must remain conjectural for the financial records for these lectures are missing from 1729 to 1745 inclusive. If the numbers registering from 1746 are any guide then it would appear that Bradley must have discovered a lucrative source of income.

[47] It suggests that Bradley and Whiteside had come to an earlier agreement which Bradley was able to act on as soon as his obligations allowed.

[48] Rigaud, "Memoirs", p. xxxviii.

[49] It is just possible that his heirs retained them for they may have contained sensitive information. Virtually nothing of a personal nature survives in the Bradley archive, undoubtedly because every letter or document containing personal or family matters was removed by the Peach family who acted as his executors during the period of the law suit between the Admiralty and themselves between 1762 and 1776.

[50] There is a record of the attendance at James Bradley's courses of lectures in experimental philosophy from the spring of 1746 until the spring of 1760 when ill health led to their termination.
[51] J.L. Heilbron, *Elements of Early Modern Physics,* University of California Press, 1982. pp. 144 – 149.
[52] Ibid. p. xxxviii.
[53] Ibid. p. xxxviii.
[54] Bodleian Library, *Thomas Hearne Papers,* No. 94, p. 61.
[55] This is Hearne's own way of stating that he supported Bradley's claim. In fact he was the only one of the electors for this office who voted in Bradley's favour. The Keeper of the Ashmolean Museum was traditionally an antiquarian and it was Joseph Andrews, fellow of Magdalen College who was elected to this office.
[56] Bodleian Library, *Thomas Hearne Papers,* No. 129, p. 138.
[57] Midgley, "University Life in Eighteenth-Century Oxford", Yale University Press, London, 1996.
[58] According to Rigaud Bradley was largely "abstemious" for much of his adult life, although as Allan Chapman once remarked to me, he appears from his portrait at the Ashmolean Museum to have been a man who very much enjoyed the pleasures of the table.
[59] Rigaud, "Memoirs", p. civ.
[60] There are no extant records of Bradley's lectures from 1729 to 1746. The pattern of his courses from 1746 to 1749, the last year he offered three courses a year was that one course (dependant on the date of Easter and Whitsun) began in March, another in May and another in November. From 1750 he offered two courses a year, starting in March and May.
[61] Bradley always attempted to attend to his observations of nutation at Wanstead at times that did not impinge on the convenience of Mrs Williams. Bradley maintained his observations for 15 years after leaving Wanstead from 1732 to 1747.
[62] Bradley maintained close and friendly relations with the Wymondesolde family after the death of Elizabeth Pound on 10th September 1740.
[63] Bradley, "Miscellaneous Astronomical Observations", found in "Miscellaneous Works", p. 365.
[64] Rigaud, "Memoirs", pp. xxxix.
[65] Bradley, "Miscellaneous Astronomical Observations", found in "Miscellaneous Works", p. 365.
[66] The construction of the observatory at Shirburne, alongside a laboratory, enabled Bradley to comprehend the problems of the Royal Observatory in 1742. The experience of building the observatory gave Bradley the insights he needed when he planned and constructed the new observatory at Greenwich, the building called the Transit House.
[67] John Hadley, "The Description of a new Instrument for taking Angles. Communicated to the Society on May 13, 1731. *Philosophical Transactions*, Vol. 37, pp. 147 – 157.
[68] Rigaud, "*Memoirs*" p. xl.
[69] Newton, "A true Copy of a Paper found, in the Handwriting of Sir Isaac Newton, among the Papers of the late Dr. Halley, containing a Description of an Instrument for observing the Moon's Distance from the Fixt. Stars at sea. Read at a Meeting of the Royal Society October 28, 1742. *Philosophical Transactions,* Vol. 42, pp. 155 – 156.
[70] Hadley, "An Account of Observations made on Board the Chatham-Yacht, August 30th and 31st, and September 1st , 1732, in pursuance of an Order made by the Right Honourable the Lords Commissioners of the Admiralty, for the Trial of an Instrument for taking Angles, described in Philosophical Transactions, Number 420, by John Hadley, Esq; Vice-President of the Royal Society." *Philosophical Transactions,* Vol. 37, pp. 341 – 352.
[71] Bradley to Hadley date unknown. Bradley stated that his first series of observations began on the 1st September, 1732.
[72] Bradley to Hadley, date unknown but probably 1732, "Miscellaneous Works", p505.
[73] See two letters addressed to the Secretary of the Admiralty by Bradley on the 10th of February, 1756, "Miscellaneous Works", pp. 84 – 85, and the 14th of April, 1760, ibid. pp. 86 – 89, testifying to the extraordinary accuracy of Mayer's tables. These tables were adapted for use by mariners by Nevil Maskelyne the fifth Astronomer Royal and published annually from 1766 as the *Nautical Almanac.*

[74] Rigaud, "Memoirs", p. lxxxiii.
[75] There is little or no direct documentary evidence for this but a perusal of Bradley's notebooks and papers suggests several visits to Shirburne Castle throughout 1738.
[76] Ibid. p. lxxxiii.
[77] Ibid. p. lxxxiii.
[78] Ibid. p. lxix. footnote y.
[79] Ibid. p. lxxxiii.
[80] Bodleian Library, Bradley MS. 31, *James Bradley's Observations at Greenwich with two Transit Instruments from 25th July, 1742 to January 1742/43. At folio 38, Lord Macclesfield's Similar Observations at Shirburne, September to December 1742, and March 1743.* fol. 39r.
[81] Rigaud, "Memoirs", p. lxxxiii.
[82] Ibid. p. lxxxiii.
[83] George Parker, *Shirburne Observation Book,* June 1745. Quoted in Rigaud, "Memoirs", p. lxxxiii.
[84] Ibid. p. lxxxiii.
[85] At this time the word "curious" meant "precisely accurate".
[86] Bradley in Greenwich to De l'Isle in Paris, 22nd August 1752. OS. In the "Miscellaneous Works", p. 478.
[87] Bradley, "A letter to George, Earl of Macclesfield, concerning an apparent motion observed in some of the fixed stars, in the Miscellaneous Works and Correspondence of James Bradley", p. 19 and p. 41.
[88] This was John Bradley, James Bradley's first assistant at Greenwich.
[89] Bodleian Library, Bradley MS. 44, *Correspondence to James Bradley and his assistants,* fol. 140r, Bliss to Macclesfield, 19th February 1743/44 OS.
[90] Bradley, "Miscellaneous Astronomical Observations" in the "Miscellaneous Works", p. 365.
[91] The timings here appear to be in civil time, with the day beginning at midnight. Most of the timings in Bradley's observation books are recorded in astronomical time with the day beginning at noon.
[92] Geoff Armitage, *The shadow of the moon: British solar eclipse mapping of the eighteenth century,* Tring, 1997. See Appendix 2, p.44. Against the remarkable sequence of three total and two annular solar eclipses from 1715 to 1764, must be contrasted the sequence of three from then to the end of the millennium, the 15th March 1858, the 29th June 1927 and the 11th August, 1999.
[93] Bradley, "Miscellaneous Astronomical Observations" in the "Miscellaneous Works", p. 367.
[94] Ibid. pp. 367 – 368.
[95] Ibid. p. 365.
[96] Ibid. p. 365.
[97] Ibid. p. 367.
[98] Ibid. p. 367.
[99] Bradley, "Observations upon the Comet that appeared in the months of January, February, and March, 1737, made at Oxford", contained in "Miscellaneous Works", p. 49.
[100] Ibid. p. 51.
[101] Ibid. p. 52.
[102] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. Book III, Proposition 14, Theorem 14. London, 1726. Translated by I Bernard Cohen and Anne Whitman, London, 1999, p898.
[103] Bradley, "Observations upon the Comet that appeared in the months of January, February, and March, 1737, made at Oxford", contained in "Miscellaneous Works", p. 52.
[104] Ibid. p. 52.
[105] Rigaud, "Memoirs", p. xliv.
[106] Delambre, "Histoire de l'Astronomie au xviii siecle", p.419 Quoted by Rigaud in the "Memoirs", p. lxiv.
[107] Bradley, "Observations upon the Comet that appeared in the months of January, February, and March, 1737, made at Oxford", contained in "Miscellaneous Works", p. 52, and Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. Book III, Proposition 14, Theorem 14. London, 1726. Translated by I Bernard Cohen and Anne Whitman, London, 1999, p898.

[108] Rigaud, "Memoirs". p. xliv.
[109] Bradley paid regular visits to Shirburne Castle, the residence of George Parker, the 2nd Earl of Macclesfield. In the late 1730's Bradley helped his friend and patron develop the finest observatory in England.
[110] Bodleian Library, Bradley MS. 1, *Bradley's notebook for lectures at Oxford* and Bradley MSS. 3 to 13, *Lectures delivered by James Bradley at Oxford as Savilian Professor of Astronomy and as Lecturer in Experimental Philosophy.*
[111] Bodleian Library, Bradley MS.1, *Bradley's notebook for lectures at Oxford.* There is a note in this source written in 1893 which estimated that it was written between 1740 and 1750. Comparison of Bradley's penmanship, paper and ink, with sources written during the early 1730s and those definitely written in the mid 1740s strongly suggests that Bradley's notebook was written early in the 1730s and may even date from the time he acquired the post of lecturer in experimental philosophy late in 1729. In addition, the text is almost the working out of a course in rough form and not a coherent presentation as would be expected if it had been written after the course had been presented for ten years and for over thirty presentations.
[112] Bradley crossed out the word "full" before he wrote "course".
[113] The word "Qualities" was crossed out and replaced with "Properties".
[114] Bodleian Library, Bradley MS. 1, *Bradley's Notebook for Lectures at Oxford.* fol. 1r.
[115] Stewart, "*The Rise of Public Science*". p. xxxii passim.
[116] Ibid. p. xxxii.
[117] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. *Memorandums of Doctor Bradley's Course of Experimental Philosophy.* [by R. Heber]There is a pencilled note on the manuscript to show that the manuscript had been purchased at auction. It states "Phillips sale. June 1893. Lot 268."
[118] Bodleian Library, Bradley MS 3, from the back cover, a record of student registrations for courses in experimental philosophy by college.
[119] Whereas his predecessor John Whiteside expounded a formal lecture before retiring with his students to an ante-room where experiments were then performed by a servant, it appears that James Bradley merely used rough notes upon which he expounded freely and performed experiments which immediately illustrated his lecture as he proceeded. In some cases he may have required assistance. Whether he used an assistant, or whether he asked those in attendance is not possible to determine beyond doubt. He probably used an assistant but there is no record of such.
[120] Bradley gave two courses of lectures in 1754. It is not possible to determine which course R. Heber attended, one began in March and the other in May.
[121] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. fol. 1r.
[122] Roger Cotes, *Philosophiæ Naturalis Principia Mathematica,* 2nd ed, Editor's Preface, London, 1713. In the Cohen and Whitman translation, London, 1999, p. 397. Cotes wrote, "Surely, this world – so beautifully diversified in its forms and motions – could not have arisen except from the perfectly free will of God, who provides and governs all things."
[123] William Derham, *Physico-Theology (The Boyle Lectures),* London, 1713 and *Astro-Theology: Or a Demonstration of the Being and Attributes of God, from a Survey of the Heavens*, London, 1714, are both based on a voluntaristic theology common amongst Newton's followers.
[124] Terrall, "*The Man Who Flattened the Earth*" London, 2002.
[125] René Descartes, *Le Monde* contained in Adams and Tannery's complete edition of Descartes works, Vol. I, p. 90, note a. Quoted in Daniel Garber, *Descartes' Metaphysical Physics,* London, 1992. p.198.
[126] Nicolas Malebranche, *De la recherché de la verité*. 3 vols. Paris, 1674 – 75. Many of his central concepts were clarified later in his *Traité de la nature et de la grace*, Paris, 1680.
[127] Daisie Radner, *Malebranche: A Study of a Cartesian System,* London, 1978.
[128] It is a method very much demonstrated in Bradley's own work, which can sometimes be accepted as a paradigmatic example of Newtonian science during the first generation after the death of Newton.
[129] Bradley to Graham, c. December 1737, in Bodleian Library, Bradley MS 45, *Correspondence to James Bradley and his assistants.*

[130] Bradley is extremely critical of Maupertuis' methodology when using the zenith sector constructed by Graham for the Lapland Expedition. It is easy to sense Bradley's dismay that a thoroughly rigorous approach to observation and measurement was not adopted in what he appears to sense was an eagerness to prove a philosophical point in what he already recognised was sometimes a rather acrimonious dispute in the Paris Academy of Sciences. Bradley discussed with Graham in the letter sent to him, probably in December 1737, the method that Maupertuis and his colleagues should rightfully have adopted to ensure valid and verifiable results. Bradley's earlier experience with the impetuosity of his erstwhile friend and partner Molyneux did not easily dispose him to overlook such shortcomings, even though he kept his opinion between Graham and himself.

[131] Bradley was sufficiently astute to recognise that such demonstrations were ever "self-evident". His lectures were of course presented within the context of the Newtonian polemical discourse that ruled English experimental philosophy, so that the term was synonymous with the epithet "Newtonian".

[132] Fontenelle arguably published the final fully committed Cartesian text in France in 1752 in his "Théorie des Tourbillons", published by the Imprimerie Royale at Paris.

[133] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. London, 1726. James Bradley undertook work on Newton's behalf, in Book 3, Proposition 42, and Newton presented a specially bound copy of the 3rd edition bound in red Moroccan leather to Bradley.

[134] Even Bradley, as his search for the causes of aberration and nutation illustrates, wasn't just concerned with the determination of the mathematical parameters or the laws of those phenomena.

[135] The Axioms or the Laws of Motion are contained in the preface to the *Principia.* Law 1 states "Every body perseveres in its state of being at rest or of moving uniformly straight forward, except insofar as it is compelled to change its state by forces impressed." Law 2 asserts "A change in motion is proportional to the motive force impressed and takes place along the straight line in which that force is impressed." Law 3 states, "To any action there is always an opposite and equal reaction; in other words, the actions of two bodies upon each other are always equal and always opposite in direction." Translation by I. Bernard Cohen and Anne Whitman.

[136] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 3r.

[137] Ibid. fol. 3v

[138] Ibid. fol. 3v

[139] Ibid. fol. 4r

[140] In the *Meditations*, the *Principles of Philosophy* and *Le Monde*, passim.

[141] Bodleian Library, Bradley MS.1, *Bradley's notebook for lectures at Oxford.* fol. 5r.

[142] Daniel Garber, *Descartes' Metaphysical Physics*, University of Chicago Press, 1992. pp. 64 – 65.

[143] R. Heber, Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 5r – fol. 5v

[144] Ibid. fol. 5v

[145] Ibid. fol. 5v – 6r

[146] Ibid. fol. 6r

[147] Ibid. fol. 6r

[148] Newton, "Philosophiæ Naturalis Principia Mathematica", Axioms or Laws of Motion, 3rd ed. London, 1726. Translated by I. Bernard Cohen and Anne Whitman as *The Principia: Mathematical Principles of Natural Philosophy*, London, 1999, pp. 416 - 430.

[149] Ibid. p. 416.

[150] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 9r.

[151] Ibid. fol. 9r.

[152] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. London, 1726. Axioms or Laws of Motion, Law 3, Corollary 1, "A body acted on by [two] forces acting jointly describes the diagonal of a parallelogram in the same time in which it would describe the sides if the forces were acting separately." Translated by I. Bernard Cohen and Anne Whitman, as *The Principia: Mathematical Principles of Natural Philosophy*, London, 1999, p. 417.

[153] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 10r.
[154] It is interesting to note that the content of Bradley's lectures as evidenced by notebooks written during the early 1730s and Heber's memorandum of 1754 remained virtually unchanged.
[155] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 11v.
[156] Newton, "Philosophiæ Naturalis Principia Mathematica", 3rd ed. London, 1726. Book 1, Proposition 2. Theorem 2. "Every body that moves in some curved line described in a plane and, by a radius drawn to a point, either unmoving or moving uniformly forward with a rectilinear motion, describes areas around that point proportional to the times, is urged by a centripetal force tending to that same point." Translated by I. Bernard Cohen and Anne Whitman, London, 1999, p. 446.
[157] R. Heber, Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 11v.
[158] Bodleian Library, Bradley MS.1, *Bradley's notebook for lectures at Oxford*, fol. 21r
[159] Ibid. fol. 27r
[160] Ibid. fols. 27r – 30v.
[161] Ibid. fol. 29r
[162] The practical skill in the accurate suspension of scales (or a suspended telescope) involves the overcoming of five problems. These are firstly, that the points of suspension and the centre point should be in the same line, secondly, that weights are equidistant, thirdly, that the weight of the brachia or the arms be equal, fourthly, that the centre of gravity should be a little below the centre of motion and fifthly, that there should be a fine edge to avoid friction.
[163] Stewart, "The Rise of Public Science", Cambridge, 1992. p. 213.
[164] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 27r.
[165] Ibid. fols. 32r & 33r.
[166] The second law of motion as translated by I. Bernard Cohen and Anne Whitman, "The Principia", London, 1999, states, "A change in motion is proportional to the motive force impressed and takes place along the straight line in which that force is impressed."
[167] Bodleian Library, MS. Eng. Misc. e. 15. Catalogue no. 31516. [Heber] *Memorandums of Doctor Bradley's Course of Experimental Philosophy,* fol. 35r.
[168] Ibid.. fol. 40r
[169] Bodleian Library, Bradley MS.1, *Bradley's notebook for lectures at Oxford*, fol. 35r. There is no apparent reference to anything other than a continuously acting force, so the reference in Heber, fol. 40r may be nothing more than a misapprehension on his behalf.
[170] Ibid. fol. 36r.
[171] Ibid. fol. 36r.
[172] This is also stated in similar terms in the "Principia". Book 1, Proposition 1, Theorem 1, states, "The areas which bodies made to move in orbits describe by radii drawn to an unmoving centre of forces lie in unmoving planes and are proportional to the times." Translated by I. Bernard Cohen and Anne Whitman, London, 1999. p. 444.
[173] See the "Principia". Book 1, Proposition 11, Problem 6. Newton states, "Let a body move in an ellipse; it is required to find the law of the centripetal force tending toward a focus of an ellipse." Translated by I. Bernard Cohen and Anne Whitman, London, 1999. p. 462.
[174] Bodleian Library, Bradley MS.1, *Bradley's notebook for lectures at Oxford*, fol. 37r.
[175] In fact the centre of the orbits of the primary planets is the centre of gravity of the solar system as a whole. As the Sun possesses almost 99.9% of the mass of the system the centre of gravitation is very close to the centre of the Sun.
[176] Bodleian Library, Bradley MS.1, *Bradley's notebook for lectures at Oxford*, fol. 40r.
[177] This is very evident in the first two lectures.
[178] C. D. A. Leighton, "Hutchinsonianism: A Counter-Enlightenment Reform Movement", in *Journal of Religious History,* Vol. 23, Part 2, pp. 168 – 184.
[179] John Hutchinson, *Moses' Principia,* London, 1724.

[180] As briefly referred to in Chapter 1, Freeman J. Dyson in an article in the scientific journal *Nature*, 1st July, 1999, Vol. 400, p. 27, called Bradley "the founder of modern science". particularly in connection with Bradley's commitment to high precision. Yet modern science is as much as any other human activity both cooperative and culturally determined. Science is a social activity even when it is undertaken in isolation for there must be a public and a prior tradition of training which determines the value of that activity. To assert, as Dyson does, that a complex human activity like modern science can have a "founder" fails to acknowledge that recognition involves a sharing of values. Bradley was a man of his age.

[181] Enthusiasm was identified with the religious fanaticism of the previous century and with unbridled passion. It was seen as a threat to the ruling values of polite society. The accusation of "enthusiasm" was intended to denigrate. The rise of the Methodist movement with its appeal to the emotions was particularly resisted by the more "respectable" members of society. Wesley's appeal found fertile ground amongst the poor and the less privileged members of society.

[182] Reason was identified with that which was orderly, rational and lawful. Bradley's work was reasonable because it sought order in a universe that was subject to natural law. The precision and accuracy of Bradley's work was predicated on the acceptance that the world was essentially rational and predictable. These values are the ruling values of modern science, and they are also expressed in the ruling values of educated or polite society in 18th century European society.

[183] I clearly recognise that I am open to the charge of "Whiggishness" at this point, but Bradley's work though very much a product of his own age possesses characteristics that later scientific workers have found significant. This is not idle speculation. The fact remains that Bradley's practice enabled Bessel to reduce his great series of observations at Greenwich from 1750 to 1762. This is the earliest significant body of observations which can be relied upon in terms of accuracy and precision by later astronomers.

[184] When giving lectures and talks on Bradley's work to learned and astronomical societies this aspect of his practice, the recording of sources of possible error, has evoked many to respond with the recognition that this really forms the basis of his apparent "modernity".

[185] As Bessel and Arthur Auwers were able to during the 19th century.

[186] In all of his dealings with George Graham, and later with John Bird, Bradley gives every indication that he regarded these men as his full partners, without whom his work would have been impossible. It might equally be asserted that it was the drive for accuracy and precision in an astronomer like Bradley which motivated these great instrument makers to the very limits of the technology at their disposal. For the problems associated with some aspects of the drive for high precision and accuracy see, Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation." in *Science in Context,* Vol. 2.1 (1988), pp. 115 – 145.

[187] This was attested by Hornsby when he discussed Bradley's character with Rigaud.

[188] Both Hornsby and the Prime Minister, Henry Pelham, who dined occasionally with Bradley, attested to the gentleness and kindliness of his demeanour and character.

[189] Lacaille to Bradley, Paris, 2nd May 1748.

[190] Rigaud, "Memoirs", p. lxxxi. Bradley refused the offer of the living at Greenwich when offered it by Henry Pelham. Bradley stated that the pastoral duties of a priest in the Church of England were sufficiently demanding to preclude any acceptance of it whilst he was Astronomer Royal.

[191] He had succeeded Newton as Lucasian Professor of Mathematics at Cambridge in 1703 but had been banished from Cambridge in 1710 when he openly expressed his Socinian beliefs.

[192] Stewart, "The Rise of Public Science" Cambridge, 1992. p.93.

[193] Bradley, "The Miscellaneous Works and Correspondence of James Bradley", p.15.

[194] Margaret C. Jacob, *The Newtonians and the English Revolution, 1689 – 1720,* Ithaca, Cornell University Press, 1976, p.270.

[195] James E. Force, *William Whiston: Honest Newtonian*, Cambridge University Press, p. 63.

[196] I thank Rob Iliffe for bringing this point to my attention. I also think that Bradley's utilitarianism is an important factor in his social and political success, for it enabled him to avoid unnecessary philosophical or religious controversy enabling him to acquire the approbation of so many diverse interests.

[197] I recognize that the use of the term "Newtonian method" is problematical, for quite aside from the diversity of interests that took possession of the term, it must be recognised that practice

changed in the light of experience and the development of theory. Bradley's own practice was always developing.

[198] Including a party of Russian visitors.

[199] Stewart, "*The Rise of Public Science*". Cambridge, 1992. p.103.

[200] Ibid. pp. 104 – 105.

[201] Benjamin Martin, *The Philosophical Grammar*, 3rd ed. London, 1748. Cited in Stewart, p.105.

[202] Steven Shapin and Simon Schaffer, *Leviathan and the Air Pump: Hobbes, Boyle, and the Experimental Life*. Princeton University Press, 1985, pp. 139 – 140.

[203] Stewart, "The Rise of Public Science", Cambridge, 1992. p.104. See also Simon Schaffer, "Glass Works: Newton's Prisms and the Uses of Experiment", in David Gooding, Trevor Pinch, and Simon Schaffer, eds., *The Uses of Experiment: Studies in the Natural Sciences,* Cambridge University Press, 1989. Chapter II, pp. 92 – 98.

[204] Bodleian Library, Bradley MS.17, *Papers relating to James Bradley's lectures on natural philosophy at Oxford from 1740 to 1760.* fols. 60 – 75.

[205] Although there are some differences between the earlier versions of his lectures and those he wrote during the 1740s, the topics remained largely untouched. A comparison of Bradley's lecture notebooks written during the early 1730s, those written during the 1740s and 1750s, and Heber's notebook of 1754 reveal a surprising continuity.

[206] Ibid. fols. 75 – 81.

[207] Newton, *Opticks*, London, 1704. By the time James Bradley was giving his lectures on experimental philosophy at Oxford the 4th edition of 1730 was commonly in use.

[208] Bodleian Library, Bradley MS.17, *Papers relating to James Bradley's lectures on natural philosophy at Oxford from 1740 to 1760.* fols. 81 – 87.

[209] Ibid. fols. 87 – 108.

[210] Ibid. fols. 108 – 119.

[211] Ibid. fol. 108.

[212] Ibid. fols. 119 – 130.

[213] Ibid. fols. 125 – 128.

[214] Ibid. fols. 130 – 135.

[215] Ibid. fols. 135 – 143.

[216] Ibid. fols. 143 – 154.

[217] Ibid. fols. 154 – 164.

[218] Ibid. fols. 164 – 176.

[219] Ibid. fols. 176 – 183.

[220] Bodleian Library, Bradley MS. 3. *Lectures on "The more known properties of the Air established ..." and notes on members attending his lectures.* The notes on Bradley's students from 1746 to 1760 are located in the back of the notebook.

[221] Rigaud, "Memoirs", p. xlviii.

[222] Bodleian Library, *Thomas Hearne Papers,* No. 94, p. 61.

# Chapter Five.

# Greenwich: The Reform of the Royal Observatory.

## The Work of Bradley at Greenwich under Halley.

From the date of the publication of the paper on the aberration in 1729 the national and international reputation of James Bradley grew, particularly after knowledge of his work on the nutation became known following his letter to Maupertuis in 1737. Throughout the 1730s Bradley worked diligently on various projects, such as the nutation, the work on pendulums, and on the development of the new observatory at Shirburne Castle. He worked occasionally with Halley at Greenwich, regularly paying visits when he also travelled to nearby Wanstead in order to undertake his observations on the nutation. The fact that Halley never accepted the validity of Bradley's work on the nutation does not appear to have interfered with the obvious respect the men held for each other. Most of the work that Bradley performed for Halley was undertaken at Oxford and Wanstead, but occasionally he also assisted Halley in his observations at Greenwich.

It is remarkable that although Halley was in his sixty fourth year when he became Astronomer Royal, he undertook a project that required observing the moon through a complete cycle of the Moon's nodes in his determination of a set of Lunar Tables for the use of mariners. Although Hearne asserted that in his opinion Halley was "somewhat lame",[1] he had no doubt about his physical strength when he became the second Astronomer Royal in 1720 after the death of Flamsteed in 1719.[2] As Rigaud remarked, Halley

> entered upon a series of lunar observations which would require nearly twenty years for their completion. He was proud of his physical powers, so that even at quite an advanced age he contemplated that he could realistically complete his programme.[3]

Although Halley remained in fairly robust health until about 1737, when in his eighty-first year his right hand became paralysed,[4] there is some evidence that the Royal Observatory was increasingly beset by problems. Margaret Flamsteed removed all of the astronomical instruments from the Royal Observatory after the

death of her husband, claiming that they belonged to his estate. When Halley took over the institution it was almost completely bereft of instruments. Halley was left with insufficient resources to replace them, and the system whereby the Astronomer Royal was supposed to use his own instruments was revealed to be impractical. The government made a grant of £500 in order to re-equip the observatory and Halley first purchased a 5-foot transit instrument in 1721. This he housed in "a little boarded shed between the study and the summer house."[5] After making tests he was able to determine that Flamsteed's brick meridian wall was subsiding, apparently because it was sited too close to the brow of the hill. At some time in 1723 or 1724 Halley built a new wall, on the same meridian, but set back a few yards. On this he had proposed to erect two identical 8-foot quadrants, one to observe the southern sky, the other the north.[6] George Graham constructed the first quadrant, but Halley possessed insufficient funds for a second, or even for the large moveable quadrant he had also intended to purchase.[7]

Halley's 8-foot iron mural quadrant was to prove to be the prototype for similar instruments in most of the leading observatories throughout Europe,[8] which was mounted in 1725 facing south. Graham's quadrant was to be important to Bradley, for he used it from 1726 through to at least 1753, and he used Bird's replacement from 1750 until the onset of his terminal illness in 1760. The key to the success of the 8-foot quadrant was its extraordinary rigidity, and its consequent capacity to allow astronomers to make precise and accurate observations. The quadrant was constructed by Jonathan Sisson under the direction of George Graham and in July 1725 Graham began dividing the quadrant in the Great Room of what is now referred to as Flamsteed House, the original observatory designed by Christopher Wren. Nevil Maskelyne later claimed it was divided with an accuracy and precision never before conceived.[9] There were two sets of divisions and readings from one could be checked against the other. The inner scale on the limb was divided according to current convention into 90°. This was subdivided by vernier to 30", and from 1745 with the application to 1". The outer scale was divided in 96 parts and was achieved by continual bisection of the 60° arc, laid off by inscribing an arc equal to the radius

of the quadrant and which was numbered 64. Successive bisections then gave 32, 16, 8, 4, 2, 1, etc. parts. One part was therefore equal to 56.15". A table was provided converting the outer scale into degrees, minutes and seconds. The outer scale reading could be directly compared with those of the inner scale.[10]

In spite of the quadrant performing well for several years, Halley began to experience problems from the early 1730s, very largely because of some degree of settlement of the meridian wall on which the arc was attached. From 1734 the problems had become marked enough for Graham, Bradley and Machin to begin making regular visits to Greenwich in order to make various adjustments to the quadrant. On the 28th June, 1734 Bradley wrote,

> I examined Dr. Halley's Mural Arch and found that ye Center fallen towards ye South so much, that the wire of the Plummet, instead of bisecting the spot (that it was at first adjusting to) fall southward of it, so that the north edge of ye wire touched the south edge of ye spot; the difference of ye present situation of the Arch from what it was when last adjusted (which I believe was about a year after it was first set up) amounting as far as I could estimate to about 20". The Index now giving the zenith distances 20" too small.[11]

On the 26th July, 1735 Bradley, Machin and Graham again felt obliged to repeat the trials and experiments on the 8-foot quadrant, which they had performed the year before. Although the formerly determined variation of apparently 20 arc seconds could be allowed for in reducing Halley's observations with the quadrant, the retaining wall was also slumping to the west as well as to the south. Bradley was not satisfied that the tests of June 1734 were accurate or precise enough and in a very characteristic manner again got Graham and Machin to join him, this time to determine the exact variation since the quadrant had been tested in 1726. He recorded their observations stating, "from this experiment it appears that I have guessed the error too much by 7" or 8" in the former experiment."[12] The rigor and precision of his work is characterised by his constant insistence on rectifying his instruments. There is in his entire approach an incipient determination to control the conditions under which he made his observations. As

I will later discuss, this concern was translated in the way that he trained his assistants.

Katherine M. Olesko has posed the question when and under what conditions refined measurements were considered trustworthy enough to be used in the construction of knowledge taken to be reliable.[13] Indeed, the question of what refined measurements were in the first place was also raised. In 1816 the Prussian government commissioned members of the Akademie der Wissenschaften to participate in the reform of weights and measures.[14] By 1828 F. W. Bessel had completed his extensive studies of the seconds pendulum which was to form the foundation of the Prussia's unit of length, the foot.[15] In 1818 Bessel had completed his reduction of the monumental series of observations made by Bradley and his assistants at the Royal Observatory at Greenwich from 1750 to 1762.[16] But whilst Bessel's philosophical views were largely shaped by the ethos of early 19th century Prussian social values and scientific ideals, there can be little doubt that his own distinctive approach to the meaning of precision was constructed in the period when he was analysing and reducing Bradley's data in preparation of the *Fundamenta Astronomiæ*. Bessel, though ascribing to the necessity of precision, never claimed that this was a key to "absolute" truth, for at this time he began to develop the concept of the "personal equation".[17] Olesko writes, "Bessel later said that social considerations determine standards of measurement."[18] His was an approach determined by the practical needs of society, of trade and commerce.

In this thesis I have argued that in his mature work Bradley rejected the notion that accuracy and precision guaranteed epistemological certitude; indeed he clearly accepted the view that empirical data was insufficient to establish theory, and recorded every source of possible error in order to enable later observers to apply different theoretical analyses when reducing his observations. Bradley's concern with accuracy and precision was integral to his concern with the significance of error. As I have already inferred he believed that knowledge was constructed from observations and experiments, but he recognised also that precision and accuracy did not necessarily lead to any form of certainty, indeed

that it could not. Epistemological nescience is a central characteristic of Bradley's work. Yet the science of positional astronomy was placed on a firm practical and theoretical footing. Bradley did not live to see the practical and observational benefits derived from his work, but this in itself would not have caused him undue concern, for as I have already stressed his entire approach to the acquisition of natural knowledge was conceived of in terms of community and in the shared optimistic values of 18th century British life.

**The Appointment of Bradley as the third Astronomer Royal.**

Halley died on the 14th January 1742.[19] Although he had often expressed a clear wish that he be succeeded in the office of Astronomer Royal by Bradley there was no guarantee that this would take place. Public offices in early Hanoverian Britain were in the gift of major office holders of state. Early in 1742 the office of Astronomer Royal was in the power of the Prime Minister Sir Robert Walpole,[20] a man who had used the award of senior offices as a means of advancing his interests and managing the affairs of state. The death of Halley came at a difficult juncture for Bradley, for it coincided with a period when Walpole's foreign and domestic policies were coming under censure in Parliament. It was against this volatile background that the actions of Bradley's patron and friend, George Parker 2nd Earl of Macclesfield became important in the process of assuring Bradley's succession to the office of Astronomer Royal. Even before Halley succumbed to his final illness Macclesfield was making representations on his friend's behalf. On the very day that Halley died Macclesfield wrote from Shirburne Castle to Sir William Jones.[21] He wrote,

> I have by this post written a long and pressing letter to the Chancellor[22] in behalf of Mr. Bradley, whom you know how rejoiced I should be to have it in my power to serve. *But my way of thinking and voting is a great obstruction to having any interest at court*, (my italics) nor can I think of any other way I can recommend Mr. Bradley to this professorship, than by means of the Chancellor and Archbishop.[23] I do not know upon what footing my kinsman Mr Justice Parker is with Lord Chief Justice Willes, it

is certain the latter has great interest with Sir Robert, and it may be of advantage if cousin Parker would state the thing in a proper light to Willes, and represent to him how much it will for Sir Robert's discredit, that the only man in England fit to succeed Halley at Greenwich, confessed so by all who know anything of astronomy, should be put by, and the finest instrument in the universe put into the hands of a person unable to make a proper use of it. I dare say my cousin's friendship for me will make him undertake this part, and endeavour to prevail upon Willes to interest himself for Mr. Bradley, if there be no impropriety in the thing:[24]

Macclesfield was not in favour in the highest circles at this time, but through the 2nd Earl Bradley enjoyed the support of various figures that had the ear of Walpole and his closest allies. Amongst these was Sir William Jones who also had access to the Walpole Whig Henry Pelham who after the 27th August 1743 became Prime Minister. Jones wrote to Macclesfield,

> it is not only my friendship for Mr. Bradley that makes me so ardently wish to see him possessed of the professorship, it is my concern for the honour of the nation with regard to science. For as our credit and reputation have hitherto not been inconsiderable amongst the astronomical part of the world, I should be extremely sorry we should forfeit it all at once by bestowing upon a man of inferior skill and abilities, the most honourable, though not the most lucrative, post in the profession, (a post which was so well filled by Dr. Halley and his predecessor), when at the same time we have amongst us a man known by all the foreign, as well as our own astronomers, not to be inferior to either of them, and one whom Sir Isaac Newton was pleased to call the best astronomer in Europe. And I dare assure your lordship, that if you are pleased to espouse Mr. Bradley's interest, you will have the satisfaction to find your recommendation of him approved and applauded universally by those who are versed in those studies, both home and abroad. As Mr. Bradley's abilities in astronomical learning are allowed and confessed by all, so his character in every respect

is so well established and so unblemished, that I may defy the worst of his enemies (if so good and worthy a man have any) to make even the lowest or most trifling objection to it. After all, it may be said, if Mr. Bradley's skill is so universally acknowledged, and his character so established, there is little danger of opposition, since no competitor can entertain the least hope of success against him.[25]

The admiration of Bradley's skills as an astronomer was linked to his gentility and his trustworthiness as a gentleman. As with Bradley's other offices, his eventual accession was effected not just by his abilities as an astronomer, but through the social and political patronage he had so diligently tended. Pelham's explicit friendship and admiration is later obvious, for he appears to have come into contact with Bradley through a connection formed with his wife, Lady Katherine, who was apparently impressed by Bradley's character. Every account of his character that I have been able to trace bears witness to his "gentle and kindly demeanour", and others reveal that he possessed a natural ability to explain difficult concepts in a candid and patient manner to various inquisitive members of the public, that he possessed the "common touch".[26] Given these social skills he must often have impressed many, including those that formed the social and political elites, especially those who were able to influence the elections and appointments that advanced his scientific career. This must be accepted as the key to so much of Bradley's advancement, for in an age when social graces were valued, such abilities must have brought him to the attention of some of the most powerful members of Hanoverian society, particularly given the fortuitous connections he first acquired through the influence of his uncle. When Walpole was finally defeated in a division of the House of Commons on the 2nd February, 1742[27] and Parliament was adjourned Bradley was appointed as the third Astronomer Royal the next day, on the 3rd February.[28] It appears that one of the first acts that Walpole undertook after his defeat in the Commons was to appoint Bradley to the vacant office of Astronomer Royal before he eventually resigned on the 11th February, 1742.[29] In the background it appears that the King also expressed an interest. Bradley first came to the attention of George II when he

was the Prince of Wales and when Bradley's partner Molyneux was his private secretary. Through this patronage Bradley had initially received the part living near Narberth in Pembrokeshire. The King possessed an enquiring intellect and appears to have availed himself of advances in astronomy and was aware of Bradley's work on aberration. Later, George II directly interceded to ensure Bradley acquired sufficient resources to build the new observatory and equip it with the finest instruments in Europe.

**The State of the Royal Observatory's Instruments in 1742.**

The difference in the observational skills of Halley and Bradley was made apparent as soon as Bradley took up the office. Halley customarily used but a single vertical wire in the focus of the quadrant's telescope, and in that of the transit instrument. As he lacked an assistant this made observation in both instruments virtually impossible unless he used Bradley's "eye and ear" method which would have been difficult in his infirmity. In contrast, Bradley utilised at least three vertical, and often five vertical wires enabling him to track the passage of a star in more than one instrument as it passed through the field of view. After being appointed as the third Astronomer Royal Bradley remained as Savilian Professor of Astronomy and Lecturer in Experimental Philosophy at Oxford. In view of these demands Bradley was determined that he would appoint an assistant. This greatly increased the observational capacity of the Royal Observatory and he would be sure that his programmes of observation were maintained on an almost continuous basis.

Before he began any observations Bradley needed to repair and rectify the available instruments. He was aware of the problems of the great 8 foot quadrant which had been designed and constructed for Halley by Graham.[30] The neglect of the instruments during the final years of Halley's life as he declined into inactivity had made them all but unusable. From the 5th July 1742, Bradley began to examine the state of the quadrant. On the following day he wrote,

> I this day took off the Telescope and the centerwork having discovered the day before that the screws (which held the brass cilinder about which the

steel plate turned) were all broke so that the Brass Cilinder itself moved round with the telescope which now was supported by means of the smaller part of the Brass center piece which turned in the hole of the square Brass Plate that is perseved to the Ironwork. The Brass cilinder itself adhered so closely to the inside of the steel collar that I could not move it which must have been the occasion of the screws being broken the friction being greater than the strength of those screws.[31]

Bradley was barely able to make any useful observations during the first few months of his incumbency. This was in part was due to the demands of his teaching commitments at Oxford, but the state of the observatory and its instruments was in such a sorry plight that much of his early work was concerned almost solely with examination, repair and rectification. This must have been a very frustrating period in his life. However patient, genteel and kindly he may have been he was also a man of almost boundless ambition, and to inherit the Royal Observatory when it was in such an advanced state of disrepair and decay must have appeared an almost insurmountable problem at times. He set himself the task of evaluating the real state of the instruments before starting a programme of observation. Bradley discovered that he had to pay close attention to the most basic requirements. Later he wrote:

> When I came to reside at the Observatory in June 1742 I made but few observations either with the mural Quadrant or Transit instrument, till I had made some alterations in both. For there being no good method of illuminating the wires of the Telescopes either of the Quadrant or Transit Instr. Soon I found it necessary to make proper Apparatuses for that use. Dr. Halley seldom attempted to make Observations when the wires required to be illuminated, but when he found <u>that</u> to be necessary he usually (for the Quadrant) placed a candle upon the south end of the wall on which it hangs, which shining upon the inside of the shutter (which was painted white) the light was thence reflected thro the Object Glass upon the wires in the focus and by raising the shutter (which slided up and

down) till the upper edge of it was in part before the Object Glass, more or less light was reflected into the Telescope.[32]

Such an industrious and exacting observer as Bradley found these primitive arrangements impossible to reconcile with a practice that demanded an extreme regard for precision and accuracy in a wide range of observational conditions.[33] Bradley saw a virtue in exactness. To him it was essentially a moral issue. He appears to have treated his predecessor's work with respect despite his observational shortcomings. Although Halley had been infirm for many years prior to his demise, it is obvious that when Bradley took up the reigns at Greenwich, he not only had to deal with the deterioration due to years of neglect, but also with the inadequacies of his predecessor's practice. Given the standards by which Halley appeared to undertake his own observations, it is easy to understand why Halley never accepted Bradley's claims to be able to sense motions as precise as half an arc second, and why he never once accepted the validity of Bradley's work on the nutation of the Earth's axis. Even as early as August 1727, when Halley visited Wanstead to use the new zenith sector constructed by Graham for Bradley, it was obvious that he was fundamentally inadequate as an observational astronomer in comparison to Bradley.[34] Yet Bradley had the highest regard for his mentor and friend, and Halley certainly reciprocated those feelings.[35] Although it must have been difficult for Bradley to accept Halley's negative opinions regarding his claims for the high precision of his observational work, he accepted that his great mentor possessed many other notable qualities and simply accepted his friend's strictures, believing that he lacked sufficient skill as an observer to appreciate that such precision was the product of a methodology which paid attention to the fine detail he regularly used in his own practice.[36]

By August 1742 Bradley was already beginning to work in tandem with his first assistant, his nephew John Bradley (son of his eldest brother William). All of Bradley's assistants were trained to the same high standards that he set himself in his observational work. Just as much as his own practice was constructed in the context of Pound's observational and Halley's mathematical

practice, then so that of Bradley's assistants was developed in the context of a practice, with its reference to both analytical and empirical skills that was precise and accurate.[37] It is Bradley's diligence to detail, and the continuous rectification of his instruments that impresses. Typical of his approach can be observed in his preparations for the use of the Transit Instrument and the Graham 8 foot quadrant in August 1742. In his notebook he wrote,

> the line of collimation of the Transit Instrument was adjusted about July 24th, but it could not be properly directed till I had a clear view of the mark which Dr. Halley had made on the Park wall; the sight of which was intercepted by the boughs of trees which had grown up since Dr. Halley had used this instrument;[38] these being cut away about the end of July 1742, I then set the Instrument by his mark, which I at first supposed to be exactly in the meridian, but afterwards found that it lay 12" or 15" in the Azimuth to the west of the true meridian.
>
> Between the first and 14th of August, several stars were observed by the Transit Instrument and the Quadrant and by comparing the times of their Transits the errors of the Place of the Mural Quadrant were found to be entered in the Table before August 14, 1742.
>
> The Quadrant was not altered till Aug. 18 when it was rectified so that the center lay exactly over the beginning of ye Divisions or the 0 point, & then a Plummet hung from the notch at top at the center Plate was found to correspond well with the point on the limb below, to which it had been at first adjusted by Mr Graham in 1726.[39]

Bradley continued making further adjustments to the instrument and on the 20th August 1742 he altered the plane of the quadrant "to lie much truer than it did, as left by Dr Halley, as appears by the Table of errors then found."[40] He had already sent the telescope attached to the quadrant to Jonathan Sisson so various alterations could be made to it.[41] On its return on the 11th September, he adjusted the line of collimation, "Mr Sisson having made for me a new apparatus for that purpose."[42] Several additions were made by Sisson to the transit instrument and the telescope was balanced so that it would stand in any inclination. The notes

made by Bradley reveal just how little use Halley had made of the transit. Bradley wrote,

> in Dr. Halley's time the eye end being the heaviest, he was obliged to support it at the proper altitude, upon which account observations could not be made so expeditiously as now.[43]

This is an extraordinary revelation. It shows that the instrument was so out of balance that it was unusable, for Halley needed to manually support the instrument while he was observing with it. This lack of use is confirmed by the notes made by Bradley when he first began using the instrument. He explained how Jonathan Sisson also put in two other wires some 15 minutes of arc from the middle wire, to facilitate accurate observation of the transit of objects, particularly when clouds interfered with measurement as the object passed through the centre wire. He wrote:

> These alterations were made, and the line of collimation of the transit instrument was adjusted about July 24, but it could not be properly directed till I had a clear view of the mark which Dr. Halley had made on the park wall, the site of which was intercepted by the boughs of trees which had grown up since Dr. Halley had used this instrument. These being cut away about the end of July 1742, I then set the instrument by his mark, which I at first supposed to be exactly in the meridian, but afterwards found that it lay 12" or 15" in azimuth to the west of the true meridian.[44]

Bradley appears to have spent almost the whole of the second half of 1742 on the rectification and improvement of the instruments of the Royal Observatory, a measure in part of his professionalism and dedication, and in part of the wretched state of the instruments, due in part to Halley's limitations as an observer and his growing infirmity during his last years of life.

**The Observations from 1742 to 1750.**

Over the next year and a half Bradley and his nephew John continued to use the transit instrument and the 8 foot quadrant, cross checking the results

obtained with the instruments at Shirburne Castle when used by Bradley and the 2nd Earl of Macclesfield, either together or singly, for the new Astronomer Royal was able to use the instruments at Shirburne almost as if they were his own. The observations at Shirburne revealed the inadequacy of the instruments at the Royal Observatory.[45] About 1,500 observations were made at Greenwich and Shirburne between 25th July 1742 and the end of the year.[46]

During the whole of August 1742 Bradley made repeated observations of *Capella*, above and below the pole at 12 hourly intervals, and the error in the position of the mark was estimated. On the 3rd September the clock was fixed against the brick wall and its dial made to face north west.[47] On the 11th of October Graham added a gridiron pendulum to the clock of the design first developed by John Harrison.[48] On the 4th of December there is an entry revealing Bradley's return from Oxford after presenting a course of lectures in experimental philosophy. From 16th of October all of the observational entries compiled in the registers are in John Bradley's hand.[49] The style, however, is entirely that of his uncle. It reveals that John had acquired many of the transferable tacit skills that his uncle had developed over three decades of ever more precise and exacting observation.

Bradley undertook various major programmes of observation once he was had attended to the many tasks he had set when he had been appointed Astronomer Royal. Eventually he was to transform the administration and the performance of the Royal Observatory, and to reform the science of positional astronomy, developing the science to levels of practical competence never before conceived of. His work in 1743 alone reveals many insights into the new levels of competency achieved by himself and his nephew.[50] The transit observations alone amount to 177 folios.[51] On the 8th of August 1743 no less than 255 accurate and precise observations were made[52] together with accurate recordings of barometric pressure and atmospheric temperature. Several times, over 200 observations a day were made during a sustained programme of extraordinarily indefatigable energy. Even more remarkably, this sustained demonstration of dedicated endurance was

performed at a time when Bradley suffered continuous problems induced by the constant necessity of having to adjust the collimation of the transit instrument.

The quadrant observations for the same year come to 148 folios.[53] These observations probably involved even greater efforts than those displayed in the work with the transit. The exact time of the passage of a star through the sights was recorded as well as its altitude. The arc was read off on the outer 96 divisions and then reduced to sexagesimal degrees, minutes and seconds. Again, the observations were numerous. On the 8th of August, the day Bradley made 255 observations with the transit, he also made 181 observations with the quadrant. The pages are ruled for 60 observations a page, although Bradley regularly used some lines for notes about the collimation and other adjustments. If it is assumed that there are about 55 observations a page it can be estimated that 18,000 rigorous, accurate and precise observations were made in 1743. This industriousness was as characteristic of Bradley's practice as his commitment to precision and accuracy.

Bradley was repeatedly rectifying his instruments in order to get them to perform to the rigorous standards that he demanded. The voluminous notes he made appear very repetitive, with the result that it is easy to overlook the fine adjustments that were to lead to his apparent satisfaction. There are several passages which record his increasing though never full satisfaction through 1744 and 1745. Thus on the 6th June, 1744, when testing the line of collimation of the quadrant he wrote,

> The Instrument now (when I look Northward) points very near the same marks as it did when Mr Graham & I adjusted the line of Collim in the year 1726 but as the Southern mark has been made near two inches more easterly than 'twas before … and I judge from the present circumstances that 'tis the mark on the Park wall that has altered, for I find the marks that I made on a chimney in Greenwich do now exactly correspond with the same distant objects with which I formerly compared them.[54]

Over the next year Bradley worked with the quadrant and two transit instruments, and as was his practice repetitively recorded his results against the

possible causes of error, instrumental or systemic, or to changing weather conditions. By 1744, he was regularly recording barometric and thermometric observations to further aid the accurate reduction of his observations. On the 1st July, 1745 he entered into his notebook,

> With a movable mark on the Park wall I tried how near I would judge when it was truly bissected; and after several trials I found that the difference of the places of the mark did not exceed 4/10 of an Inch, which answers to 2½". So that from this experiment it may be concluded, that we can direct the Instruments to the same point always little more than one second in Azimuth.[55]

Yet this scarcely met the requirements of the rigorous standards of Bradley's mature observational practice. It must be recalled that while he was wrestling with the perceived shortcomings of the instruments left to him at Greenwich, he was moving into the last two years of observation of the nutation at Wanstead, four or five miles away, north of the River Thames. The zenith sector, which was later to be used to help collimate the instruments of the Royal Observatory, could be counted on to detect motions as small as half an arc second. When the transit observations are examined the passage of the stars through the wires was usually recorded to the nearest second, but where there was a perceptible interval Bradley commonly entered either a + or a – next to the data, signifying a variation of about a ⅓ or a ½ second of arc.[56] Later, from September 1772, Bradley's successor, Nevil Maskelyne was to introduce a decimal notation which was to become the standard method of recording small sub-second arcs,[57] but Bradley's equipment lacked the refinements that even a few decades would bring. His wires were thicker, and as well as lacking achromatic lenses, the resolving powers of his instruments were considerably poorer than those enjoyed by those who followed him. For example, in 1743 Bradley wrote, "This day I put a shorter eyeglass into the transit telescope, its focus is about 1½ inch, so that the telescope magnifies now about forty times."[58] This compares very unfavourably with the telescopes used by his successors, Maskelyne, Pond and Airy.

It was Maskelyne who first remarked that Bradley was the first observer who "noting the proportional distance of the star from the wire at the two beats immediately preceding and following the transit across the wire,"[59] greatly improved the efficacy, accuracy and precision of his observations through the application of a standard observational technique, which moreover he was to transmit to his assistants. So much of Bradley's technique was acquired and refined through constant practice, and which once developed, was analysed so that such skills could be passed onto assistants who were trained to observe to the same exacting standards. When using Graham's 8 foot quadrant, Halley's maxim was "secure your minute",[60] and with the divisions on the vernier for the outer arc of 96 divisions being equivalent to 13.18", which he read off by estimate to about half this arc, his observations were rarely more accurate than 7". At first Bradley was equally limited by these circumstances, but on the 18th of July, 1745 a new micrometer screw was fitted to the instrument. He wrote "the threads of which are much finer than those of the former, viz. 39¼ threads in one inch … so that one revolution of it will alter the inclination of the telescope 53 seconds."[61] After the addition of this refinement, Bradley was able to read off angles as small as a second of arc. In the middle of August Bradley wrote that he

> fitted a shorter eyeglass into the quadrant telescope (viz. one of two inches focus, the old one being about one inch longer,) and likewise put a small horizontal wire, that is only 1/440th part of an inch in diameter, or subtending [an] angle of 5" only; the former, that was of the same size with the perpendicular wires, seeming too gross for small stars.[62]

In spite of all these adjustments, and his constant rectification of his instruments, Bradley was evidently less than pleased with the tools at his command. In comparison with the instruments that he had access to at Shirburne he recognised that those of the Royal Observatory were less than what was required for the task he was setting himself. He coordinated these early observational programmes by himself and his nephew at Greenwich with those undertaken at Shirburne by in partnership with Macclesfield. Increasingly they revealed the shortcomings of the instruments at the Royal Observatory in comparison with those at Shirburne.

During 1746 Bradley discovered that a distortion in the figure of the quadrant of up to 15¾"

> in the whole part of the quadrant is occasioned by the cylinder [on which the telescope turned] being placed … about 1/191 part of an inch from the true centre of the arcs, in a line passing through it at 45° on the arch; for such a position of the cylinder will occasion such a defect in the quadrantal arc as was found in Sept. 1745.[63]

He repeated his examination of the quadrant in February 1747. At this time he was able to determine that the eccentricity did not amount to much more than 1/400 of an inch, having also estimated that the length of the quadrantal arc had been gradually diminished. This testified to the changing figure of the instrument. The quadrant had been constructed by Graham to preserve the rigidity of the framework, but by the mid 1740s, after twenty years of use, it was obvious that this could no longer be relied upon. It was about this time that Bradley was fully reconciled to the necessity of acquiring new instruments for the Royal Observatory. No matter how much he rectified and refined his instruments they were simply not up to the task to which he was now committed, an astrometrical survey of all of the brighter stars[64] observable from Greenwich.[65]

It was during this period, early in 1747 as he was nearing the end of his work at Wanstead, that he began planning the new observatory and the instruments that he would need in order to undertake the programmes of systematic observation he now had in mind for the Royal Observatory. From 1749 Bradley amended his teaching commitments at Oxford, mindful of the task to which he was committed, nothing less than the complete reform of the Greenwich Observatory. The evidence for this conclusion must remain largely circumstantial for very little if any documentary sources related to this important transition in Bradley's life appear to be extant.[66] Up to 1748 Bradley had presented his courses in experimental philosophy three times a year. After 1749 his expanding commitments at Greenwich connected with the work he needed to undertake in planning the new transit house, and the acquisition of the new instruments he

needed for his observations necessitated a cut-back in his commitments at the university.

The series of registers recording the observations made by Bradley during the entire period of his incumbency were originally given to the University of Oxford some time in 1776 after the Board of Admiralty had abandoned its law suit against the executors of Bradley's estate. With the publication of the *Nautical Almanac* under the supervision of the 5th Astronomer Royal Nevil Maskelyne from 1766[67] and the continuing delay in any prospect of the publication of Bradley's observations the Admiralty no longer considered the suit worth pursuing. It remained a continuing bone of contention until after several representations the registers were finally transferred to the safe-keeping of the Royal Observatory in the mid-19th century. The observations from 1742 until 1762 fill 14 registers.[68] In addition there were a series of copies in "fair hand" which were commonly used as the basis of their later publication and reduction.[69] There are various records still in the possession of the Bodleian Library which cover various periods from 1742 to 1762, including the observations made by the Bradleys at Greenwich from 1742 to 1743.[70] These extraneous records and registers reveal that not all of the observations made at Greenwich by and under the direction of Bradley were transferred from the Bodleian Library to the Royal Observatory. Indeed there is a lot of material found at Oxford which should really be included in the Royal Greenwich Observatory Archives at Cambridge University Library.[71] I assess that the number of recorded observations that were made using the transits and quadrants was somewhat in excess of 250,000. The first of Bradley's "working registers" dates from 1743.[72] It records transits of the major stars using Halley's transit instrument filling 177 folio pages. At this time Bradley and Macclesfield were busily cross comparing the results obtained at Greenwich with those acquired at Shirburne. A considerable number of the entries at Greenwich are made in John Bradley's hand[73] just as the Shirburne registers[74] possess many entries in James Bradley's hand.[75] In this first register there are included calculations to discover the difference between the meridians through Oxford and Greenwich and between Shirburne and Greenwich. Also dating from

this early period of Bradley's tenure at the Royal Observatory is a register of observations made with Graham's eight foot quadrant.[76] Again, these quadrant observations were conducted simultaneously with a similar set made at Shirburne.[77] The register is also interesting because it includes evidence that Bradley's eventual assistant and successor to the office of Astronomer Royal, Nathaniel Bliss, was working closely with him at Oxford, for it includes an account of Bliss's observations of Jupiter at Oxford on the 21st May 1743.[78]

After the initial period of assessment and adjustment Bradley undertook a sustained programme of observation both with the transit used by Halley and the Graham quadrant. The second transit register covers the period from 1744 to 1746.[79] The next of the quadrant registers includes a veritable pot pouri of odd miscellaneous items in addition to the record of observations from 1744 to 1747.[80] Most important of these items is a roughly written table of variations to the quadrant from 1743 to 1744 which reveals Bradley's continued dissatisfaction with this instrument, for he was forced to rectify it with a repetitiveness that hindered his ongoing observations. The number of observations that John and James Bradley were making during these years must have made it a tiresomely frustrating task, attempting observations with an instrument subject to so many variations to the figure of the quadrant as well as to the retaining wall which was in motion east to west as well as north to south. There are several loose papers which refer to these problems including a record of the collimation errors of the quadrant,[81] errors of the planes of the quadrant of various times from 1742 to 1745,[82] and various data relating to the adjustments made to the quadrant.[83]

There is documentary evidence from this time demonstrating that Bradley was not merely concerned with the state of the observational instruments of the observatory. There are some loose papers on the errors and rates of clocks at his disposal which also include part of a letter concerning the building of a new observatory. It is undated but the papers on the clocks range from 1743 and 1748.[84] It suggests that Bradley was discussing the complete rebuilding and re-equipping of the Royal Observatory at least before 1748. The paper and the ink used appear similar to other papers written during 1746. Having been instrumental

in the construction and the equipping of the Shirburne Observatory from 1739 to 1740 on behalf of Macclesfield,[85] it is surely possible that Bradley was considering a similar programme at the Royal Observatory from an early period of his incumbency. The ongoing problems that Bradley was experiencing with the eight foot quadrant must surely have increased his resolve.

In the register containing observations with the Graham quadrant from 1747 to 1753[86] is a useful summary inside the front cover of the various repairs and alterations made to the instrument from 1742 to 1753. It reveals the extent of the problems that Bradley had to contend with before the Bird quadrant was constructed and mounted in 1750. The equivalent register for the observations made with the old transit instrument from 1746 to 1749,[87] contains a supplement on lunar eclipses from 1748 to 1751. There is also a register from 1749 to 1750,[88] which in addition to containing further transit observations from 1754 to 1755 also contains a record of an earthquake shock recorded in Greenwich at 8.23am on 8th of February, 1750.

This concludes the brief survey of the registers and papers relating to the early years of Bradley's incumbency. Although he experienced the continuous frustration of using instruments that did not meet with his own exacting standards, his rigorous practice somewhat countered their inadequacies. His method of continuous rectification helped to deal with the variations experienced with the Graham quadrant, but as the summary of his repairs and alterations demonstrates[89] he must have found the instrument almost impossible to rely upon at times. As Bird was to argue when he constructed the new quadrant to replace it, the rigidity of the frame was undermined by the weight of the ironwork. The new quadrant used Graham's design but was constructed in brass, a move that was to demonstrate its effectiveness in the years after 1750. This survey also reveals that when the registers were transferred from Oxford University to the Royal Observatory in the middle of the 19th century a lot of relevant material remained at Oxford including all of the observations made at Greenwich during 1742.[90] The 43 folios here recorded are a serious omission from the records of the Royal Observatory.

**The Development of the New Observatory.**

As I have already intimated, the Royal Observatory was in a pitiful state when Halley died, but throughout his incumbency he had lacked the resources necessary to maintain adequate programmes of observation and was grateful for the assistance of men like Bradley, Machin and Graham. Graham's 8 foot quadrant was a remarkable instrument, which was to become the model for several such instruments that were to be placed in leading observatories in Europe. But no matter how well designed and constructed, the 8 foot quadrant was by 1745, and certainly by 1747, no longer capable of meeting the demands being placed upon it. Bradley had come to recognise that it was inadequate not merely for his current needs, but more importantly to his future objective, the exacting astrometrical survey of all of the brighter stars observable from Greenwich.[91]

Although his studies of pendulums at Greenwich date from the 13th September 1743 and were continued to 1st August 1745, his attentions were further directed to this problem in 1748 when his observational work inevitably lessened as he began to plan the future development of the Royal Observatory. On the 28th of January 1749[92] he began his final experiments and began to write up notes on the subject. This was a topic which had acquired his attention from 1719 and from experiments at Wanstead he had discovered that the length of a seconds pendulum at the latitude of London was between 39.14 and 39.168 inches.[93] At Greenwich Bradley used a wire that was 0.1" in diameter, giving it a rigidity that gave his apparatus an immense improvement over previous attempts.

In the summer of 1748 he represented the state of the Royal Observatory and its instruments to the Visitors of the Royal Society. During August 1749 the Council of the Royal Society considered a petition to the Lords of the Admiralty, which had been prepared by Bradley.[94] Once it had been approved by the Council it was signed by the President Martin Foulkes, and the rest of the Council. Some alterations were made to the petition so it was not presented to the Lords of the Admiralty until October. On the 9th October the President informed the Council that he and Bradley "had attended the lords of the admiralty, and that their

lordships had promised their assistance in forwarding the petition."[95] There was a detailed schedule attached to the petition, prepared in his usually thorough way by Bradley, together with an account of the sum required, which was estimated to be £1,000. It was granted by George II without delay.[96] Following the publication of the paper on the nutation and the award of the Copley Medal in 1748, Bradley's pristine national and international reputation was a considerable aid in the acquisition of patronage and influence, but Bradley had already enjoyed the patronage of George II when he was still the Prince of Wales, and the King had long taken an interest in the astronomical exploits of the Astronomer Royal.

George II directed an order that the sum of £1,000 be paid to Bradley,[97] to repair the old instruments and to provide new ones. Rigaud listed the particulars of the estimates in the *Memoirs of Bradley.* It included backdated payments of £85 16s 0d for instruments and repairs from December 1742 which had been paid for out of his own pocket.[98]

| | | £ | s | d |
|---|---|---|---|---|
| 1742, Dec. 8. | By Mr. Sisson's bill for altering the transit. | 19 | 8 | 0 |
| 1742, Aug. 10. | By a pair of globes of Mr. Senex. | 8 | 9 | 0 |
| 1742, Feb. 9. | By Mr. Graham's bill for a pendulum. | 15 | 13 | 0 |
| 1744, Jan. 3. | By Mr. Graham for a pendulum. | 10 | 0 | 0 |
| 1745, Oct. 1. | By a micrometer for the mural quadrant, by Mr. Sisson. | 3 | 3 | 6 |
| 1746, Feb. 11. | By an apparatus for trying the line of collimation, by Mr. Bird. | 2 | 12 | 6 |
| 1746, May 29. | By an arch for the transit instrument, by Mr. Bird. | 8 | 13 | 0 |
| 1747, July 9. | By a level by Mr. Bird. | 1 | 11 | 6 |
| 1748, Oct. 12. | By alterations in the transit instrument, &c. by Mr. Bird. | 5 | 15 | 6 |
| 1743 – 1748. | By alterations made in the quadrant and transit rooms by smiths and carpenters, their bill | 10 | 0 | 0 |
| | By glasses and a perspective of Mr. Mann. | 3 | 18 | 0 |
| | By a chamber-alarum by Mr. Graham. | 3 | 3 | 0 |
| | By a parallactic sector 12½ feet radius. | 45 | 0 | 0 |
| | By a diurnal sector, with a telescope of 30 inches. | 35 | 0 | 0 |
| | By an apparatus for frequently observing the variation and inclination, or dip of the magnetic needle. | 20 | 0 | 0 |
| | By a 15 feet refracting telescope and a micrometer. | 20 | 0 | 0 |
| | By a clock per Mr. Graham. | 39 | 0 | 0 |
| | By a brass mural quadrant by Mr. Bird. | 300 | 0 | 0 |
| | By a moveable quadrant per ditto. | 200 | 0 | 0 |
| | By a transit instrument ditto. | 73 | 13 | 6 |
| | By a 20 feet refracting telescope ditto. | 7 | 10 | 0 |
| | By a barometer per ditto. | 2 | 12 | 6 |
| | By a thermometer per ditto. | 1 | 15 | 0 |
| | By alterations to the old mural quadrant per ditto. | 32 | 10 | 0 |
| | By fees at the treasury. | 2 | 2 | 0 |
| | By Mr. Short in part for a six feet reflecting telescope. | 100 | 0 | 0 |

| | | |
|---|---|---|
| 972 | 0 | 0 |
| 28 | 0 | 0 |
| £1,000 | 0 | 0 |

Rigaud also included a footnote in which he assumes that the payment of £3 3s 6d to Jonathan Sisson should have been given as £3 13s 6d in order to make up the sum of £972.[99]

The costs of the new buildings, including the Transit House, to house the new and old quadrants and the new transit, which were built from 1749 to 1750, must have been considerable. These outlays were probably borne by the Board of Ordinance.[100] The ten pounds paid to smiths and carpenters must have been for work subsequent to the earlier construction of the new transit building.[101] Shortly after its construction Bradley wrote a memorandum:

> In the year 1749, £1,000 was given by his majesty, to be paid by the treasurer of the navy out of money arising from the old stores of the navy, (upon the representation of the lords of the admiralty, and principally upon Lord Anson's recommendation,)[102] to buy some astronomical instruments for the use of the Royal Observatory; when it was proposed by Mr. Foulkes, Mr. Graham, and Mr. Robins, who were consulted with on that occasion, that in the catalogue of instruments to be purchased a parallactic sector should be inserted, as very useful for observing stars near the zenith; and the sector which I formerly hung up at Wansted, in 1727, (with which I afterwards discovered the laws of aberration of the fixed stars, as also the nutation of the earth's axis,) being judged by them worthy of a place at the Royal Observatory, I removed it from Wansted in July 1749, and procured a new apparatus for suspending it, (made by Mr. Hearn, as the old one was;) and I likewise took care, while the rooms of the new observatory were building, that there might be made convenient places for hanging the sector, both in the new quadrant room and in the new transit room: my view (in providing for its suspension in either room) was, to render it useful for settling the true zenith,) whereby errors of the line of

> collimation of the telescopes of the mural quadrants may be found with great ease and certainty.[103]

Rigaud stated that the provision for two separate places of suspension was with the view to the advantage of reversing the instrument, the face of the sector being turned to the east when it hung in the quadrant room, and to the west in that of the transit.[104]

The clock placed at Graham's account was not made by him for by now he was almost 75 and too old to work to the exacting degrees of precision required by Bradley.[105] Rather it was constructed by Shelton under Graham's instructions. Most notably, several of the instruments were constructed by John Bird. This was to be a partnership that would be as important to Bradley as his former friendship and partnership with Graham. Of all the instruments constructed by Bird, the most celebrated was the new 8 foot quadrant, modelled on the quadrant designed and constructed for Halley by Graham in 1726. Bird constructed the frame out of brass so that the new instrument retained the strength and rigidity of the former, while avoiding many of the problems associated with internal distortion due to the weight of the instrument. It had been ordered by Bradley in February 1749,[106] and was suspended on the 16th of February 1750,[107] and by June it was finally ready for observation, the first recorded observation being undertaken on the 10th August, 1750.[108] Throughout Bradley's own lifetime there was no measurable variation in the figure of the new quadrant,[109] although Bradley's own tests revealed that it was 2" short of a complete quadrant, which Bird verified, although he held that it was nearer 1½" short of the full 90°.

Bird stated that when he recognised that he was to be employed to construct a new quadrant for the Royal Observatory he examined and thoroughly familiarised himself with the quadrant constructed for Halley by Graham in 1726.[110] Although he admired Graham's workmanship he early recognised that the alteration in its figure was largely due to various sections being forged in iron which he contended was incapable of being forged as easily as brass into the form that was required. The new mural quadrant constructed by Bird to Graham's

design was the first instrument in which the double division of the quadrant was applied.[111]

In 1753, when Graham's quadrant was moved from where it had been situated since 1726, a new set of divisions was marked on it by Bird.[112] With these on the 12th August 1753, Bradley examined the arch by means of his micrometer. He wrote,

> I measured with the screw of my micrometer the difference of the arcs (of 64/96) as set of by Mr. Graham originally, and by Mr. Bird when he put on a new set of divisions upon the old quadrant, and I found that Mr. Graham's arc was less than Mr Bird's by 8/40 divisions of my micrometer, which to a radius of 96 inches answers to 10.6"; so that the whole arc of 96 differs from a true quadrant 15.9", which is the same difference that I formerly found by means of the level, &c.[113]

As well as the new quadrant, Bird also constructed a new transit instrument which also reflected his immense skill and craftsmanship. It was mounted in the transit room which defined the Greenwich meridian from 1750 until 1816.[114] The original specification reveals that it had an 8 foot focal length with an aperture of 2.7 inches which was stopped down to 1.5 inches with an elliptical reflector. It possessed, in conformity with Bradley's earlier practice with the zenith sector in Wanstead, one horizontal and five vertical wires, illuminated by a lamp attached to a nearby pier which could be reflected down the tube by the annular reflector in front of the object glass.[115] The transit initially possessed a single eyepiece,[116] but the field of view could often become indistinct near the edge. To remedy this, a double eyepiece was applied to the transit in January 1753.

It was also a part of Bradley's specification for the new observatory that there should be a Newtonian reflector of about 6 feet in length. The estimate was that this would cost about £115. Bradley paid £100 on account to James Short for this purpose, this being mentioned in the minutes of the Council of the Royal Society for the 5th June, 1755.[117] It appears that there may have been some discussion about Short taking back a Gregorian reflector provided to the Royal

Observatory which he had provided while he was at work on the Newtonian. Short agreed to this and agreed to deliver it in four months. When Bradley died in 1762 the money remained on account so it appears that Short had not been able to fulfil the terms of his contract with the Royal Observatory. Short was often a very difficult and secretive man, who had made a fortune making the finest reflectors on the commercial market until his own death in 1768. He was so secretive about his methods that he destroyed his own instruments before he died. It must have been a disappointment to Bradley, for he had been a pioneer in the use of reflecting telescopes when his friend John Hadley had produced the world's first astronomically useful reflector in the early 1720s.

It appears that Short had earlier given his services to the Royal Observatory for there is notice of a letter sent to a "Mr. Davall", probably the Secretary of the Royal Society, on his methods of making large lenses. In the *Memoirs of Bradley*, Rigaud wrote,

> He delivered "a full account" of it *sealed up*, to be preserved with the papers of the society, and not to be opened till he gave leave for its publication. He appears to have made no communication on the subject during his lifetime, but after his death it was opened up by the council, and printed in the Phil. Trans. (vol. LXIX. p.507.) under the title of "A method of working object glasses of refracting telescopes truly spherical."[118]

The first mirrors that Short had made for his reflecting telescopes were made of glass and he described his method of producing a spherical figure to be so good that in 1749, at Bradley's request, he ground some object glasses for the new instruments at the Royal Observatory, which Bradley considered were much superior to everything else he compared with them, making much use of them in following years.

**The Observations from 1750 to 1762.**

The observations made by Bradley and his assistants at the Royal Greenwich Observatory from 1750 until 1762[119] were the culmination of everything that he had attempted up to then, the consummation of a practice

constructed during the investigation of phenomena in which the imperatives of rigour, accuracy and precision were pushed to the limits of the available technology. Into those observations went almost forty years of accumulated knowledge, technique and practice.[120] With Bird's new instruments, he achieved a greater degree of reliable precision and accuracy than those he had been obliged to rely on with those he had inherited from Halley.[121] By the time Bradley undertook his new programme of observation he was systematically recording the barometric pressure and the atmospheric temperature,[122] as well as the errors in his clocks, in the collimation of his instruments and indeed every possible cause of bias or error induced by the variations in the seeing conditions under which the observations were made. Corrections were applied for the associated effects of annual stellar aberration, for precession and for the nutation of the earth's axis. No astronomer prior to Bradley was so well equipped, either in terms of the instruments he had at his disposal or in terms of his observational practice. None had achieved such rigour in his methods.

It was a strict rigour that was inculcated into his well trained assistants,[123] some of whom were to acquire an eminence of their own. His kinsman Nathaniel Bliss succeeded him as Astronomer Royal whilst Charles Green worked with Bliss and his successor Nevil Maskelyne. The registers containing the observations of Bliss and Green using the Bird Transit Instrument and the Bird Quadrant were continuous with Bradley's and by most criteria were virtually indistinguishable in the terms of their rigour and exactitude. Charles Mason, who in partnership with Jeremiah Dixon made precise observations of the transit of the Sun by Venus in 1761 at the Cape of Good Hope was also a highly competent astronomer and surveyor who later claimed some degree of notice when he and Dixon surveyed the border between Virginia and Pennsylvania, a line that later acquired significance as the boundary between the slave owning states of the south and non-slave owning states of the north in the nascent republic of the United States of America. Bradley's first assistant, his nephew John, left Greenwich to take up a post teaching mathematics at the naval college at Portsmouth.

It is here that I must discuss an extraordinary omission from the Bradley archive. In all of his papers there is almost nothing of the correspondence between Bradley and his assistants. There is a letter addressed to Bradley from his nephew dated the 9th of April 1752, partly concerning a portrait of Tycho Brahe sent to Oxford by Flamsteed, about observations made with Bird's new transit instrument reduced to the beginning of 1751. John stated

> I have sent the particulars of the observations, and under each difference, the mean difference by the number of observations against it; beginning with the difference between Aldebaran and Capella, then Capella and Rigel, and so on to α Cygni, and then between α Cygni and Aldebaran; and I make the sum of differences to be 360° 0' 21.1".[124]

Interestingly Bradley recalculated the mean differences and amended the sum of differences to be a considerably more acceptable 360° 0' 4.3".[125] Rigaud professed to having difficulties retrieving Bradley's papers from Hornsby. It is not that Rigaud thought that Hornsby was being deliberately obstructive, simply that his elderly predecessor had gathered a lot of Bradley's papers (from the extended time when he'd been working on the first volume of the Greenwich observations) and had either mislaid or in many cases simply lost them. Having held the papers for well over 40 or more years this is perhaps to be expected.[126] If the correspondence between Bradley and his assistants is almost non-existent it seems judicious to presume that they were among the papers mislaid or lost by Hornsby. This of course is conjectural but it is difficult to believe that the Peach family withheld them during their dispute with the Admiralty as executors of Bradley's estate.[127] These papers could have dealt with nothing but the minutiæ of the day to day affairs of the Royal Observatory and they could not have contained any personal papers which may have compromised Bradley's reputation. Hornsby may have used them to clarify the information recorded in the registers. For whatever reason, it is to be regretted that they do not appear to be available in either of the major Bradley archives at Oxford or Cambridge,[128] nor indeed in the Hornsby papers at Oxford. I have also searched through archives at Portsmouth,[129] Southampton[130] and London[131] but to no avail. This regrettably

leaves a serious omission from the Bradley archive, for it is difficult to establish how Bradley dealt with his assistants and how he may have trained them. It is possible to infer from the observational registers that his assistants attained standards of practice comparable with his own and this is evident from the final registers when observations made by Nathaniel Bliss and Charles Green with both the transit[132] and the new quadrant[133] are recorded in conformity to those attributed to Bradley up to 1762. In Bradley's miscellaneous papers at the Bodleian Library there are some observations made by Green of an eclipse of the moon in May 1762.[134]

It is possible to infer how at least one of his assistants responded to him. Much of Bradley's achievement was lost to his immediate successors due to the extended dispute between his executors and the Board of Admiralty. These observations comprised of 931 large folio pages when they were eventually published in two volumes in 1798 and 1805.[135] Before these were printed, however, Bradley's other assistant Charles Mason made his own calculations and produced a catalogue of 387 fixed stars from them. This short catalogue was included with the *Nautical Almanack* for 1773. In a letter addressed by Mason to Hornsby on the 27th July, 1774 he wrote,

> If your good-nature will excuse my freedom, I will beg leave to observe to you that I have seen one of the greatest of men lost. Some poor faithful hand might have been found, that, drove by necessity, would gladly for about £30 a year have reduced Dr. Bradley's observations one year under another, and placed them in form proper to have appeared in the annals of fame forever.[136]

Rigaud inferred that Mason probably meant "his own faithful hand" but he concluded that,

> He would, however, have hardly done it so well as M. Bessel, who applied himself to the whole of this vast work at once: although Mason was a laborious computer, he had not the science necessary for such an undertaking as the composition of the *Fundamenta Astronomiæ.*[137]

Whether or not Mason was capable of a full reduction of Bradley's observations to anything like the same rigor as that achieved later by Bessel, one thing is certain, and that is that the value of the observations of 1750 to 1762 was clearly appreciated by those with first hand knowledge of them. Bradley's assistants do appear to have been loyal. His nephew John remained as his assistant from 1742 until 1756 when he took up a teaching post at Portsmouth with his uncle's approval. Charles Mason was appointed some time in 1756 and was still in the employ of the observatory when he was appointed by the Royal Society in partnership with Jeremiah Dixon to observe the transit of Venus in the East Indies in 1761.[138] He appears to have maintained an association with the Royal Observatory up to the 1770s. Nathaniel Bliss was undertaking work with Bradley in Oxford from the late 1730s and became his senior assistant from the mid 1750s,[139] eventually succeeding him as Astronomer Royal in 1762. Charles Green was employed as an assistant from the late 1750s and was still in the employ of the Royal Observatory during the 1770s.

The observations which formed the great series that was to signify the pinnacle of Bradley's observational achievement were made with the new instruments ordered by him to furnish the new observatory. The older instruments sometimes remained in use, particularly the Graham quadrant.[140] The first register recording the observations made with Bird's new transit instrument placed at the median position of the new Transit House, marking the position of Bradley's meridian at Greenwich dates from 1750.[141] This volume also contains a brief resume of adjustments and measurements from 1750 to 1755 on the inside cover and the record of a lunar eclipse on the 2nd December 1750. The first observations recorded on Bird's new eight foot brass quadrant are recorded in a register that also contains a record of the continuing observations made with the original Graham quadrant.[142] The registers that record the continuing observations made with the new transit instrument during Bradley's incumbency make up two further volumes.[143]

The first of these volumes recording the observations from 1755 to 1758 also contain a record of a lunar eclipse dated 30th July 1757, however, of greater

interest is a memorandum of some of Bradley's plans for the transit of Venus due in 1761, including a list of comparative observations to be made with Nevil Maskelyne whilst he was at St. Helena from 1761 to 1762.[144] From the time when in partnership with Pound from 1718 to 1721 he had used observations of Mars in successive oppositions to help determine the mean distance of the Earth from the Sun,[145] Bradley had possessed a continuous interest in the scale of the solar system. The coming transits of Venus due in 1761 and 1769 were widely accepted as the best opportunity astronomers possessed to determine the distance between the Earth and Venus[146] and by triangulation the distance between the Earth and the Sun and thus the size of the "Magnus Orbis".[147] The final registers of observations made with the new quadrant during Bradley's incumbency are both exemplifications of his mature practice and that of his assistants. In the register covering the period from 1753 to 1757 the entries are usually in Bradley's hand but judging from existent written notes and correspondence,[148] it is possible to assert with some assurance that most of the other entries are in Charles Mason's hand.[149] The final register of Bradley's tenure (covering the period 1758 to 1762) sees his entries diminished considerably during 1760 with the handwriting of Bliss and Charles Green coming to the fore. This estimation is confirmed when the first register following the death of Bradley, that from 1762 to 1765,[150] which is signed by Bliss and Green is examined and compared with its immediate predecessor. When the observations made by Bradley and his assistants are compared with most of those which preceded them the magnitude of the achievement becomes evident.

**The Reform of the Calendar.**

Having re-equipped and restructured the Royal Observatory, Bradley was fully prepared to initiate the programme of observation he had been planning for so many years. These observations were begun on the 2nd December, 1750 when he began using Bird's new transit instrument and began recording the registers that were to form the pinnacle of Bradley's astronomical achievements. However, after he had made his own representations about the subject to various figures, he

was elicited to pay attention to the reform of the calendar.[151] Most of continental Europe had abandoned the Julian calendar and had adopted the Gregorian which was introduced into much of Italy, Portugal, Spain and the German Catholic states in 1582. Pope Gregory XIII's reform was based on restoring the vernal equinox to March 11th as it had in 325 AD. By 1581 it fell on March 21st. After the 4th of October 1582, the following day was designated as the 15th of October. Many Protestant states resisted converting to the new calendar, the Protestant German states converted in 1699. By 1750 the continued divergence of the British calendar, now some 11 days behind much of the continent, and with the official new year officially beginning on the 25th March, was perceived as a hindrance to the growing commerce and political discourse with the rest of Europe as the British economy expanded.

The original promoter in Parliament of the assimilation of the British calendar with much of continental Europe[152] was the 4th Earl of Chesterfield.[153] At the time he promoted the Act his out of town residence was the Ranger's House in Greenwich Park, a short walk from the Royal Greenwich Observatory.

Undoubtedly the problems associated with the calendar were first discussed with Bradley before he introduced the bill in Parliament. There can be little doubt that the inconveniences created by the British calendar being out of step with that in France was brought to Lord Chesterfield's notice by Bradley who was in regular and continuing correspondence with various astronomers in France, and was constantly and tiresomely forced to convert reported observations between Old Style and New Style. The difference of 11 days potentially led to chronological inaccuracies and confusion, but he also recognised that it was a hindrance to trade. The bill for reforming the calendar was forwarded by Philip Dormer Stanhope, the 4th Earl of Chesterfield, on the 25th of February, 1751 (OS) and the second reading took place on the 18th of March, when it was seconded by the 2nd Earl of Macclesfield. Both the Prime Minister Henry Pelham and the Lord Chancellor Philip Yorke, Earl of Hardwicke supported the measure. Chesterfield had long been an opponent of both Walpole and Pelham but it appears that this did not prevent Bradley from making his presence felt. Chesterfield was a

raconteur and a wit who famously authored *Letters to his Son* and *Letters to his Godson*, publications described by Samuel Johnson in 1755 as teaching "the morals of a whore, and the manners of a dancing master." Pelham, of course, was a long standing friend of Bradley, and must surely have discussed the various problems posed by the retention of the Julian calendar. The bill, subject to a few minor alterations and emendations, was passed through both houses almost without opposition and received the Royal Assent on the 22nd of May, 1751.[154]

The influence of Bradley on the reform of the calendar can only be established by inference, though there can be little doubt that his influence was real. Henry Pelham's wife Lady Katherine was appointed Ranger of Greenwich Park in 1745[155] and the Prime Minister often resided in a house in Greenwich. Pelham and his wife both held a very high opinion of Bradley, and a real friendship was maintained until Pelham died in March 1754. It is also possible to establish the connection between Bradley and Chesterfield, for when Macclesfield spoke in favour of the bill in the House of Lords he referred to the Astronomer Royal and his discussions with Pelham and Chesterfield.[156] According to the Macclesfield speech the three general tables at the end of the bill were calculated by Bradley. This is confirmed by an undated letter to Macclesfield when Bradley wrote, "I was informed at Mr. Basket's that the tables were all printed off, so that those alterations could not be made which your lordship proposed. I have sent your lordship a copy of the general tables in the form which I at present judge they may be disposed."[157] Davall's astronomical contributions were carefully examined by Foulkes and Bradley, though their competence did not include any examination of the legal proposals and enactments which were very much more within the Secretary of the Society's abilities.[158]

The Act of Parliament[159] passed in May 1751 was for the adoption of a reformed Gregorian calendar in Great Britain, Ireland and the Colonies. From 1752 the year would officially begin on the 1st of January (instead of the 25th of March) and 11 days would be omitted between the 3rd and the 14th September, 1752. The changes in the calendar were not greeted with universal approval. There were intermittent riots and disturbances, recorded in a well known painting

and engraving by William Hogarth entitled, *Give us back our eleven days!* According to Rigaud there was an oral tradition that when Bradley entered his final painful illness in 1762, it was widely believed "by the common people" that his painful terminal illness was a punishment from God for taking away 11 days of their lives.[160] It surely reveals that although Bradley may have been a man who had dedicated his life to rational pursuits, and achieved much to propagate the imperatives of Newtonian astronomy, the larger part of the population of England was untouched by the new knowledge.

**Bradley and the 1761 Transit of Venus.**

During his final years at Greenwich whilst his health and strength was in decline, Bradley began planning an observational programme of the coming transits of the planet Venus, even though he must surely have recognised that there was little or no chance that he would be able to undertake the observations. He had formerly planned observing the phenomenon himself possibly with the assistance of Maskelyne, but as his health and strength declined he must have recognised the impracticality of this and began to support the notion of sending one of his assistants to undertake the task. This phenomenon, when the disk of Venus is observed as it passes over the Sun's disk is not common. It happens in pairs of events, each about eight years apart, separated by over a century. During the 18$^{th}$ century it was observed in 1761 and 1769, and it was not observed again until 1874 and 1882. The next pair is predicted for 2004 and 2012.

It was Halley who first indicated the importance of the expected transits of Venus across the solar disk as a means of determining the solar parallax and thus determining the mean distance of the Earth from the Sun,[161] now termed the "astronomical unit". He suggested that astronomers observe the moments of inner contact during the planetary ingress and egress from the solar disk. By careful timing of these events from different parts of the Earth's surface Halley surmised that the differing time periods that the planet spent crossing the solar disk could lead to a very accurate calibration of the solar parallax, and thus determine the scale of the solar system and perhaps if annual stellar parallax ever be determined,

the scale also of the entire universe. Halley's method was modified after his demise in 1742 by Joseph-Nicolas De Lisle, for he determined that observations of ingress or egress would be sufficient, and both would not be required.[162]

The transit of Venus of 1761 was the last major astronomical phenomenon to which Bradley was able to extend his attention. Lacaille had earlier expressed his doubts as to whether it was possible to observe the phenomenon with the degree of precision that was required according to the method determined by Halley, in a letter to Bradley written in Paris on the 2nd of May 1748.[163] Transits of the planet Mercury had been observed several times prior to the transits of Venus.[164] These observations helped astronomers become aware of some of the difficulties connected with the measurement of planetary transits. Aware as he was of Bradley's observational abilities, De Lisle attempted to gain his interest in the determination of the solar parallax through the measurement of planetary transits of the solar disk. He wrote on the 15th July 1752,[165] but the letter was misdirected, and only reached Bradley after a delay.[166] Following this De Lisle and Bradley regularly exchanged letters.[167]

De Lisle also attempted to elicit the interest and the support of the Royal Society, and in a letter addressed to Thomas Birch stated that the 1761 and 1769 transits of Venus would be observable in the English (sic) colonies, and that precise observation of the phenomenon there, in conjunction with those which would be obtained in the East Indies would be of great consequence in the determination of the solar parallax. He implored Birch, "that it would be to the credit of the Royal Society to arrange as exact observations as possible in the English (sic) colonies."[168] De Lisle's urgings were needless, for English astronomers were just as interested in the coming transits of Venus as their French counterparts, with increasing interest in Halley's paper on the subject.[169] In spite of the difficulties created by the declaration of war by Great Britain against France on the 17th of May, 1756,[170] communications between British and French astronomers were maintained, albeit with difficulty. De Lisle despatched an important Memoir on the coming transit of Venus three days before it was officially presented to the Académie Royale des Sciences on the 30th of April,

1760.[171] This was presented to the Royal Society on the 5th of June, 1760.[172] De Lisle recommended sending instruments to St. Helena in the South Atlantic and to Bencoolen on Sumatra, with Batavia in Java as the back-up for this site if there were any unforeseen difficulties in Sumatra. Whilst De Lisle's communication was still in transit the Council, on the 22nd May, 1760, with Lord Cavendish in the chair[173] in the absence of Lord Macclesfield, the President, considered the problem of sending "proper persons to proper places, to observe the approaching Transit of Venus over the Sun the Several Instruments that will be necessary for the work; and the Expenses which are likely to attend it."[174]

As a Council member Bradley was present at the deliberations of the 5th of June, the day De Lisle's letter was received, and was referred to in order to determine which instruments were required to equip such an expedition. Bradley recommended a "Reflecting Telescope of Two Foot with Dolland's Micrometer, Mr. Dolland's Refracting Telescope of Ten Feet: a Quadrant of the radius of Eight Inches; and a Clock or time piece."[175] It cannot be denied that the relative cost of scientific and astronomical instruments were high in the mid-18th century, for they placed the very highest demands on the skill and technique of the very finest instrument makers. The degree of precision and accuracy needed for the determination of terrestrial nutation, the figure of the earth, the determination of longitude or the determination of the solar parallax was also impelled by the ruling imperatives of the finest observers, including Bradley and Lacaille. The equipping of an expedition to the South Atlantic and the East Indies with the appropriate instruments, especially in a time of war, was expensive. The instruments were provided and two expeditions were readily equipped. Nevil Maskelyne was chosen to travel to St. Helena assisted by Robert Waddington. The other expedition, on the recommendation of Bradley, employed his assistant of several years Charles Mason, accompanied by Jeremiah Dixon.[176] Unfortunately Mason and Dixon were unable to get to Bencoolen. Given passage in the HMS Sea Horse, they were intercepted a few leagues off the Start Head by a French frigate. Although they beat off the attack the sloop suffered so much damage that it had to return to Plymouth for a refit.[177]

Bradley, in one of the last memoranda he wrote before the onset of his terminal illness made all work impossible, wrote to Mason to give explicit instructions about how to undertake his observations at Bencoolen. He wrote.

> If the external and internal contacts of Venus and the sun's limbs happen to be well observed, there will be less need of measuring the distance with the micrometer between their limbs afterwards; however, if time will permit, that sort of observation should be frequently repeated as soon as can be after the total ingress,[178] and again near 3h. after the ingress, when Venus will be nearest to the sun's centre, and when the distance of the nearest limbs should be frequently taken, and the time noted. The diameter of Venus may be measured by the micrometer at any intermediate time, when the forementioned observations become less useful.[179]

This gives an insight into the thoroughness of Bradley's continuing practice. Later in the memorandum addressed to Mason a further note is included on the observational methodology that Bradley advises his assistant to apply. It is based on a lifetime of experience. It may seem obvious advice but could so easily be overseen by the unwary. Bradley added,

> As the exact time of the first contact of the limbs of the sun and Venus is somewhat uncertain, you must begin to look for it some time before the earliest computation gives it, which is about 7' after nine of the clock in the morning of the sixth day of June; and if both telescopes can be made use of, and either observer should have occasion to alter much the position of his telescope, the other ought to avoid altering his at the same time, that one at least may always be looking on that part of the sun's limb where the first contact will happen. By computation the first contact will be about 48° from the apparent lower limb on the right hand, or near the middle point, between the vertical circle passing through the sun's centre, and an horizontal line passing through the same.[180]

After being thwarted from reaching Bencoolen, which was also to be captured by the French during the period of the transit, Mason and Dixon arrived

at the Cape of Good Hope on the 27th of April, 1761 and stayed there in order to prepare for the transit, rather than continue to the East Indies across thousands of miles of hazardous ocean during the cyclone season with French warships loose on the main trading routes. They constructed a temporary observatory, mounted the quadrant and began recording temperatures three times daily, morning, noon, and night. They set their astronomical clock going on the 4th of May 1761 and after careful observations revealed that the clock was losing 2 minutes and 17 seconds a day.[181] They were able to fix their exact location by observing various astronomical phenomena, including an eclipse of the moon on the 18th of May, as well as repeated observations of eclipses of Jupiter's satellites and occultations of stars by the moon. All of these activities, as well as the observations of Venus itself were clearly set out by Bradley. Mason and Dixon followed all of Bradley's instructions, including measuring the temperature of the clock case. The day of the transit on the 6th of June, the visibility was clear and Mason and Dixon independently recorded the moments of ingress and egress. Dixon measured the moment of ingress 4 seconds earlier than Mason, and the moment of egress 2 seconds earlier.[182] The latitude and longitude of their observatory was fixed so accurately that when the first issue of the *Nautical Almanac* was published in 1767, Nevil Maskelyne was able to write, "it is probable that the situation of few places is better determined."[183]

In contrast to the success of the programme conducted by Mason and Dixon under Bradley's instructions, the ambitious programme of observation devised by Maskelyne and Waddington fared badly. Cloudy weather made any useful observations in the month prior to the transit impossible, and on the day of the transit on the 6th June, they were only able to gain occasional sights of Venus against the sun's disk, when they measured its position on the solar image and recorded the time.[184] The ambitious attempt to measure the parallax of *Sirius* failed when it was recognised that the hastily manufactured zenith sector was defective. Other facets of the programme during the stay in St. Helena were more successful, but the work on the transit of Venus was admitted to be a failure.[185] The work on the transit of Venus in 1761 exemplifies the thoroughness and rigour

of Bradley's methodology. The work performed by Mason and Dixon at the Cape was in every way indicative of a practice that had been perfected at the Royal Greenwich Observatory under his administration.

In the two decades that he held the office of Astronomer Royal Bradley transformed Greenwich into a "centre of calculation"[186] and made it the centre of a British "methodology". Before he took over the Royal Observatory he regarded it as the pinnacle of his ambitions for he knew that it would be the place where he would be able to take his skills and hone them to their limits. His practice found full reign as he focused on the problems of constant rectification and observational reduction as he coped with inadequate instruments, which though repaired and modified failed to meet the demands that he placed upon them. In his work at Wanstead, Oxford, Shirburne and Greenwich he transformed the science of stellar astrometry, developing a rigorous methodology that emphasised exactitude. The precision and accuracy of his observations were underlined by a practice that emphasised constant correction and rectification and an evaluation of all the sources of possible error. In applying this virtuous and "curious"[187] methodology Bradley isolated various phenomena. In perfecting his practice with the acquisition of a range of unprecedented skills his incumbency of the Royal Observatory witnessed its transformation from a centre of mediocrity into the finest centre of excellence in Europe.

His appointment as Astronomer Royal gave vent to the full range of his rigorous tendencies. He trained assistants into the same exacting methods that he had formerly perfected at other centres and soon his hard won skills were standardised and routinised. Training assistants to such exacting standards of accuracy and precision demanded a clear recognition that many of the skills were acquired intuitively and tacitly. Observation is an art that every practising astronomer recognises involves many unspoken skills which can be demonstrated or shown, but rarely explained explicitly. In recalling his own heuristic as a young man he was able to transform it and create a tradition of observation and calculation at Greenwich which gave it pre-eminence far beyond his own incumbency. It was a centre that other centres in Europe sought to emulate.

Greenwich under Bradley became a centre in which precision and accuracy became routine. His assistants were such a facility that even when he was absent or ill the Royal Observatory's routines continued almost without change. The work of his successors exemplified the rigour of Bradley's own search for error reducing methods and practices. His mark on the practices of the work at Greenwich remained well into the 19th century.

**The Publication of the Greenwich Positional Observations.**

In 1765 the Board of Longitude asked the Secretary of State to request possession of Bradley's observations at the Royal Observatory, but due to complications connected with the fact that their inheritor Susannah Bradley was not of legal age,[188] it was decided to wait until January 1767 when she would gain her majority, when she would be requested to hand them over.[189] When Susannah became of age, she handed over possession of her father's papers to her uncle, the Rev. Samuel Peach who refused to give them up, believing that he possessed an interest, and sought some form of financial recompense. Following his demise[190] the observations were inherited by his son John who also refused to relinquish them to the Board of Longitude. In 1772, the year that his father died, John Peach wrote to the Board of Admiralty requesting that the law-suit be abandoned but the Commissioners decided to continue. John Peach also thought he possessed property in the papers and would not abandon them without compensation. In 1775, when an action was to be instituted against Peach, the papers passed into the possession of his younger brother, the Rev. Samuel Peach who was now married to Susannah. Samuel Peach (Junior) then presented all of Bradley's papers to Lord North in his position as the Chancellor of the University of Oxford in 1776. He in turn, being entirely unaware of the legal dispute, then passed the papers to the University on condition that the important observations, including the Greenwich observations from 1750, should be printed and published by the Clarendon Press. It was subsequently decided that the Crown no longer had right to these papers and the legal action was finally abandoned since both the Royal Society and the Board of Longitude believed that the papers were in the hands of

the printers. Over fifteen years later during 1791 concerns were being raised about the delay in publication. This prompted Sir Joseph Banks, President of the Royal Society and a leading member of the Board of Longitude to write to the Vice-Chancellor of the University of Oxford.[191]

The Rev. Samuel Forster, Registrar to the Delegates of the Clarendon Press replied to this enquiry in a letter dated 12th June, 1791[192]

> Sir,
>
> I received your letter; and duly communicated it to the Delegates of the Oxford Press at their usual meeting immediately before the expiration of Term. From whom I am instructed to say that they are not conscious of any unnecessary delay respecting the publication of Dr. Bradley's Papers: But that their Astronomical Professor into whose hands they were originally put,[193] has given every requisite attention to them, and made as much progress as his health and the time would admit of – That a considerable part of them is already printed, and in the most handsome and useful manner – That the only impediment that they have laboured under, has been the unforeseen Illness of Dr. Hornsby, which (tho' not so severe as wholly to interrupt his Academical Lectures) has for some time past retarded him in the prosecution of the Work …

This letter, it must be remembered, was addressed almost thirty years after Bradley's demise, and fifteen years since Lord North had given instructions that they be passed to the press for publication. When these observations were finally published in 1798[194] and 1805[195] they were not reduced, even though all of the information required to reduce them was included in the registers. The task fell to Bessel who in 1807 began work reducing the observations made from 1750 to 1762. He completed the task with the publication of the *Fundamenta Astronomiæ* in 1818.[196] Bessel's work revealed the "incomparable" practice not merely of an outstanding astronomer, but the way that he had initiated a corporate practice in which these skills were constantly transmitted and transformed.

[1] Rigaud, "Memoirs", p. li.
[2] Ibid. p. xliv.
[3] Ibid. p. xliv.
[4] Ibid. p. xlv.
[5] Francis Baily, *An Account of the Rev. John Flamsteed, the First Astronomer Royal; compiled from his Manuscripts and other authentic Documents, never before published,* London, 1835. p. 343.
[6] Derek Howse, *Greenwich Observatory: One of three volumes by different authors telling the story of Britain's oldest scientific institution: The Royal Observatory at Greenwich and Herstmonceux, 1675 – 1975: Volume 3: The Buildings and Instruments.* Taylor and Francis, 1975. p. 6.
[7] Ibid. p. 6.
[8] Ibid. pp. 21 – 24. This followed John Bird's construction of a copy in 1750 for Bradley, though this time in brass. By this time the reputation of Bradley and Graham for accuracy and precision, together with a respect for Bird's immaculate craftsmanship as the leading hand-graduator of the 18th century created a growing market for English instrument makers throughout Europe. Graham's 8-foot quadrant was the instrument that did so much to establish the reputation of English instrument makers.
[9] Maskelyne, "Astronomical Observations, 1766 – 1774", London, 1776, Vol. I, Section 1.
[10] Howse, "Greenwich Observatory" Vol. 3, pp. 22 – 23.
[11] Cambridge University Library, Royal Greenwich Observatory Archives, Document RGO 3/31, *Observations: Transits of major stars including adjustments to the telescope and observational method, 1743*, fol. 1.
[12] Ibid. fol. 1.
[13] Kathryn M. Olesko, "The Meaning of Precision: The Exact Sensibility in Early Nineteenth-Century Germany" in M. Norton Wise (ed), *The Values of Precision*, Princeton University Press, 1995, p. 104.
[14] Ibid. p. 122.
[15] Ibid. p. 122.
[16] Bessel, *Fundamenta Astronomiæ,* Königsberg, 1818.
[17] Simon Schaffer, "Accurate Measurement Is an English Science" in M. Norton Wise (ed.) *The Values of Precision,* Princeton University Press, Princeton, 1995, pp. 135 – 172.
[18] Olesko quotes Bessel, "Ueber Mass und Gewicht im Allgemeinen und das Preussische Längenmass im Besonderen", in *Populäre Vorlesungen*, pp. 269 – 325, on pp. 269 – 270.
[19] Rigaud, "Memoirs", p. xlv.
[20] His office was officially then (as it remains) First Lord of the Treasury.
[21] Not to be confused with the later Sir William Jones, the poet and notable orientalist, who was also one of Sir Joseph Banks' correspondents.
[22] The Chancellor at this juncture was Philip Yorke Earl of Hardwicke. He held the office from 21st February 1737 to 20th November 1756.
[23] The Archbishop of Canterbury was Dr. John Potter, who held the office from 1737 to 1747.
[24] George Parker 2nd Earl of Macclesfield to Sir William Jones, 14th January, 1741 – 42. Quoted in full in Rigaud, "Memoirs", p. xlvi.
[25] Rigaud, "Memoirs", pp. xlvii – xlviii.
[26] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/43. *Miscellaneous Observations and Calculations 1742 – 1757.* On a loose sheet of paper in a box.
[27] A central plank of Walpole's foreign policy had been the maintenance of peace. Difficulties with Spain over the West Indies trade forced him to declare war when his hand was forced by a majority in Parliament. Walpole admitted his opposition to the war to his cabinet colleagues and his views eventually became known to the rank and file. The so-called "War of Jenkins' Ear" was indecisive and opposition within Parliament grew, though he succeeded in winning the 1741 general election. However, many Whig politicians and several independent members mistrusted his capacity to direct the war with the vigour considered necessary and his resignation was eventually forced over a minor issue. After his resignation he was created 1st Earl of Orford by George II and was granted an annual pension of £4,000. An attempt to impeach him over his

ministry failed. In the final years of his life he attempted all in his power to have Carteret dismissed (who succeeded to the office of Secretary of State on his own dismissal). He sought the promotion of Henry Pelham his protégé who was the leader of the Walpole Whigs. Pelham became Prime Minister in 1743. Pelham dismissed Carteret (who was earlier created Earl Granville) in 1744 when the secretary of state sought to increase Britain's involvement with the War of the Austrian Succession in conflict with France and Prussia. Both Walpole and Pelham remained close confidents of George II. Pelham's close relationship with the king was to have consequences when Bradley sought the reformation of the Royal Observatory.

[28]Rigaud, "Memoirs", p. xlviii, citing the Supplement to the Biographical Dictionary, p. 56.

[29] Ibid. p. xlviii, Rigaud wrote, "It appears, therefore, that one of the first things which Walpole looked to, as soon as he had determined to retire, was to make use of the power which yet remained in his hands, to secure the office of astronomer royal for the man who of all others was the most deserving of it."

[30] Ibid. p. xlix. Rigaud here suggested that Macclesfield's assertion that "the finest instrument in the world" was Graham's 8 foot quadrant. However Macclesfield must have been acutely aware of the state of that instrument, for Bradley often attended to its faults and insufficiencies prior to his appointment as Astronomer Royal. Macclesfield may have been speaking more tangentially, referring to the Royal Observatory.

[31] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/31, *A Miscellaneous Collection of Handwritten Notes.* doc. 1, fol. 1v.

[32] Ibid. doc. 5. fol. 1r.

[33] These arrangements further underline the view that Halley was not as exacting an observer as Bradley. It also reveals why Halley considered Bradley's observation and measurement of motions as small as half a second was unsupportable. Bradley's practice had developed various observational techniques that used refinements such as the screw micrometer made available by Graham's consummate craftsmanship.

[34] This inadequacy was clearly demonstrated by Halley's observation of the star *Capella* on the 2nd September 1727 using the zenith sector that Graham constructed and erected at Elizabeth's Pound's house for Bradley. He possessed an inability to bisect the star as it passed through the centre wire and this led to an observation that was inaccurate. The fluttering of the star as it passed through the observational field of the object glass induced further inaccuracy. Halley's lack of observational proficiency revealed that such a fine instrument as Graham's zenith sector, which Maskelyne later confirmed was capable of measurements as small as half a second, was of little use in the hands of an inadequate practitioner. It also reveals that Bradley's great achievements were not merely a product of his possession of some of the finest instruments of his age. Such precision in construction demanded equal precision of observational technique.

[35] Bradley was a witness of Halley's Will and joint Executor.

[36] Bradley possessed two fine mentors. Halley imbued Bradley with the mathematical skills he required and Pound inculcated the observational disciplines which were such an important part of his armoury of skills.

[37] It was a practice that had been constructed upon Bradley's development of the imperatives of Newtonian experimental philosophy with its emphasis not just on accuracy and precision, but also on a meticulous attention to detail. That Bradley was a consummately skilled observer cannot be doubted, but the significance of his practice lay both in its contexts, embedded as it was in a reiterative methodology and the demands of an acquisitive society which sought the application of new knowledge for practical or commercial ends, but also in the way he was able to analyse his practice and pass it on to his assistants, creating a tradition of accuracy and precision, which not only transformed the work of the Royal Greenwich Observatory, but through the agency of his scientific prestige the practice of astronomy throughout Europe.

[38] This indicated that some time had passed since Halley had last used the Transit Instrument.

[39] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/31, *A Miscellaneous Collection of Handwritten Notes.* doc. 5. fol. 2r – v.

[40] Ibid. doc. 5. fol. 2v.

[41] Ibid. doc. 5. fol. 2v.

[42] Ibid. doc. 5. fol. 2v.

[43] Bradley, *State of the Instruments at the Greenwich Observatory when Dr. Bradley became Astronomer Royal,* in the "Miscellaneous Works", p.382.
[44] Ibid. p. 382.
[45] Bodleian Library, Department of Western Manuscripts, Bradley MS 31, *Miscellaneous Astronomical Observations at Greenwich with Two Transit Instruments from 25th July, 1742 to January 1742/43. At folio 38, Lord Macclesfield's Similar Observations at Shirburne, September to December 1742 and March 1743.*
[46] Rigaud, "Memoirs", p. lii.
[47] Ibid. p. lii.
[48] Ibid. p. lii.
[49] Bodleian Library, Bradley MS 31, *James Bradley's Observations at Greenwich with two Transit Instruments from 25th July, 1742 to January 1742/43. Lord Macclesfield's Similar Observations at Shirburne, September to December 1742, and March 1743.* fol. 28r.
[50] Bradley was assisted by his nephew John Bradley, who was to remain at Greenwich from 1742 to 1756.
[51] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/1, *Observations: Transits of major stars including adjustments to the telescope and observational method, 1743.*
[52] Ibid. fol. 108r – v.
[53] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/8, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1743 – 1744.*
[54] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/31, *A Miscellaneous Collection of Handwritten Notes.* doc. 6. fol. 1r.
[55] Ibid. doc. 6. fol. 2v.
[56] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/2, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1744 – 1746.*
[57] Rigaud, "Memoirs", p. lv.
[58] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/1, *Observations: Transits of major stars including adjustments to the telescope and observational method, 1743.*
[59] Maskelyne, "Astronomical Observations, 1766 – 1774", Vol. III, Transit Observations, p. 339.
[60] Rigaud, "Memoirs", p. lv.
[61] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/2, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1744 – 1746.*
[62] Rigaud, "Memoirs", p. lv.
[63] Ibid. p. lv.
[64] By "brighter stars" are all of the stars down to about magnitude 7. The faintest stars normally visible to the naked eye in good seeing conditions are about magnitude 6. Bradley's survey included all of the stars that would be visible in a mariner's sextant in good seeing conditions.
[65] Bradley's survey included all of the stars down to the 7th magnitude, including all of the stars observable by the naked eye which generally includes stars down to the 6th magnitude.
[66] The reasons for this are possibly connected with Thomas Hornsby's work on Bradley's Greenwich Observations from 1750 to 1756. Rigaud refers to the difficulties he had retrieving many of Bradley's papers from his predecessor as Savilian Professor of Astronomy. Hornsby did not always treat Bradley's papers with the respect they deserved. Rigaud with Hornsby's permission went through his papers and gathered as many of Bradley's papers as he locate. See Rigaud, "Memoirs", p. xxxviii passim.
[67] Maskelyne succeeded Nathaniel Bliss as the Astronomer Royal in 1765. In 1763 he had published the *British Mariner's Guide*. This had been prepared during his voyage and sojourn on St Helena, where he attempted to observe the transit of Venus in 1761. He attempted to determine the longitude by observations of the Moon's position and this method was published in the *Guide*.
[68] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/1 to 3/14.

[69] Ibid. RGO 3/15 to 3/29. These are "fair copies" of RGO 3/1 to 3/14.
[70] Bodleian Library, Bradley MS. 32, *James Bradley's Observations at Greenwich with Two Transit Instruments from 25th July, 1742 to January 1742/43.* At fol. 38r to fol. 43v, *Lord Macclesfield's Similar Observations at Shirburne, September to December 1742, and March 1743.* These latter observations are copies of the Shirburne registers.
[71] Bodleian Library, Bradley MS. 28* *Calculations concerning the Precession of the Equinoxes. Absolute and Relative Motion of Certain Fixed Stars compared with Flamsteed's Observations,* Bradley MS 29 *Observations and Calculations by James Bradley of the Fixed Stars,* Bradley MS 33 *Miscellaneous Astronomical Observations and Computations,* Bradley MS 42 *Observations and calculations on refractions. Observed refractions,* and Bradley MS 43 *Miscellaneous Astronomical Tables by James Bradley. Tables of the Sun's declination. Mean precession in longitude, etc.*
[72] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/1, *Observations: transits of major stars; including adjustments to the telescope and observational method, 1743.*
[73] Bradley spent a large part of the second half of 1742 training his nephew in the skills required in order that he might be safely entrusted with the continuation of observations during the periods he was residing in Oxford. Until 1749 Bradley maintained his teaching schedule of presenting three courses a year in experimental philosophy and his lectures in astronomy.
[74] Bodleian Library, Bradley MS. 32, *James Bradley's Observations at Greenwich with Two Transit Instruments from 25th July, 1742 to January 1742/43.* At fol. 38r to fol. 43v, *Lord Macclesfield's Similar Observations at Shirburne, September to December 1742, and March 1743.*
[75] Rigaud, "Memoirs", p. liv.
[76] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/8, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1743 – 1744.*
[77] Rigaud, "Memoirs", p. liv.
[78] Bliss's observations were addressed to Bradley.
[79] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/2, *Observations: transits of major stars; including adjustments to the telescope and observational method, 1744 to 1746.*
[80] Ibid. RGO 3/9, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1744 – 1747.*
[81] Ibid. RGO 3/32 *Collimation errors of the Quadrant, 1742 – 1752,* doc. 12.
[82] Ibid. RGO 3/32 *Errors of the planes of the Quadrant at various times between 1743 – 1745,* doc. 14.
[83] Ibid. RGO 3/32 *Various data relating to adjustments taken on the Quadrant,* doc. 16.
[84] Ibid. RGO 3/32 *Errors and rates of clocks, 1743 – 1748,* doc. 15.
[85] It is very difficult to ascertain the exact dates when the observatory was constructed. Lord Macclesfield did not preserve his correspondence with Bradley and very little of the converse correspondence exists in Bradley's papers at Oxford or Greenwich.
[86] Ibid. RGO 3/10, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1747 – 1753.*
[87] Ibid. RGO 3/3, *Observations: transits of major stars; including adjustments to the telescope and observational method, 1746 – 1749.*
[88] Ibid. RGO 3/4, *Observations: transits of major stars; including adjustments to the telescope and observational method, 1749 – 1750.*
[89] Ibid. RGO 3/10, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1747 – 1753.* The inside of the front cover of this register details a record of repairs and alterations to the Graham quadrant.
[90] Bodleian Library, Bradley MS 31, *James Bradley's Observations at Greenwich with two Transit Instruments from 25th July, 1742 to January 1742/43.* At fol. 38, *Lord Macclesfield's Similar Observations at Shirburne, September to December 1742, and March 1743.*

[91] Bradley observed 3,222 stars based on Flamsteed's Catalogue down to the 7th magnitude, ensuring that every star observable with the naked eye had its position precisely and accurately located for use with a mariner's sextant. This was an important precondition, together with an adequate Lunar theory, for determining the longitude at sea.
[92] Bradley, "Experiments to determine the Length of the Pendulum vibrating seconds at Greenwich", in the "Miscellaneous Works", p. 385.
[93] Rigaud, "Memoirs", p. lvi. Rigaud mentions that nothing was known of the standards by which these results were obtained, nor the temperature at which these results were obtained. He proffers the view that they formed merely a first order approximation, by which Bradley familiarised himself to the problem.
[94] Royal Society, *Minutes of the Council of the Royal Society,* 1749.
[95] Rigaud, "Memoirs", p. lxxiv.
[96] Ibid. p. lxxiv.
[97] Ibid. p. lxxiv.
[98] This sum includes the £3 13s 6d he paid to Sisson for the micrometer for the Graham mural quadrant on the 1st October 1745. It is recorded as a payment for £3 3s 6d but Rigaud states that 10 shillings be added to this sum in order to balance the accounts.
[99] Rigaud, "Memoirs", p. lxxiv.
[100] Maskelyne, "Astronomical Observations, 1766 – 1774, Transit Observations", Vol. II, p. 160.
[101] This is the Transit House of the Old Royal Observatory, through which the internationally recognised Greenwich Meridian passes. The present meridian dates from the 19th century and marks the position of George Biddell Airy's transit instrument. Bradley's meridian runs through the centre of the transit house which he constructed, and is some 18 feet to the west of the present meridian. Interestingly, the maps of the Ordnance Survey are still based on the Bradley meridian.
[102] Lord Anson led a naval expedition which was sanctioned to capture the annual Spanish Acapulco galleon. Whilst a military success amounting to a prize of over £250,000, the voyage was marked by great suffering induced by the failure to determine the longitude when becalmed in the doldrums when the location of islands necessary to re-provision became impossible. Many men died from scurvy. The expedition lasted from 1740 to 1744. See Leo Heaps, *Log of the Centurion*, London 1974, an account of the voyage based on the papers of Captain Philip Saumarez, captain of the HMS Centurion, Lord Anson's flagship.
[103] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/43, *Miscellaneous Observations,* doc. 2.
[104] Bradley, *Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII.* Vol. I, MDCCL to MDCCLV, *Observations with Transit and Mural Quadrant from 1750 to 1756,* ed. Thomas Hornsby, Oxford 1798. Vol. II, MDCCLVI to MDCCLXII, *Observations with Transit and Mural Quadrant from 1756 to 1762,* ed. Abram Robertson, Oxford 1805. "Observations with the Zenith Sector", Vol. I, p. 91.
[105] George Graham died on 20th November 1751.
[106] Howse, "Greenwich Observatory: Volume 3: The Buildings and Instruments", p. 25.
[107] John Bird, *The Method of Constructing Mural Quadrants,* London, 1768, pp. 23 – 24.
[108] Howse, "Greenwich Observatory: Volume 3: The Buildings and Instruments", p. 25.
[109] Rigaud, "Memoirs", p. lxxvii.
[110] John Bird, "The Method of Constructing Mural Quadrants", p. 1.
[111] John Bird, *The Method of dividing Astronomical Instruments,* London, 1767, p. 5.
[112] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII." Vol. I, MDCCL to MDCCLV, "Observations with Transit and Mural Quadrant from 1750 to 1756," ed. Thomas Hornsby, Oxford 1798.Vol. II, MDCCLVI to MDCCLXII, "Observations with Transit and Mural Quadrant from 1756 to 1762", ed. Abram Robertson, Oxford 1805. "Zenith Distances to the Southward" Vol. I. ed. p. 57.
[113] Bradley, "Extracts from Dr. Maskelyne's Observations on the Latitude and Longitude of the Royal Observatory at Greenwich", in the "Miscellaneous Works", p. 78.
[114] Howse, "Greenwich Observatory: Volume 3: The Buildings and Instruments", p. 34.
[115] Ibid. p.35.

[116] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII." Vol. I, MDCCL to MDCCLV, "Observations with Transit and Mural Quadrant from 1750 to 1756," ed. Thomas Hornsby, Oxford 1798.Vol. II, MDCCLVI to MDCCLXII. "Observations with Transit and Mural Quadrant from 1756 to 1762," ed. Abram Robertson, Oxford 1805, Vol. II, Preface, pp. iii – v.

[117] Rigaud, "Memoirs", p. lxxix.

[118] Royal Society, *Journals of the Royal Society,* 30th April, 1752.

[119] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3, and the registers designated as RGO 5, RGO 6, and RGO 7 for observations made with the new transit instrument, and RGO 11, RGO 13 and RGO 14 for the observations made with the new 8 foot mural quadrant.

[120] It is likely that James Bradley was introduced to astronomy in or about 1711 when he first lived with his uncle, James Pound. The earliest recorded observations date from 1715 and these reveal a degree of skill and adeptness that had evidently been acquired well before these results were recorded.

[121] All the rectifications and calibrated instrumental errors were recorded. It was an essential aspect of Bradley's technique that he repeatedly checked or rectified his instruments, often in the midst of a run of observations. It is due to such a rigorous application of his observational techniques, that his observations can be reduced with such confidence and precision. Later Bessel was able to distinguish the observational patterns of Bradley's technique from those of his various assistants and recognised small but perceptible variations which he identified as the "personal equation". See Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation." in *Science in Context,* Vol. 2.1 (1988), pp. 115 – 145.

[122] This was certainly well established by the time Bradley had helped construct Macclesfield's observatory at Shirburne Castle. The observations made there by Bradley or Macclesfield, or by the servants of the household, were all accompanied by barometric and thermometric measurements. These readings were made in order to allow for possible variations in the refractive index of the atmosphere, a possible cause of error when making observations where accuracy and precision were so essential.

[123] It must be regarded as an aspect of Bradley's rigorous practice that he carefully analysed his methods and then passed these on to his assistants so that they were capable of working to the same precise and accurate standards. It began a tradition for accuracy and precision at Greenwich that was maintained after Bradley's death, and which was soon adopted in every leading observatory in Europe. During Bradley's lifetime his methods were developed by many leading astronomers including his long standing admirer and correspondent Lacaille.

[124]Bodleian Library, Bradley MS.45, John Bradley to James Bradley, *Correspondence to James Bradley and his assistants,* Letter dated Greenwich April 9th, 1751/52 (OS). It accompanied a letter written in French addressed to Bradley from the Russian astronomer A. N. Grischow in St. Petersburg dated 2nd April 1752. (NS). See also James Bradley, *Miscellaneous Works and Correspondence of James Bradley,* pp. 466 – 470.

[125] In the letter from John Bradley to James Bradley dated 9th April, 1751/52 (OS) James Bradley crossed out 21.1" and substituted 4.3" after making his own assessments of the incorporated data.

[126] According to the letter sent by Maskelyne to the Board of Longitude on 22nd December 1791 some of Bradley's papers relating to his Greenwich observations had been in Thomas Hornsby's possession from between 1776 and 1791. See Cambridge University Library, Royal Greenwich Observatory Archives, RGO MSS. 532, *Board of Longitude,* Vol. 4, p.268. Also see Rigaud's account in the *Memoirs of Bradley* p. xc, passim. It is possible to infer that many of these papers were still in his possession as late as 1828 to 1830.

[127] The Peach family was in a legal dispute with the Board of Admiralty over the custody of Bradley's papers from 1762 to 1776.

[128] These are the Royal Greenwich Observatory Archives at Cambridge University Library and the Miscellaneous Papers of James Bradley at the Bodleian Library.

[129] I have searched for any archives of John Bradley at the Royal Naval Museum at Portsmouth.

[130] At the Hampshire County Records Office in Southampton where the Molyneux Archive is housed.

[131] At the British Library, the Public Records Office, the Science Museum Library and the National Maritime Museum.
[132] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/7, *Observations: transits of major stars; including adjustments to the telescope and observational method, 1758 – 1762.*
[133] Ibid. RGO 13 and RGO 3/14, *Observations, using the new quadrant, of major stars, including adjustments to the telescope and observational method. 1758 – 1762 and 1762 – 1765.*
[134] Bodleian Library, Bradley MS. 32, *Miscellaneous Astronomical Observations by James Bradley,* fol. 72r.
[135] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII". Vol. I, MDCCL to MDCCLV, "Observations with Transit and Mural Quadrant from 1750 to 1756", ed. Thomas Hornsby, Oxford 1798, Vol. II, MDCCLVI to MDCCLXII, "Observations with Transit and Mural Quadrant from 1756 to 1762", ed. Abram Robertson, Oxford 1805.
[136] Charles Mason to Thomas Hornsby, 27th July 1774. This letter is extensively quoted by Rigaud in his *Memoirs of Bradley,* p. lxxxix, p. xcii, and passim.
[137] Rigaud, "Memoirs", pp. xcii to xciii.
[138] In fact Mason and Dixon did not reach the East Indies and instead they observed the transit at Cape Town. Dixon was an amateur astronomer from County Durham who after his association with Mason in Cape Town and in North America returned to County Durham where he died in 1777.
[139] Nathaniel Bliss, who was Bradley's immediate successor to the post of Astronomer Royal, was a distant cousin of his. In Bliss's correspondence with Bradley he almost invariably signed himself as "Your most affectionate brother".
[140] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/10, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1747 – 1753* and RGO 3/11, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1750 – 1757.* Separated into two parts: the observations from 1750 to 1753 were taken on the New Quadrant, those from 1753 to 1757 on the Old Quadrant.
[141] Ibid. RGO 3/5, *Observations: transits of major stars; including adjustments to the telescope and observational method, 1750 – 1755.*
[142] Ibid. RGO 3/11, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.* Separated into two parts: the observations from 1750 to 1753 were taken on the New Quadrant, those from 1753 to 1757 on the Old Quadrant. Working Copy, 1750 – 1757.
[143] Ibid. RGO 3/5 and 3/6, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational methods, 1755 – 1758* and *1758 – 1762.*
[144] It seems that until his health broke Bradley had every intention of undertaking these observations with the assistance of Maskelyne.
[145] Bradley, "Miscellaneous Astronomical Observations" in "Miscellaneous Works", p.350.
[146] Harry Woolf, *The Transits of Venus: A Study of Eighteenth-Century Science,* Princeton, 1959.
[147] The term "magnus orbis" was the current term used to describe the mean diameter of the Earth's orbit around the Sun. It was superseded by the modern term "astronomical unit" which was the mean radius of the Earth's orbit.
[148] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/43, *Miscellaneous Observations: Transit observations in 1754* "with later annotations signed by John Mason". Box 5, doc. p. xvii.
[149] The earliest observations in John Mason's hand date from October 1756. The last entries in John Bradley's hand date from September 1756. The earliest observations I have been able to trace in Green's hand is November 1760. Bliss's handwriting occasionally appears during the 1740s along with Gael Morris and J. Nevis, although neither of the last two figures were ever his assistants. It appears that the annual stipend for the assistants amounted to £26. When Mason wrote to Hornsby in connection with the possibility of reducing Bradley's observations he alluded to this sum.

[150] Cambridge University Library, Royal Greenwich Observatory Archives, RGO 3/13, *Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method, 1762 – 1765.*
[151] Officially in response to Lord Chesterfield's motion though as I state in the text it was after Bradley's earlier promptings.
[152] Sweden converted to the Gregorian calendar one year after Great Britain in 1753.
[153] S. P. Rigaud refers to the evidence of Coxe's, *Memoirs of Pelham's Administration,* Vol. II, Letter XXVII.
[154] Rigaud, "Memoirs", p. lxxx.
[155] *Gentleman's Magazine*, Vol. XV, p. 109.
[156] *Hansard's Parliamentary Debates*, Vol. XV, p. 22.
[157] Bradley to Macclesfield, in the "Miscellaneous Works", p. 461.
[158] Rigaud, "Memoirs", p. lxxxii.
[159] Bodleian Library, Bradley MS.41, *The Printed Bill and Act (24 Geo.2, cap. 23: AD1751) for regulating the commencement of the year.* Followed at fol. 26 by some notes and tables on the subject by Bradley.
[160] Rigaud, "Memoirs", p. lxxxi. Rigaud stated, "when Bradley, worn down by his labours in the cause of science, was sinking under the disease which closed his mortal career, many of the common people attributed his sufferings to a judgement from heaven for his having been instrumental in what they considered to be so impious an undertaking."
[161] Halley, "A New Method of Determining the Parallax of the Sun, or his Distance from the Earth", *Philosophical Transactions,* Vol. 29, p. 454.
[162] Harry Woolf, *The Transits of Venus: A Study of Eighteenth-Century Science.* Princeton University Press, 1959. Preface, p.vii.
[163] Lacaille to Bradley, Paris, 2nd May, 1748. In James Bradley, *Miscellaneous Works and Correspondence of James Bradley,* Oxford, 1832, pp. 438 – 442.
[164] The transits of Mercury were closely observed on 11th November 1736, 2nd May 1740, 5th November 1743, 6th May 1753 and 7th November 1756. It was during the earlier transit of the 9th November 1723 that De Lisle first began to develop his method.
[165] De Lisle to Bradley, Paris, 15th July, 1752. In James Bradley, *Miscellaneous Works and Correspondence of James Bradley,* Oxford, 1832, pp. 470 – 471.
[166] On the 22nd August Bradley wrote to De Lisle in reply, "I had not the honour of receiving your letter of July 15th n. s. till very lately; it having been delivered to a gentleman of my name that lives with the Sardinian ambassador in London, who without examining the superscription, had opened it before he perceived the mistake, and having occasion to go into the country for some days left it to be sent to me; but on his return was surprised to find that through his servant's neglect, the letter had been detained from me much longer than it needed to have been. If it had been directed to me at Greenwich, instead of London, the postman could not so easily have made the mistake."
[167] Bradley, "Miscellaneous Works", Joseph-Nicolas De Lisle to James Bradley, 31st January, 1751, pp. 463 – 465. De Lisle to Bradley, 15th July, 1752, pp. 470 – 471. Bradley to De Lisle, 22nd August, 1752, pp. 477 – 478. De Lisle to Bradley, 30th November, 1752, pp. 478 – 483.
[168] L'Observatorie de Paris, B.1, 1-8, (140), No. 23, fol. 1, Nicolas De Lisle to Thomas Birch, 30th November, 1752. Quoted in Harry Woolf, "The Transits of Venus: A Study of Eighteenth-Century Science", p. 47. In the original French he wrote, "de vouloir bien proposer la chose à la Societé Royale et … de vous employer de tout vôtre Credit pour nous procurer de vos Colonies angloises les observations les plus exactes qu'il sera possible."
[169] Halley, *A New Method of Determining the Parallax of the Sun, or his Distance from the Earth,* in the *Philosophical Transactions,* Vol. 29, p. 454. Halley suggested making observations of the transits of Venus at Trondheim in Norway, The Gulf of the Ganges, The Kingdom of Pegu, Fort St. George (Madras), Hudson's Bay, Bencoolen, Pondichery, and Batavia.
[170] The Seven Years War lasted until the signing of the Treaty of Paris in January 1763.
[171] Harry Woolf, "The Transits of Venus: A Study of Eighteenth-Century Science". p. 72.
[172] Ibid. p. 72.

[173] Lord Cavendish had joined Bradley at Wanstead on various occasions through 1727 and 1728 and assisted him in observations of the phenomenon of annual aberration with the zenith sector constructed by Graham.
[174] Royal Society, *Council Minutes of the Royal Society*, Vol. IV, pp. 228 – 229.
[175] Ibid. pp. 229 – 230.
[176] I have been unable to determine why Jeremiah Dixon was chosen. Prior to the expedition he was an amateur astronomer living in County Durham. After working with Mason on the transit of Venus, and partnering him on the survey of the border between Virginia and Pennsylvania, the Mason-Dixon Line, he appears to have returned to his former obscurity in Durham.
[177] Rigaud, "Memoirs", p. c.
[178] This reiterative and active observation of the phenomenon is so characteristic of Bradley's practice, and from this passage it is possible to infer that he took great care when he instructed his assistants to ensure that they conformed to this practice.
[179] Bradley, "Memoranda" in the "Miscellaneous Works", pp. 388 – 390.
[180] Ibid. p.390.
[181] Harry Woolf, "The Transits of Venus", p. 131.
[182] Ibid. p. 132.
[183] T. D. Cope, "The First Scientific Expedition of Charles Mason and Jeremiah Dixon", in *Pennsylvania History,* Vol. XII, January 1945, pp. 3 – 12.
[184] Harry Woolf, "The Transits of Venus", p.132.
[185] Maskelyne, "Account of the Observations made on the Transit of Venus, June 6, 1761, in the island of St. Helena", in *Philosophical Transactions,* 1763, p. 196ff.
[186] Bruno Latour, *Science in Action,* London, 1988. See the final chapter entitled "Centres of Calculation".
[187] ie. precise and accurate.
[188] Susannah Bradley was born in Greenwich in January 1746. She was Bradley's only child.
[189] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII". Vol. I, MDCCL to MDCCLV, "Observations with Transit and Mural Quadrant from 1750 to 1756", ed. Thomas Hornsby, Oxford 1798, Vol. II, MDCCLVI to MDCCLXII, "Observations with Transit and Mural Quadrant from 1756 to 1762", ed. Abram Robertson, Oxford 1805. Vol. I, preface.
[190] The Rev. Samuel Peach died in 1772. Bradley's papers were inherited by his son – in – law, also the Rev. Samuel Peach who held onto them "in order to receive a consideration".
[191] Cambridge University Library, Royal Greenwich Observatory Archives, MSS. 532, *Board of Longitude,* Vol.4, pp. 245 – 246.
[192] Ibid. pp. 247 – 248.
[193] Dr. Thomas Hornsby was Bradley's successor as Savilian Professor of Astronomy. His major achievement was the construction of the Radcliffe Observatory.
[194] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII". Vol. I, MDCCL to MDCCLV, "Observations with Transit and Mural Quadrant from 1750 to 1756", ed. Thomas Hornsby, Oxford 1798.
[195] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII". Vol. II, MDCCLVI to MDCCLXII, "Observations with Transit and Mural Quadrant from 1756 to 1762", ed. Abram Robertson, Oxford 1805.
[196] Bessel, "Fundamenta Astronomiæ", Königsberg, 1818.

# Chapter Six.

## A Newtonian Astronomer in Hanoverian England.

### The "Incomparable" Bradley.[1]

After the death of Bradley in 1762 an awareness of the importance of his work at the Royal Observatory was greatly diminished by the protracted legal dispute which delayed the publication of the observations made under his direction from 1750 to 1762.[2] Charles Mason evidently believed, and was aggrieved that the actions of the Peach family in retaining Bradley's papers and denying access to them by the Board of Longitude were inadvertently destroying his reputation. In his letter to Thomas Hornsby on the 27th of July 1774 he clearly spelled this out. He wrote, "I will beg leave to observe to you that I have seen one of the greatest of men lost."[3] The protracted delays at the Clarendon Press left their publication in 1798[4] and 1805[5] stillborn.[6] By 1805 Bradley's reputation was moribund to all but a few philosophers concerned with the importance of accuracy and precision in the science of positional astronomy.[7] If he was remembered at all it was as "the discoverer of the aberration of light" or by a lesser number, as "the discoverer of the nutation of the earth's axis."

Posterity should therefore be thankful for the fortuitous intervention of the German physician and amateur astronomer Wilhelm Olbers.[8] The Clarendon Press volumes of Bradley's Greenwich observations were purchased by Olbers in 1807 and he brought them to the attention of Friedrich Wilhelm Bessel, a young and as then amateur astronomer of enormous ambition and ability. In 1806, on Olber's recommendation Bessel had accepted the post of assistant at Lilienthal Observatory, near Bremen. The following year he began work reducing Bradley's Greenwich observations. In 1809, when aged 25 he was appointed the director of Wilhelm III of Prussia's Königsberg Observatory and Professor of Astronomy. The observatory was under construction up to 1813 and whilst overseeing this work Bessel continued his work on the reduction of Bradley's observations, which was finally completed in 1818.

What struck Bessel was that the English astronomer had recorded every possible source of observational error.[9] Despite the fact that Bradley lacked a sufficient or satisfactory method and used ad hoc procedures with respect to the problems posed by atmospheric refraction in differing barometric and thermometric conditions, Bessel still respected his work.[10] As a technically precise and accurate observer Bradley was valued by many in the astronomical community after Bessel published the *Fundamenta Astronomiæ*.[11] In a brief flowering of interest in the work of an astronomer who had been deceased for sixty years, his work came to be admired by Sir John Herschel and by Stephen Peter Rigaud. With the reduction of the Greenwich observations, Rigaud determined to publish everything else he considered of importance, in an attempt to re-establish the reputation of a British astronomer who by this time was generally regarded as one of the founders of the now mature science of positional astronomy. As well as holding the same chair at Oxford as Bradley, Rigaud was also a competent historian of mathematics, and the possessor of a fine library of the works of mainly 18th century mathematicians and natural philosophers.[12]

Rigaud's work on Bradley's surviving archive was an achievement of permanent worth, for there can be little doubt that much more of Bradley's correspondence and miscellaneous papers would have been lost without Rigaud's intervention. Bessel had demonstrated the worth of Bradley's work at the Royal Observatory, but it took Rigaud's researches to underline that achievement. Rigaud's work laid before the public the value of Bradley's earlier career, and demonstrated that the observations at Greenwich were the culmination of a lifelong development of a set of skills that had been honed over the previous forty years.[13] Rigaud was sufficiently adept as an historian of science to recognise that the terms of reference of much of Bradley's work were not the same as those of astronomers of the early 19th century. However, his republication of much of the work previously published in sources such as *Philosophical Transactions*, together with his *Memoirs of Bradley* tended to emphasise the impression that Bradley was little more than a consummate technician.[14] This evaluation, so far as it went, was accurate, but what it failed to do was emphasise that the work of

Bradley was developed within a context that was different from that of Rigaud's contemporaries.

**The Astronomical Contexts of Bradley's Work.**

Of 20th century historians Eric Forbes felt impelled to denigrate Bradley's contribution in a rather offhanded manner. He quoted Francis Baily's review, again written in the context of the early 19th century reconstruction of the work of Bradley. Baily asserted,

> Bradley's labours at the Royal Observatory, in this department of the science, consist almost wholly of a re-observation of the stars in Flamsteed's catalogue. He caused these stars to be reduced to the year 1744, and arranged in the order of right ascension, as a sort of working catalogue for his own use; which book still exists in the library of the Royal Observatory. Very few other stars have been observed by Bradley, except such as occasionally entered the field of his telescope whilst he was watching for those of Flamsteed. We are thus indebted to Flamsteed for the subsequent labours of Bradley: for had not Flamsteed led the way, there is much doubt whether Bradley ……would have pursued a similar independent course.[15]

Even a cursory familiarity with the work and the methods of Bradley can only regard Baily's statement as worthy of contempt. Baily's comments taken at their face value would suggest that Bradley was a mere dilettante ready to observe any star that should "occasionally [enter] the field of his telescope"[16] That Bradley capitalised on Flamsteed's catalogue should not be regarded as something reprehensible. On the contrary it served his purpose, for it saved him unnecessary effort in determining which stars he was to observe and paid Flamsteed the compliment which he had long since acknowledged, for he had made extensive use of Flamsteed's catalogue during his formative years as an astronomer.[17] Bradley's object was to observe the stars with unprecedented precision and accuracy that he believed would make his observations of use. Yet the problem of Flamsteed's observations lay not so much in their possible lack of accuracy or the

comparative imprecision of many of his measurements, *but in the consistent failure to record the sources of potential inaccuracy, instrumental, observational or systemic*.[18] It is in the observations of Bradley that the first systematic recording of the sources of possible inaccuracy is carried to a level of proficiency that enables repeated reductions possible with changing standards and theory.

Throughout much of Bradley's mature career as an astronomer he was concerned with the exploration of the limits of accuracy and precision. He recognised that no matter how far he pushed these limits uncertainty would always remain, so he recorded both his observations and all of the factors which might affect their accuracy. Bradley was acutely aware that with technological advances and theoretical innovation his own reports would be improved on, if he also recorded the factors which he believed could be used to reduce his observations with ever greater accuracy and precision.[19] By way of example he was sure that eventually, with increased knowledge and the development of new theory, the problems of atmospheric refraction would be reduced with precision. Indeed this was the main reason why he regularly recorded the barometric pressure and the atmospheric temperature. Bradley worked long and hard on the problems associated with the variables of refraction and attempted to resolve the problem, but he always believed his solutions were inadequate.[20]

The contrast between his character and Flamsteed's could not be more marked. The first Astronomer Royal was a fine observational astronomer with a good grasp of theory, and in this Bradley may be compared with him. Flamsteed, however, grew to distrust many of his contemporaries,[21] and given the relationship he had with both Newton and Halley, this is perhaps understandable. In contrast, Bradley was a man much more at ease with his fellow men. Bradley perceived of his work as a cooperative venture, not merely with his contemporaries, but also with posterity, which accounts for the extraordinary lengths he went to record every possible variation in his observations. Bradley sought to make observations that were of use to others in society, particularly those who might serve his social or scientific purposes. His astronomical practice

reflected the mores of a society in which the "exchange" of ideas and discourse was a central facet of social and political conduct.

Some may claim that Bradley was little more than an astronomical technician who would not have achieved as much if he had not been served by some of the leading scientific instrument makers of the day. In response Bradley was the first to acknowledge his debt. The opening words of his account of his observations of terrestrial nutation were a tribute to an elderly friend and partner, George Graham.

> The great exactness with which instruments are now constructed hath enabled the astronomers of the present age to discover several changes in the positions of the heavenly bodies, which by reason of their smallness had escaped the notice of their predecessors.[22]

There is in these words an implicit criticism of his instrument makers' predecessors. The debt, however, was a mutual one. Bradley's search for exacting precision and accuracy provided a necessary stimulus to several outstanding instrument makers. He worked closely with his instrument makers, particularly Graham, whom he regarded as a close friend as well as an invaluable partner, as well as Jonathan Sisson and John Bird. Of these men he remained unstinting in his praise. Yet his admiration of them must not give rise to the view that his achievement as an astronomer was simply due to his good fortune in working at a time when the precision of scientific instruments and clocks had reached a degree of reliability denied to his predecessors. His first major contribution to astronomy, on the aberration of light, was well within the capability of astronomical instruments from the 1670s. The phenomenon had been repeatedly observed, but its isolation and resolution required the insight of a man who identified a counter-intuitive anomaly which appeared to have a regular cause. Most of his predecessors had attempted to identify the observed motion with annual parallax, as had both Hooke and Flamsteed, or simply regard it as an anomaly created by the unreliability of their instruments or data, as had Picard and Maraldi.[23]

Reflect on Bradley's "discovery" of the aberration of light. The modern figure for the constant of aberration is 20.49". This figure is based on the semi-

major axis of the Earth's annual orbit. In common with other astronomers of his time Bradley measured annual motions based on the *orbis major*, which is the full diameter of the terrestrial orbit. Over the course of a year Bradley observed implied motions of up to 40" or 41" (the equivalent in modern parlance of 20" or 20.5") of arc in many different bright circumpolar stars. Even a limited observer like Halley, who was convinced that the limits of precision were about 5 arc seconds, was capable of observing a motion of such a magnitude. From the inception of telescopes fitted with micrometers in the mid 17th century the phenomenon of annual aberration was well within the capacity of the instruments at their disposal. The main practical barrier lay in the confidence that astronomers had in their instruments and the observations and measurements they made with them. The real contribution made by men like Graham and Sisson lay in the confidence their craftsmanship instilled. As Allan Chapman has stated,

> While it is a truism amongst philosophers of science that the intellectual apparatus of Newtonianism derived from Kepler, it is my argument that Newtonianism could not have existed without the *second* technological breakthrough that was contemporaneous with the Restoration. Without the great improvement in data-collection made possible with the invention of the telescope sight, micrometer and pendulum clock, Newton would have lacked the observational bedrock upon which the consequences of [the] *Principia* could be developed.[24]

From the 1670s astronomers were reporting "anomalies". Jean Picard observed anomalies of 30" or 40" in the position of *Polaris,* and although he briefly entertained the view that it might be evidence of annual parallax, he astutely recognised that the inferred motions did not conform to the predictions implied by parallax theory. Repeated observations of anomalies in the motions of *Polaris* have been proffered as evidence that Picard was the discoverer of the aberration of light.[25] Picard has been characterised as the founder of modern astronomy in France and his work is much to be admired, *but Picard did not discover the aberration of light*. He identified an anomalous motion that was *not* annual parallax.

Whilst Picard was measuring an arc of the meridian, Robert Hooke was making observations of γ Draconis, which passes within 2 minutes of arc of the zenith at the latitude of London. In contrast to the consummate thoroughness of Picard's work, Hooke's was handicapped by illness and overwork.[26] On the basis of four observations made from July to October 1669 he made his famous claim that he had discovered the annual parallax of a star.[27] Natural philosophy was undergoing a major transformation at this time in which qualitative descriptions and causal conjectures were to be transcended by *the mathematical analysis of reiterative data* based on accurate and precise empirical observation.[28] Within this context Hooke's claim was simply unsustainable. This revolution was constructed on the back of the growing market for scientific instruments. The rigour of the work of an astronomer such as Picard was made possible by this change. This rigorous reiterative approach appeared to give firm empirical support to theory when applied by natural philosophers such as Huygens and Newton. The work of controversial though brilliant polemicists like Hooke was gradually abandoned because the mathematisation of nature required the rigorous analysis of precise and accurate observation.

This created an approach to natural and experimental philosophy which was much more *a shared experience* in which knowledge is socialised within a shared milieu of common values and expectations. With the increased mathematisation of nature and its attendant "objectification",[29] it was possible to record data that was capable of being used by other workers. Data was shared across linguistic and national boundaries, and preserved for later workers to use in their own investigations. Of course this did not see the end of controversy or of polemical disputes, for the eighteenth century was to have its own share of vituperative arguments, not least over "the figure of the earth". The new "dispensation" meant that the theoretician was inordinately dependent for his data on observers and experimenters, who were willing to enter that community of mutual interests, and the practitioner was in turn equally dependent on theoretical contexts within which observations and experiments were conducted. This underlined the tragedy of the final breakdown in relations between Newton and

Flamsteed, for the first Astronomer Royal, though perhaps lacking the modesty, self-criticism and the brilliance of Picard, was nevertheless an industrious and a consummate observer. Flamsteed observed annual motions of *Polaris* of about 30 and 40 arc seconds. He made an initial claim that this was evidence of annual parallax which was demolished with great vigour by Cassini II, just as Picard exposed the errors in Hooke's claim for his observations of 1669.

When Bradley joined Molyneux and Graham in December 1725 in order to measure the annual parallax of a star, he was entering a well worn field of endeavour. I will argue that Bradley must have perceived that he was a junior partner in more ways than one in spite of him being Savilian Professor of Astronomy at Oxford. He must have been only too consciously aware of Molyneux's superior social position as the Prince of Wales's private secretary, as a member of both the Westminster and Dublin Parliaments and as a man of enormous wealth, political and social influence. Bradley must also have respected Graham's position as a successful businessman, a respected craftsman and a widely admired gentleman. His participation in the experiment was due to the demise of his uncle.[30] Undoubtedly Bradley was respected by both Molyneux and Graham out of their regard for Pound and for their awareness of his nephew's abilities as an astronomer. Bradley joined his partners at a further disadvantage because of his difficult financial position following the death of his uncle. This is clearly evidenced by his concern to determine the state of his finances earlier that year.[31] These factors help to explain why Bradley was associated with an experiment that took on the character of an implied assault on Newtonian theory, although he could not possibly have apprehended this before he committed himself to his partnership with Molyneux.[32] His calculations and notes in his own copy of the Kew Observation Book[33] suggest that he was seriously concerned about the possibility of confirming Hooke's *result*.[34] In spite of this concern Bradley evidently felt a sense of obligation to his partner partly out of friendship and partly because it was through Molyneux's access to the Prince of Wales he obtained an additional living for him before he was elected to his professorship at Oxford.

Far from confirming the Hooke experiment of 1669, Bradley's work on the "new discovered motion" supported Newton's contention that the stars were very distant from the solar system.[35] The assumptions derived from Bradley's practice,[36] that the Earth was in orbit around the Sun, and that the velocity of light was finite, were overwhelmingly accepted by the astronomical community. This general belief led to the acceptance of the hypothesis with the implied contention that the Earth was in motion. The failure to observe a parallax of a single second led to the widely accepted conclusion that the stars were set at incomprehensible distances from the solar system. The conclusions drawn from this paper finally destroyed any expectations that the stars were near enough for their annual parallaxes to be observed using the technology and observational techniques then available.[37] In the published version of his paper Bradley discussed the upper limits of possibility of an expected observation of annual parallax, and that after observing the annual motions of some seventy bright circumpolar stars he asserted

> I am of [the] opinion, that if it were 1" I should have perceived it, in the great number of observations that I made, especially of γ Draconis; which agreeing with the hypothesis (without allowing anything for parallax) 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; and consequently that it is above 400,000 times farther from us as the sun.[38]

The earlier estimates made by James Gregory and Newton based on the application of photometric methods utilised the assumption that the intrinsic brightness of every star was the same, and that the Sun was in every way a typical star. With these assumptions *the apparent magnitude of a star was solely a function of its distance* from the Earth. An accurate evaluation of the brightness of the Sun and that of a star would thus give the linear distance of a star. Until work was undertaken on binary stars, stars which are bound together by their mutual gravitational interactions, there was no way of determining that stars varied enormously in their intrinsic luminosities. Bradley's failure to detect a single

instance of annual stellar parallax of even a single second of arc appeared to place the stars at inconceivable distances.

Bradley's assertion was supported by work undertaken by Halley on the observed extinction rates of first magnitude stars during lunar occultations, work that was directly appealed to in the third *unpublished* version of his "aberration" paper.[39] Addressing Halley, the recipient of the paper on the "new discovered motion", Bradley offered a passage of extraordinarily percipient insight. Halley had demonstrated that in spite of the brightest stars displaying images of four or five arc seconds in diameter their light was extinguished apparently instantaneously when occulted by the Moon. Bradley asserted:

> if we suppose them no bigger than the Sun, and that they are placed above 400,000 times farther from us as γ Draconis seems at least to be, then their diameters would not be 1/200$^{th}$ part of a second.[40]

In this passage Bradley referred to the observations of *Aldebaran*, which in contrast to most stars, which were extinguished "instantly", appeared to "linger" when occulted by the Moon. *Aldebaran* is now known to be a giant star with a diameter 50 times greater than the Sun. Its angular diameter is about 0.25" and as the Moon takes about 2 seconds to cover 1 arc-second as it moves in its orbit as observed from the Earth, it can be quickly deduced that Aldebaran will take about half a second for its image to disappear as it is occulted by the Moon, a perceptible period.

Prior to this paper many astronomers had considered it a feasible objective to attempt a measurement of the annual parallax of a star, but the enormity of the apparent task after the publication of Bradley's paper on the new discovered motion suppressed all serious attempts until the 19$^{th}$ century. It was to be a century before technology and technique was to be up to the task.[41] Bradley had determined the probable parameters of annual parallax and had revealed the incredibly exacting demands that the measurement of the phenomenon required. On the other hand the small and diminishing number of geocentric astronomers still argued that the continued lack of any evidence for annual parallax, even when an astronomer as exacting as Bradley failed to detect it, was strong evidence in

favour of their resistance to Copernicanism. As I have shown Bradley admitted the possibility in his paper, for he conceded that his hypothesis was based on unverified conjectures.[42] Geocentric models were rejected less because they were ever proven to be incorrect but simply because they fell into disuse. The common language of astronomical discourse simply accepted heliocentric models as a given. The acceptance of such paradigms formed part of the practice of most of the astronomical community. Even though Bradley conceded that the "anti-Copernicans" possessed grounds to object to his hypothesis, the context of the common practice of contemporary astronomers surely and overwhelmingly accepted it as "evidence" of the motion of the Earth.

Although Bradley's work on the aberration of light revealed the enormity of the task of determining the annual parallax of the stars, it also transformed the practice and theory of astrometry which was a necessary prerequisite for such an achievement. Even as Bradley was publishing his paper on the new observed motion, the precision of his observations was just beginning to suggest the existence of yet another phenomenon contained within the residuals that persisted between the predictions of the law and hypothesis of the new discovered motion, and the data he acquired on the annual motions of about seventy bright circumpolar stars. In his identification of a lunar induced nutation Bradley was challenging not merely the limits of the technology available to him but also the bounds of what his mentor Halley considered verifiable. There must have been tension between the two men on this point, even though they obviously shared a genuine respect for each other. In questioning Bradley's claim that he could detect motions as small as half a second of arc Halley wasn't merely challenging his protégé's competence as an astronomer but also by implication his integrity as a gentleman.[43] Seemingly it appears that Bradley simply accepted his mentor's strictures recognising that nothing he could do or say would ever convince him that it was ever possible to detect such minute changes in angular position or motion. On the basis of my studies and analyses I believe he accepted it out of respect for Halley, fully recognising that his friend simply lacked the abilities required to distinguish such minute changes.

Halley had used the zenith sector Graham had constructed for Bradley at Wanstead and his lack of observational technique failed to utilise this extraordinary instrument to anything close to its real potential, convinced as he was from the outset that the observations of motions of much less than four or five arc seconds were never a possibility. Halley's perception was limited by his firmly held presumption that any such claims were fraudulent at worst and deluded at best.[44] An examination of Halley's observational practice, from the evidence of the observations he made at Wanstead and Greenwich, and the way he used his instruments, as evidenced by Bradley's careful examination of the state of the Royal Observatory when he succeeded his friend, reveals his limitations as an observer. Bradley knew this but apparently he did not challenge his mentor's competency as an astronomer, for he recognised that the older man's other gifts were equally precious knowing that he would not have been half the astronomer he was without Halley's earlier guidance.

If Halley did not accept the validity of Bradley's work on the nutation, plenty of other astronomers certainly did. If the paper on the aberration marked his entry into a wider scientific community than that inhabited by the intimate circles of Pound and Newton, the paper on the nutation was the one which fully established his European reputation. From the date of his letter to Maupertuis on the 27th October 1737,[45] to the publication of the nutation paper in 1748,[46] Bradley was courted by an ever wider community of astronomers, natural philosophers and mathematicians, often seeking his opinion, cooperation and support. Although he never visited France he developed a correspondence with some of the leading lights of French astronomy, and with various figures all over continental Europe.

It is not possible to reconstruct the full extent of Bradley's correspondence with other astronomers. He rarely kept copies of his replies to letters, and often used the correspondence addressed to him as rough paper on which to make notes and calculations.[47] It is possible that many papers were lost or mislaid when they came into the possession of Thomas Hornsby or were withheld by Samuel and Susannah Peach. This is much to be regretted. Rigaud was of the opinion that

Bradley was careless in his correspondence,[48] possibly failing to reply to letters he received, but this contradicts almost everything it has been possible to infer about his upright character and demeanour as a conscientious gentleman.[49]

Much of his mature work at the Royal Observatory was centred on the problem of determining the longitude,[50] on his survey of the observed positions of 3,222 stars,[51] on the development of the lunar theory[52] and on his continued observations of the Jovian system[53] at a time when he was a co-opted member of the Board of Longitude.[54] When Bradley was living with his uncle, he developed an abiding interest in the resolution of the problem of determining the longitude at sea. He recognised the potentiality of Hadley's precursor of the mariner's sextant when he personally undertook sea trials with the instrument in 1732.[55] Although Hadley had expressed grave doubts as to the usefulness and efficacy of his instrument as a practical aid to maritime navigation in his paper,[56] Bradley had no doubts. In a letter addressed to Hadley,[57] Bradley informed him that "your invention will be of singular use to all sailors, and instrumental in saving the lives of thousands."[58] The real potential of the sextant in determining not merely the latitude but also the longitude was immediately obvious to Bradley. In his letter to Hadley he wrote,

> I am now convinced, that by means of this contrivance, we may observe at sea with a degree of exactness that 'twas before vain to expect, and which will undoubtedly contribute very much towards the perfection of navigation, not only in determining the latitude to great exactness, and thereby correcting the course as usually measured by the log; but if the moon's theory is ever completed, 'twill be of the utmost consequence in determining the longitude likewise.[59]

In this letter Bradley revealed his "Baconian" propensities. The imperatives of his practice were concerned with the resolution of utilitarian objectives. This tendency was also revealed in his lectures in experimental philosophy.[60] The evidence of his correspondence and papers also suggests that his drive to the limits of precision and accuracy was in accord with his concern with practical objectives.[61] His major objective was the retention of data that would be of use to

the astronomical community in the pursuit of utilitarian objectives, particularly the resolution of the problem of the longitude.[62]

The main context of Bradley's astronomical work was thus utilitarian. As evidenced by his major papers on aberration[63] and nutation[64] it is possible to draw the conclusion that he perceived the inability of empirical evidence to determine theory. Several contending hypotheses could equally account for the empirical evidence. This was illustrated in the paper on aberration. He determined *the law of the new discovered motion* by his analysis of the parameters of the motions of the 60 or 70 stars he had observed from August 1727 at Wanstead. His exposition was deductive, based on the mathematical analysis of precise data.[65] In accordance with the Baconian imperatives of Newtonian natural philosophy, Bradley considered the determination of the parameters of the new discovered motion was of greater import than the construction of hypotheses to account for it. His exposition of *the hypothesis of the new discovered motion* based on the application of a practice that accepted the motion of the Earth and the progressive motion of light[66] conceded that geocentric objections to his hypothesis were valid.

Bradley's achievements can only derive their meaning in the contexts in which they were made, but these contexts cannot be located solely in the "scientific" aspects of his life. These contexts were rooted deeply in the values and mores of Hanoverian social and political life. Many of the values of Newtonian natural philosophy reflected those of the society in which they were formed.[67] I want to show how these values shaped Bradley's astronomical career and how it was advanced by the social mores that Bradley shared with the patrons that formed such a crucial role in his career. Before examining this career and the social and political contexts within which it developed, it is necessary to overthrow the picture of Bradley constructed in the early 19th century. It has undermined an adequate appreciation of his work and continues to affect appraisals of it.[68]

**A Refutation of the 19th Century Interpretation of Bradley's Work.**

In the epistemological and methodological distinctions made between practitioners and theoreticians Bradley was firmly placed in the former when his contributions were appraised during the early 19th century. After the publication in 1818 of the *Fundamenta Astronomiæ*,[69] and the *Miscellaneous Works and Correspondence of James Bradley*[70] along with the *Memoirs of Bradley* in 1832.[71] he was perceived as an exemplar of rigour, precision and accuracy. But as I have attempted to show in my reconstruction of the work of Bradley, such rigid distinctions between theory and practice in the work of such an astronomer are simply unsustainable. His work revealed that his practice was informed by theory and that the theoretical schema within which he worked were in turn informed and modified by his practice. Because Bradley was for so long perceived as a "mere" observer his construction of the theory of aberration was not merely ignored it was not even recognised.[72]

The aberration of light was long characterised as an empirical discovery, and not as a conceptual construction based on the practice of an astronomer who incorporated the theoretical and the practical aspects of his heuristic. Because the aberration of light was perceived as an empirical discovery, made during an attempt to measure the annual parallax of a star, his "discovery" has sometimes even been perceived as entirely fortuitous, made whilst failing to measure the annual parallax of a star. This misconception has retained ramifications up to the present age. I will give just one example. At a meeting of the Royal Astronomical Society in November 1962, called to honour the bicentenary of Bradley's death, the President of the Society Professor W. H. McCrea presented a paper entitled *The Significance of the Discovery of Aberration*.[73] McCrea argued that Bradley made an empirical discovery of the phenomenon of annual aberration. If this were so, then certainly Picard, Flamsteed or Manfredi might have a claim of priority over that of Bradley. However, annual aberration is a "concept" which therefore cannot be "observed". McCrea stated that

> Bradley showed, in effect, that by looking at the fixed stars, one could tell that the Earth is going round something, even if one could not see the

> Sun.[74] In fact, aberration could have led to the discovery of the Sun. In a sense it did. This was because no earlier *observations* [McCrea's Italics] has shown the Sun to exist as something about which the Earth revolves in a way in which we cannot say the Sun revolves about the Earth.[75]

Two things can be stated at this point. Firstly, McCrea clearly viewed *the aberration of light* as an empirical discovery and believed it was discovered solely by observation. Secondly, that the observations made by Bradley pointed indisputably to the theory of aberration. Both of these views are drawn, almost in their entirety, on the early 19th century reconstruction of his work. A study of the primary sources reveals that neither of these conjectures are true.

An examination of McCrea's assertions, that the aberration of light was an empirical discovery and that it was made through *observations*, cannot withstand simple comparison between the work of Bradley and that of his near contemporary, Manfredi. It remains a commonplace that Bradley had offered an empirical proof of the motion of the Earth. It is an assertion repeated *ad nauseum*. In his *Cosmic Discovery* Martin Harwit writes that "Bradley published his findings in 1728.[76] He had *proved* that the earth moves about the sun, though his dreams of discovering the stellar parallax were to remain unfulfilled."[77] However, Bradley had *proved* nothing of the sort. These assertions have simply failed to comprehend the conceptual frameworks within which Bradley had made his observations and the force of the controversy which Manfredi's own contribution raised. Far from claiming that he had demonstrated that the Earth was in motion, he was well aware of the weaknesses of the conceptual foundations of his hypothesis. Indeed he wrote that "the Anti-Copernicans have still room … to object against the motion of the earth".[78] Bradley knew that serious geocentric astronomers, if they existed at all, were very much in a minority, but he clearly apprehended that the motion of the Earth was postulated on the acceptance that the velocity of light was finite. This was accounted for in his practice, in the variation he introduced into the tables of the observed periods of the Jovian satellites which he produced for Halley. In turn, the progressive motion of light was postulated on the acceptance of the motion of the Earth around the Sun, a

presumption that underlay the variation in the Jovian Tables. But neither the motion of the Earth nor the "progressive motion of light" had been demonstrated independently of each other.

When Bradley's paper was translated into Italian and forwarded to Eustachio Manfredi in Bologna,[79] his hypothesis came to the attention of an astronomer who remained committed to geocentricity, and was about to publish a treatise in which he believed he had successfully challenged the Copernican thesis.[80] The conclusion of Manfredi's account led to him to assert, in the context of attempts to measure annual stellar parallax that "a reason must be sought for the movements of this star other than the annual rotation of the earth around the sun."[81] When analysing Manfredi's treatise what becomes obvious is that it incorporates an examination of *specific instances* which he was sure invalidated claims for the discovery of annual parallax by demonstrating that these motions contradicted parallax theory. Manfredi clearly believed, in the contexts of a practice that conformed to Jesuit concepts, that the "aberrations" he had observed as the apparent motions of the "fixed stars" in right ascension were "monsters". Aristotelian and Scholastic traditions usually asserted, "singulars" were never evidence of universal claims, nor did they represent the normal course of nature. Peter Dear refers to the work of the Jesuit philosopher Louis Bertrand Castel specifically in his response to Newton's work on colours. He quotes Castel that singular facts are contrary to nature that "a unique fact is a monstrous fact".[82]

Bradley would have agreed wholeheartedly with Manfredi that all of the claims and other aberrations that he examined had not conformed to the predicted motions of parallax theory. To Manfredi this clearly signified the invalidity of the Copernican thesis. The various "singulars" that he observed - the various phenomena which he called "aberrations" (the very word signifies unsound or aberrant behaviour), were in every way identical to Castel's "monstrous facts". The real divide that separated Bradley and Manfredi lay in their working presumptions and the methodologies and practices based on these presumptions. It is obvious that Manfredi did not believe that his *aberrations* were universal. they were singulars. He was truly convinced that the stars were *fixed*. In a

revealing passage when he discussed the relative motions of two stars Manfredi asserted that,

> we are forced to acknowledge that it is impossible to judge which of the two stars the error is to be attributed, to that whose aberrations we are seeking, or to the other with which we are comparing it, or perhaps to both.
>
> In this uncertainty, nothing better occurs to me than if we choose two or more of the fixed stars, whose intervals of arrival at the meridian are found by many precise observations made at different times of the year *[to be] perpetually the same, .....I do not doubt that this happens to many among the vast number of stars* ..... (my italics) and refer to these or to some one of them, the times of that star, whose aberration we have undertaken to investigate.[83]

In complete contrast with Manfredi's assumption that the stars were indeed "fixed", Bradley assumed that the general phenomenon of the *new discovered motion* which he had observed in dozens of stars was *universal.* In his paper on the new discovered motion Bradley wrote (my italics),

> At last I *conjectured* that all the phenomena mentioned proceeded from the progressive motion of light and the earth's annual motion in its orbit. For *I perceived that, if light was propagated in time,* the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object; and that when the eye is moving in different directions, *the apparent place of the object would be different.*[84]

The contrast between Bradley and Manfredi lies less in that the Englishman was committed to a heliocentric system whilst the Italian was attached to a geocentric scheme, than in the status of their differing knowledge claims. Bradley's work reveals strong Platonic tendencies and for him the world was rational, mathematically explicable and analysable. Bradley presented his audience with a *law* of the new discovered motion, which was acquired by the mathematical analysis of the newly apprehended phenomenon. This was not

acceptable to Manfredi. Although both men agreed to the observed parameters of the motion, once it was presented as a universal *law* Manfredi was unable to accept it. He never accepted Bradley's *law* and always rejected the *hypothesis.* Manfredi's work and the way he processed evidences owed much more to an Aristotelian standpoint, in which he felt distinctly disconcerted by the notion of postulating a universal process or a law on the basis of a series of singular observations. Ironically, however, Manfredi's response to Newton's work on colour was distinctly different to that of Castel, for he was an active and energetic supporter of Newtonian optics in Italy.[85]

Bradley's conjecture was constructed on an astronomical practice incorporating commonly accepted physical assumptions. Both the progressive motion of light[86] and the orbital motion of the Earth were accepted by the overwhelming majority of astronomers and natural philosophers in early 18th century Europe. Manfredi did not accept the motion of the earth, though given his support of the Newtonian theory of light; he may have been able to accept the progressive motion of light, although even in this instance he may have retained reservations.[87] He possessed a tendency in his practice to avoid extrapolating beyond what he had observed. Bradley, however, agreed with Manfredi that he had *not proved* that the Earth was in motion. Giorgio Tabarroni wrote,

> Manfredi regarded these explanations [of annual parallax] as insufficiently tested hypotheses and remained, like Cassini[88] and certain other contemporary astronomers, a lifelong adherent of the geocentric and geostatic conception of the world.[89]

The data recorded by Bradley of motions in declination were compatible with those recorded by Manfredi in right ascension. Manfredi admired Bradley's work, and agreed it confirmed his own observations and analyses. Yet one proposed that the observations might support the interpretation that the Earth was in motion, whilst the other proffered the view that his work displayed evidence that the Earth was not in motion.[90] This contradiction in the interpretation of evidences undermines any view that the aberration of light was either an empirical "discovery" or that the observational evidences pointed unproblematically to the

establishment that the earth was in motion. It is an irony that Manfredi's terminology was utilised by Bradley and applied to the concept of the new discovered motion of the fixed stars. Within a year of the publication of the paper on the new discovered motion Bradley was using the term "aberration" to describe his hypothesis. At first Bradley sought to distinguish between annual parallax and annual aberration by applying the terminology of *parallax on account of distance* for the former phenomenon and *parallax on account of light* for the latter.[91] Manfredi's concept of *aberration* was simply too convenient a term to resist. The earliest document in which Bradley uses the term of *aberration* is preserved in his so-called aberration papers.[92] It is entitled, *Rules for computing the Aberration or Parallax of the Stars arising from the Motion of Light.*[93] It was an ironic turn that probably did not amuse Manfredi.

Bradley's hypothesis of the new discovered motion was erected on provisional conjectures which gave rise to a pivotal concept that transformed the science of positional astronomy. This concept was constructed within a mutually corroborative theoretical structure which enclosed several conjectures. This structure was very persuasive to most of his contemporaries, the conjectures though not independently confirmed, appeared reasonable in a milieu in which heliocentricism was a "given", opposed only by a rearguard of conservative astronomers whose opinions were motivated by different considerations.[94] The concept of stellar aberration was persuasive. It clarified the theoretical bases of positional astronomy, which made possible the technical and organisational advances of the science of astrometry during the rest of the century. It established Bradley at the forefront of contemporary European astronomy.

**The Character of James Bradley.**

I have discussed the advancement of Bradley's astronomical career and his entry into the elites of British social and political life. I think it is necessary to investigate the character of Bradley. It is the key to understanding his success in society. It would be easy to suggest that his opportunities came about because he was his uncle's nephew, and that Pound's many connections in society guaranteed

his success, given the amazing capacity that his uncle possessed in social networking.[95] Bradley was widely liked because of his easy conviviality, a man equally equipped in the social graces as his gregarious uncle. As I have intimated he was less "obviously" active in courting advantage than his uncle, but conversely Bradley acquired an extraordinary reputation for his gentility and kindness and for his patience with people from every background and walk of life. I do not suggest that Bradley was any less ambitious than his uncle for his cultivation of friends and patrons was obviously pursued with equal assiduity.[96] His patience was deeply characteristic. This is discerned in his astronomical work. Not here the eager desire to spread premature findings as his late partner Samuel Molyneux, or his later French protagonist Pierre Le Monnier, who sought to capitalise on Bradley's research on the nutation and pass a lot of it off as his own as he published a premature paper on the subject.[97] It appears that Bradley's response was one of indifference, for he knew that Le Monnier's paper would be regarded as a piece of incorrigible opportunism of the worst kind, and that the French scientific "establishment" was waiting for him to complete his research before it was finally published in 1748.

I think it is necessary to return yet again to Bradley's work on the aberration of light, not to examine it in its astronomical contexts but in order to underline the political milieu in which he presented his paper. I think it is important to recognise that he wrote his paper under many different pressures all of which left their mark on his so-called "discovery" paper. He did not merely compose a narrative; he created a subtle work of propaganda in which he sought to preserve his interests in a society in which his future advancement would inordinately depend on the reception of his paper in a culture of patronage. It has to be recognised that the work at Kew and Wanstead was not undertaken in a scientific or a social vacuum.

At the time he wrote his paper it was widely known that Bradley had begun the observations which were to lead to the discovery of the phenomenon later known as annual aberration with the Hon. Samuel Molyneux, the King's former private secretary, a Lord of the Admiralty and an M.P. in both the British

and Irish Parliaments. Given the notoriety surrounding Molyneux's death, Bradley knew it was incumbent upon him to uphold the character of his erstwhile friend and partner, even though he recognised that he had almost certainly pursued objectives which he was sure would possibly undermine his own hopes and interests. There is every reason to suppose that the Kew experiment was a determined attempt to undermine Newtonian theory, which Bradley therefore deliberately construed as a "disinterested" attempt to measure the annual parallax of a star. Bradley's connections with the Newton circle were still central to his future scientific ambitions. His paper was therefore consciously and very deliberately *Newtonian par excellence*.

I have already referred to the composition of this paper in the second chapter of this thesis and believe I have no need to reiterate the various stratagems that Bradley used in his account. Here, however, I believe it necessary to draw out more specifically the reasons for the strategy. I have already suggested that Bradley had shown concern lest Hooke's *result* be conformed, for he must have been only too aware what this could mean for the validity of Newtonian theory. When the instrument was being tested in December 1725 and the first observations of an entirely inexplicable motion in declination were first detected and it was recognised that it could not possibly be annual parallax, Bradley must have been both relieved and puzzled. Relieved, because it appeared that the motion could not be annual parallax and that such a large motion did not therefore signify that the stars were so close as to challenge the veracity of Newton's writings. Puzzled, because the motion was not merely inexplicable but counter-intuitive. The implied motion of the star appeared to be in the same direction as the motion of the Earth.

It would undoubtedly have offended Bradley if he had ever discovered that his partner had approached his greatest mentor with the suggestion that he had "discovered" a phenomenon that "destroyed completely the Newtonian system.[98] Nowhere in Bradley's papers is there direct evidence that he ever knew of the approach, but there is circumstantial evidence to suggest that he later became aware of it. According to Rigaud, after Molyneux was appointed one of

the Lords of the Admiralty he was too busy to join Bradley at Wanstead except on the day when the new zenith sector was set up. There is no evidence of a meeting between August 1727 when Bradley began his observations at Wanstead and April 1728 when Molyneux collapsed and later died at Kew. There is some indirect and circumstantial evidence to support the view that there was a "cooling" of the relationship between the former partners. Yet in the perception of many who were familiar with the work of Molyneux and Bradley at Kew the two were irretrievably associated with each other. When Bradley came to write his account of the "new discovered motion" he was forced to reconcile the work of his erstwhile colleague with that which he had undertaken at Wanstead and suggest that the experiment at Kew was in every way continuous with it, even though the two experiments were constructed on different grounds and were directed to different objectives. Bradley was also aware that many interested parties knew that Molyneux's sector at Kew was designed on the same principles as the instrument used by Hooke and that the experiment was an attempt to measure the annual parallax of the same star that Hooke had had observed. This meant that Bradley had to reconcile the work undertaken at Kew with that later undertaken at Wanstead.

His account was written in the form of an epistle addressed to Halley. Bradley wrote that the experiment was undertaken in the expectation that the earlier attempt by Hooke to measure annual parallax was "attended with circumstances that promised greater exactness in them, than could be expected in any other that had been made."[99] As I have stated before, anyone familiar with Bradley's methodology can only read this passage with incredulity. This passage reveals that the account of the work on the "new discovered motion" is not all that it appears to be, namely a narrative account of the work he had undertaken. On the contrary, its status as a reconstruction is reinforced when Hooke's claim is set against the rigour of Bradley's reiterative practice. .

So why did Bradley write this astounding passage? Bradley was a thorough-going "Newtonian" practitioner as the Wanstead programme so well illustrates, but in writing up his work on the new discovered motion he was

caught in a distinct quandary. He had to acknowledge his work with his late partner and give the impression that the work at Kew was undertaken in a spirit of unbiased and disinterested inquiry. As I have attempted to show there are strong documentary and circumstantial reasons to believe that Bradley joined an experiment at Kew that ran counter to many of his own beliefs and commitments. He, therefore, carefully crafted an account which gave as much due to Molyneux as would be considered respectful, and yet construct a narrative which glossed over the various distinctly "anti-Newtonian" aspects of the experiment at Kew.

This is why Bradley attempted to convince the public that he entered into the observations at Kew with an open mind, in a spirit of curiosity, apparently sure that the earlier attempt to measure the parallax of a star made by Hooke was both convincing and capable of being verified.[100] He obviously recognised the difficulty that a confirmation of the Hooke result would have posed for Newton's system of universal gravitation and makes his own calculations. He apparently avers that it would probably be sufficiently distant to reconcile it with Newtonian theory, but there must have been an element of distinct unease about it, or else the calculation would not have been made, nor would the conclusion have been voiced. Part of the difficulty of deciphering Bradley's account is that he deliberately conflated the Kew experiment with the Wanstead programme. This has undoubtedly confused many historians[101] and has created a false impression of the distinctly different nature of the observations made at Kew and Wanstead. I do not need to make full quotations in order to make an important point here. In the version of the paper published in the *Miscellaneous Works*[102]Bradley stated,

> after I had continued my observations for a few months, I discovered what I then apprehended to be a general law, observed by all the stars, viz. that each of them became stationary, or was farthest north or south, when they passed over my zenith at six of the clock, either in the morning or evening.[103]

Now this referred to the observations made at Wanstead during the spring and summer of 1728 when he began to recognise the law of the new discovered motion, and began to evaluate the parameters of the phenomenon as displayed in

all of the stars he observed. A few sentences later he referred to some of the observations he made at Kew in the spring of 1726 and even though it was Bradley who made the majority of these observations he deferentially states that

> it appeared from *Mr. Molyneux's observations*,[104] that γ Draconis altered its declination about twice as much as the forementioned small star almost opposite to it;[105]

These were the observations that Bradley was making of the imputed annual nutation when Molyneux made his premature approach to Newton to inform him that he and Bradley had discovered a nutation that "destroyed entirely the Newtonian system."[106]

Why did Bradley ascribe these observations to Molyneux when it was obvious from an examination of the observation books that it was Bradley who made them? I think he had at least two motives. I believe Bradley was ensuring that he would be making a good impression if he also ascribed important insights to his late partner; it was the act of a gentleman, an important part of his strategy of placing himself in the best possible light in a community that valued such conduct. I also think that there is good reason to believe that Bradley was seeking to distance himself from any actions made by Molyneux which were connected to the implied annual nutation.[107] If he ascribed the observations that disproved the conjecture of the annual nutation to Molyneux, he was also absolving him from any imputation that any suspected approach made to Newton was vindictive or ungentlemanly.[108] In a sense Bradley was laying his late friend to an honourable rest.

In his account Bradley simply discarded a whole number of conjectures in just a handful of lines, including the conjecture that the observed motions may have been created by refraction as the Earth passed through a dense inter-planetary medium. He wrote, "Then I considered what refraction might do; but here also nothing satisfactory occurred."[109] It must be appreciated that the whole purpose of observing at the zenith was to avoid the problems associated with atmospheric refraction so this pointed anti-Newtonian conjecture is quietly buried

by the next line in which Bradley presented his hypothesis like pulling a rabbit out of a hat.

> At last I conjectured that all the phænomena hitherto mentioned proceeded from the progressive motion of light and the earth's annual motion in its orbit.[110]

This brief analysis of some of the motives that lay behind his strategy in the composition of the aberration paper reveals Bradley as an intelligent and complex character. There is a conscious "self-fashioning" in which he presents himself as a gentleman, a man who does not claim anything more than his modest endeavours merit. He understood his potential audiences. Bradley sought to advance his own credentials as a gentleman by giving credit to his late friend. This modesty in achievement, or so he judged, was the means by which he might acquire the approbation of those in society he most sought. The paper was an astute, carefully constructed work of propaganda directed to more than one audience,[111] in which he sought to establish his reputation.

**The Meaning of Patronage in the Life and Work of James Bradley.**

From 1711 through the offices of his uncle, Bradley entered a circle of outstanding practitioners who were also closely associated with Newton. His life was largely determined by the social, political and scientific connections associated with this elite circle. Bradley's observational skills were in part a product of his own abilities, and also the product of the skills and techniques he acquired from Pound. Bradley's analytical and mathematical skills were acquired very largely under the guidance of Halley. By 1720 he was increasing recognised as a practitioner of the highest order. Newton died in March 1727, before he was able to appreciate the full extent of Bradley's remarkable abilities. In many eyes the 1729 paper on "the new discovered motion" exemplified Newtonian methodology. I doubt that Bradley would ever have claimed this, for he was a man who was sceptical about such general inferences other than as a statement of general approbation. Rob Iliffe writes:

methodologies are essentially mythical and do not represent the way discoveries were made, and like the "discovery stories" themselves must be seen in serving a specific function in positioning the work with respect to other philosophies. Second, whatever this initial function, methodologies are extremely adaptable and are easily transformed to serve the interests of later writers.[112]

Bradley rarely concerns himself with the philosophical aspects his work and where he does it is intended as a means of aligning himself with those interests which then serve his purposes.[113] If Bradley attempts to exemplify those qualities of rigour, repetition, accuracy and precision, it is because they have become central to a practice that was perceived by many of his contemporaries to be "virtuous". It is a commitment that is gladly upheld by an astronomer who perceives this identity as a means of forwarding his place within the astronomical community and in polite society. Thus while Bradley is ostensibly dealing with astronomical matters, he is doing so within a broader context in which such commitments are seen to uphold and exemplify values that many in polite Hanoverian society placed importance.

There can be little doubt that many in eighteenth-century England disapproved of formal, systematic, and "speculative" learning.[114] The 4th Earl of Chesterfield, who later came to know Bradley when he took up residence at the Ranger's House, wrote that "deep learning is generally tainted with pedantry or at least unadorned by manners".[115] He referred to Maupertuis as the sort of person "one rarely meets with, deep in philosophy and mathematics, and yet *honnête et amiable*".[116] Yet there was another strain of social conduct that was extolled by Joseph Addison and Sir Richard Steele, which perceived immense value in the cultivation of such learning, for they argued that "a man of polite imagination is let into a great many pleasures that the Vulgar are not capable of receiving."[117] Addison and Steele perceived the complete dissolution of all standards of morality and taste, and shared some at least of the ideals of the Society for the Propagation of Christian Knowledge, founded in 1698 to address the "profligacy" attributable to "ignorance". It met with Newton's approval in his own stated wish

to leave behind "paganism and superstition".[118] Bradley sought to present himself in society as a gentleman of polite imagination.

The key to his success in Hanoverian society cannot be easily reduced to the access he had to the sources of patronage he acquired through his uncle's connections, for he appeared to be able to influence many who were never members of the circles in which Pound moved. The approbation he received from people as diverse in outlook and conduct as Pelham, Hearne, Macclesfield and Chesterfield suggests that he was highly sociable and at ease in polite society. Add to this the view of Bradley apprehended by men such as Hornsby and Sir William Jones, that he was noted for his gentility and kindliness, which appended to the admired qualities of his work as an astronomer in which rigour, accuracy, precision, fortitude, patience and industry all play such a central role, it is possible to perceive that he exemplified all that he sought when he published the paper on the "new discovered motion". To society Bradley had successfully become an exemplar of virtue. There is little doubt that he was able through his personal and social skills to affect and influence men from all political and cultural backgrounds, including Lord Chesterfield, who in some measure undertook the lead at Bradley's instigation in sponsoring the Act of Parliament that was to reform the calendar. He was a man capable of maintaining a lifetime's friendship with Lord Macclesfield whilst entreating his political rival[119] to act on an issue in which he betrayed an interest.

By 1730 Bradley was the one of the most skilful and productive Newtonian practitioners in England and his reputation was spreading abroad. A decade later he was widely recognised as one of the leading astronomers in Europe, and this was later specifically acknowledged by Lacaille. Both men admired each other and respected each other's work. By 1750, after the publication of the paper on the nutation, when he was beginning his programme of observations in a Royal Observatory fully re-equipped and reformed, with full time, well trained assistants, Bradley was widely admired as "Newton's astronomer" the exemplar of Newtonianism in its most exacting guise. He was now a permanent member of the Council of the Royal Society, a member of the

Board of Longitude, and an astronomer which every leading scientific academy in Europe was proud to call a member, including those at Paris, Berlin, St. Petersburg and Bologna.[120] The values espoused by Bradley's work had become exemplars of the astronomical sciences throughout much of Europe. This is clearly indicated by the copy of the eight foot quadrant that John Bird made for Bradley which he constructed for the Imperial Observatory at Pulkovo near St. Petersburg.[121] The value given to Bradley's work was a reflection of the rise of Newtonian triumphalism. It was a validation of the shared values of stellar astrometry of which Bradley's work was the *sine qua non*.

It is evident that Bradley was well respected as a gentleman whose demeanour was regarded as gentle and patient, and yet lively and convivial in his social relations. It is equally evident that he was a very industrious and ambitious man. He was a person who could be extraordinarily single minded in the pursuit of his objectives. In the society of 18th century England these characteristics of ambition and gentility were mutually supportive. The evidence of Pound's life suggests that his own success was constructed upon the mutual regard he shared with an ever widening social circle, and it was this milieu which formed the basis of Bradley's advancement in society. This circle was associated with the monied interest.[122] G. J. Barker-Benfield has argued that the triumph of "the monied interest" although regarded with misapprehension in many quarters, even by many Whigs who had done so much to achieve it, depended very much the development of an ethos in which a new conception of what constituted a "gentleman" was accepted.

> Despite their adherence to the Bank of England and the Stock Exchange, *The Tatler* and *The Spectator* presented moneymaking as ignoble. The ideology they embodied attempted to uphold "a single standard of ethical value – a standard that embodied the eighteenth-century conception of the landed patriotic gentleman-citizen". This standard claimed to be the same as that of the Greek and Roman republics, to which that gentleman-citizen had access by way of his privileged education.[123]

These were ideals, arguably more observed in their absence, in such an expansive, materialistic consumer society, but nevertheless they were ideals that were often freely espoused.

However, as Barker-Benfield also records, a gentleman was often regarded as little more than anyone who could distinguish himself from "the common sort of people" by genteel dress and carriage, good education, learning, or an independent station.[124] Yet in early Hanoverian society the reform of manners under the influence of authorities such as Addison and Steele witnessed the development of new sensibilities. A gentleman was regarded as someone who was "mannerly", "courteous" and "polite". Such forms of conduct were regarded as necessary qualities in order to conduct business and arrange credit. In such self-fashioning lay social and economic advancement. The prospects of unlimited acquisition and conspicuous accumulation and consumption required this radical transformation in the rules of civility and politeness.[125] The financial life of the City was constructed on networks of trust and credit.

Bradley's industriousness and attention to detail and his concern with accuracy and precision is a moral characteristic and was undoubtedly taken to be a mark of his gentility. The experimental, or in his case, the observational life was bounded by the same kinds of ethical, moral, social and political restraints as were found in wider society. The apparent incongruity of Bradley's ambition and his evident gentility is perfectly conformable to the values and ideals of 18th century English social and political life. The notion of gentility that developed during the 18th century encompassed both worldliness and idealism.[126] The same networks of trust and mutual regard were equally in evidence in natural philosophy. *Nullius in verba* may have been the adopted motto of the Royal Society but often the verisimilitude of papers published in the *Philosophical Transactions* was determined by little more than the word of the contributor as a gentleman.

In his quest for accuracy and precision Bradley was also led to recognise the importance of standardisation. His observational practice was analysed and projected into a sequence of tacit procedures which were adopted and mastered by his assistants, who worked to the same criteria of rigour, accuracy and precision.

Yet he also recognised that such procedures depended inordinately on such tacit knowledge and that all his procedures, no matter how well observed and accounted for, were essentially impossible to repeat with exactitude. Bradley's "eye and ear" methods of measuring transits led to distinct variations in the patterns of observation between himself and his assistants and also between the various assistants he used. These variations were identified by Bessel when the observations from 1750 to 1762 were analysed and reduced by Bessel who introduced the notion or concept of the "personal equation" to account for it.

The values of Bradley's scientific practice were coterminous with the values and mores of his social life. Bradley possessed authority as an astronomer and as an experimental philosopher and this was signified by the reception of his work, not merely within the astronomical community, but within the bounds of civil society.[127] I have also attempted to argue that he lived in a society in which all advancement was effected through the interchange of mutual obligation and advantage. In Hanoverian England patronage was the means by which mutual interests and ambitions were both promulgated and fulfilled.[128] Power and authority in 18th century Britain was often contentious, due to the rise and fall of various factions in court and in public life. Subsequently it was fluid, and advancement in this society was often fragile. In this context Bradley accepted friendship and patronage from whatever quarter he could acquire it, and it was evident that his strategy was successful when he was appointed Astronomer Royal. His main patron Lord Macclesfield was unsure whether he would be able to support his friend's claim,[129] but Bradley had surreptitiously acquired the admiration and support of others, including Henry Pelham, who had direct access to Walpole.[130] Bradley had acquired access to Pelham through his friendship with Pelham's wife Lady Katherine, a frequent visitor to Greenwich. He undoubtedly made an impression on her, just as surely as he later did on Chesterfield, a man who consciously despised pedants and bores.[131] Bradley is described by Rigaud as being "abstemious in his habits" and states that he "was averse from the promiscuous conversation of common society".[132] Yet contrary to this, his portraits, which suggest that he obviously enjoyed the pleasures of the table,[133] is

reinforced by the evidence that the successful pursuit of his ambitions was achieved not merely through the application of his scientific skill but also through his sociability and for a wide apprehension and appreciation of his easy and generous demeanour.

It is interesting to compare and contrast the patterns of patronage and preference enjoyed by Bradley with those earlier presented to Galileo. The Italian astronomer and mathematician enjoyed the patronage of Cosimo II, de' Medici. In the Medici Court everything was directed towards the glorification of the prince. Favourites rose and fell according to their usefulness to the ambitions of the prince. Mario Biagioli, in his study, characterised the trial of Galileo as an exemplar of the trope of "the fall of the favourite".[134] Throughout, Biagioli argues that the character of Galileo's science is integrated with the role that patronage played in its construction. He draws on the earlier work undertaken by Richard Westfall,[135] particularly in his critique of Pietro Redondi's *Galileo eretico*.[136] Biagioli, with reference to the concept of the fall argued that "once the downfall had begun, it could not be stopped. The prince's power was at stake in the absoluteness of the fall."[137] In a society based on the application of absolute power Galileo was helpless to resist his fate once he had been rejected. Patronage in Hanoverian England by contrast was more widely diffused than in Medicean Florence and whilst scandal and gossip might undo a person's prospects in society such a dramatic fall was much less likely. Conversely, a person's rise through the ranks of society tended to be achieved through the diversification of social contacts.

Unlike the absolutist state of Galileo's Florence Bradley's England was a society where power was always relative and ephemeral. Even the maintenance of Walpole's power base was based the way he elicited the loyal and continuing support of George I and George II and the access this gave him to the arcane and astute application of royal patronage for his political ends. The acquisition of patronage in such a society could often be ephemeral. Once Bradley entered polite society he acquired patronage from wherever it was tendered. Sometimes this patronage relied on friendship, mutual respect and regard,[138] but because it was

drawn from different political lobbies he was able to maintain his interest as the influence of different sources of power shifted.[139]

Bradley's scientific ambitions were to all intents coterminous with his social and political objectives and it was recognised by some political elites that his work was of national importance. Bradley possessed the personal support of the prime minister and the king.[140] In his own successful self-fashioning which displayed the virtues of his social demeanour and the integrity of the precision and accuracy of his work he successfully integrated his own interests with the concerns of the elites of British society.[141] There was a confluence of mutual interest between Bradley's ambitions and the objectives of the society within which these ambitions were formed. I argue that this was not coincidental. If Bradley was to succeed in society it is obvious that his work would be directed to the achievement of the objectives and needs of the milieu he inhabited.[142] Bradley was successful because his objectives and ambitions were aligned with the requirements of the British state, and he was admired and honoured because his work reflected the values and mores of British society. I argue that the meaning of Bradley's networks of patronage and mutual interest lay in the way they coincided with the ruling values and imperatives of Hanoverian society.

**The Significance of the Work of James Bradley.**

What Bradley achieved at Greenwich from 1750 to 1762 was to remain a closed book to all but a handful of his closest collaborators,[143] especially after the Peach family entered into their protracted dispute with the Admiralty.[144] By the time the observations were published in 1798[145] and 1805[146] their significance was all but lost, and it was only after their reduction in 1818,[147] nearly seventy years after the observations were begun, that their importance was belatedly recognised. The work of Bradley was reinterpreted and revised within the contexts of 19th century positional astronomy, and the "James Bradley of early Hanoverian England" was lost sight of. It has been a primary objective of this thesis to attempt a reconstruction of the work of an astronomer and experimental philosopher who so closely represented the development of Newtonian science in

the decades immediately after the demise of Newton himself.[148] It is a modest exploration of the work of a remarkable Newtonian.

The study of 18th century Newtonianism, whether in its popular guise or in terms of the rigour of its mathematical adepts, has tended to avoid the study or analysis of the "work" of many of its most distinctive practitioners, and more attention has been paid to various controversialists and apologists. In the first three decades after the demise of Newton I can think of no single practitioner who most exemplified the rigour, precision and accuracy of Newtonian principles. Bradley was completely immersed in the practices of a group of outstanding astronomers and experimental philosophers who were guided and motivated by the rigorous methods often associated with Newtonian experimental and natural philosophy. Almost every aspect of his work, his observational work as an astronomer and his pedagogical work as a lecturer was directed to ends that were similarly supported by Newton. Bradley was not "the founder of modern observational astronomy",[149] nor was he "the first modern scientist",[150] for each such evaluation merely serves to diminish an appreciation of what I consider to be the importance of Bradley in the history of science.

The observations made from 1750 to 1762 reduced to epoch 1755 by Bessel,[151] were recognised as an irreplaceable database of accurate and precise stellar positions. Bradley's concern with the sources of error had given the observations permanent value as the earliest reliable extant source of data of stellar positions. His work on the Jovian system in support of Halley's tables produced a set of ephemerides of the satellites of Jupiter[152] that not only produced more reliable tables in support of a possible method of determining the longitude at sea, but created the theoretical framework which helped Bradley to develop aberration theory. He calculated the orbits of several comets using Newton's method, based almost entirely on his own observations. He also produced practical reductions for refraction.[153] And finally I will choose to refer to the improvements he made to the Lunar Tables, in the pursuit of the method of finding the longitude at sea using lunar distances.[154] This had been a practical objective from the time he undertook sea trials of Hadley's forerunner of the

mariner's sextant when he recognised that the instrument was not only capable of determining accurate readings of latitude, but also in conjunction with a satisfactory lunar theory, accurate calculations of longitude as well.[155] Bradley's contribution to the development of precise astrometry was essential and fundamental. His work made the resolution of the problem of obtaining the longitude at sea using astronomical methods a real possibility. Eventually these were used in addition to the horological methods achieved after John Harrison perfected the chronometer.

Bradley's achievements were manifold. The most important clearly recognised in his own lifetime was his root and branch reform of the exacting science of astrometry. As I have already stated, his work was the *sine qua non* of the practice of precise and accurate positional astronomy. The law and hypothesis of the new discovered motion, which was eventually reified as the theory of annual stellar aberration, was his first notable achievement.[156] The work he undertook on the nutation of the terrestrial axis from 1727 to 1747 was remarkable not merely for its rigour, precision and accuracy, but also for its sheer perseverance.[157] It was widely admired throughout Europe and led not only to the award of the Copley Medal by the Royal Society, but to his accession as a member of most of the leading scientific academies. He did not merely build the Transit House and re-equip the Royal Observatory with new precisely crafted instruments and clocks; he created a tradition of continuous observation to standards of rigour, precision and accuracy never before achieved, which were adopted in most of the leading European observatories. At Greenwich, which became the European centre of excellence for calculation and observation, he created a tradition of pre-eminence that was maintained for over a century and a half, until in 1928 the International Astronomical Union changed the designation of the standard time of the zero meridian at Greenwich as "Universal Time".

When Bradley became the third Astronomer Royal in 1742 the Royal Greenwich Observatory was in a neglected state. This was partly the product of endemic underfunding but also the infirmity of Halley as paralysis and illness terminated his observations. By the time Bradley ceased regular observation in

1760 his own impending terminal illness did not interfere with the continued efficient running of the observatory as a centre of precise observation and calculation. After his death in 1762 the Royal Greenwich Observatory continued as before, the observations of Bliss and Green being entirely continuous with those made under the supervision of Bradley up to the time when illness finally intruded early in 1761. By then it was the leading national observatory in Europe in which the procedures of observation, measurement and calculation were established and routinised. The rigour, precision and accuracy of the practices of the Royal Observatory were entirely the creation of the work of Bradley. The routines he initiated at Greenwich were as distinctive as the practices that earlier led to the papers on aberration and nutation.

Bradley was an astronomer and a "gentleman", a man who epitomised the values of gentility and utility in a society that extolled both assertiveness and restraint in a dialectic of manners that was transmitted through education and experience extolling the ideals of courtesy and politeness.[158] As a gentleman amongst gentlemen Bradley was to acquire the important connections that were so critical in a society constructed very largely on patronage and obligation. As a gentleman his activities as an astronomer were valued as an exemplification of the virtues of patient fortitude, rigour, precision and accuracy and as the producer of useful knowledge that would benefit commerce and society. The ambitions that drove Bradley were in almost every way conformable to the ruling ideals and objectives of the dominant political and economic elites of British society. The scientific, social and ethical values of Bradley's work reflected many of the ruling values of Hanoverian society.

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[1] Bessel, "Fundamenta Astronomiæ", Königsberg, 1818.

[2] In 1719 Margaret Flamsteed had removed all of her husband's instruments claiming that they were his personal property. An equally proprietary attitude was displayed regarding her late husband's observations. In view of this unfortunate precedent it is perhaps surprising to discover that Bradley's executors were able to enter into such a dispute with the Board of Longitude. The legal status of an Astronomer Royal or his assistant's observations whilst at the Royal Observatory should have been resolved before the demise of Bradley.

[3] Charles Mason to Thomas Hornsby, 27th July 1774. It is quoted at various times in Rigaud's "Memoirs", p. lxxxix, p. xcii, and passim. In a sense Mason must also have believed that any loss of Bradley's reputation reflected on him, for he had undertaken a good proportion of the observations himself utilising Bradley's methods.

[4] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII." Vol. I, MDCCL to MDCCLV, ed. Thomas Hornsby, Clarendon Press, Oxford, 1798.
[5] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII". Vol. II, MDCCLVI to MDCCLXII, ed. Abram Robertson, Clarendon Press, Oxford, 1805.
[6] By the time these unreduced observations had been published in 1798 and 1805, 43 years after they had been completed their relevance appeared to be little more than a historical curiosity.
[7] It was this interest in the work of Bradley by astronomers who were concerned with the issues of accuracy and precision, that was to lead to the reconstruction of his work in the context of early 19th century positional astronomy. This reconstructed "James Bradley" forms the basis of all later evaluations of his work.
[8] His full name was Heinrich Wilhelm Matthäus Olbers.
[9] Bessel, "Fundamenta Astronomiæ". Preface, p.2.
[10] Rigaud, *Analysis of Calculations made by Bradley when investigating a Rule for Refraction.* Supplement to the "Miscellaneous Works".
[11] Bessel, "Fundamenta Astronomiæ", Königsberg, 1818.
[12] Rigaud (1774 – 1839) was a Fellow of Exeter College, Oxford 1794 to 1810, Savilian Professor of Geometry 1810 – 1827 and Savilian Professor of Astronomy and Radcliffe Observer from 1827 to 1839. After his death, his large library of mainly 18th century authors was sold at auction in London. His books on astronomy, mathematics and physics were purchased by the Radcliffe Trustees for the Observatory, while much of the rest on theology, the classics and other subjects were dispersed. In 1935 some 840 books were presented by the Radcliffe Trustees to the Bodleian Library and the rest sold at public auction.
[13] Bradley's work in the context of his contemporaries lies in the way that he demonstrated in his practice a methodology that was also placed at the service of Newtonian theory. His reputation was inextricably bound up with his early association with Isaac Newton who had described him as "the finest astronomer in Europe".
[14] Bessel referred to Bradley as the "incomparable", very largely because his work was so accurate and precise, and also because he had also recorded every conceivable source of error.
[15] Francis Baily, *The Catalogue of Stars of the British Association for the Advancement of Science,* London, 1845. p. 74. Quoted by Forbes on p. 97 of Volume 1 of "Greenwich Observatory: The Royal Observatory at Greenwich and Herstmonceux 1675 – 1975 : Vol. 1 : Origins and Early History (1675 – 1835)". London, 1975.
[16] Ibid. p.74.
[17] This is evident from the early 1720s when Pound initially agreed with Margaret Flamsteed to translate the *Preface* of the *Historia Coelestis Britannica* into Latin, a task he abandoned after coming under pressure from both Halley and Newton.
[18] At the end of the 18th century Caroline Herschel had attempted to reduce Flamsteed's observations but discovered that many observations could not be confirmed or even identified with the stars indicated. Many of the errors appeared to have been systemic.
[19] It was this which so struck Bessel when he first obtained the published editions of Bradley's Greenwich observations in 1807.
[20] Rigaud, "Analysis of Calculations made by Bradley when investigating a Rule for Refraction". Supplement to the "Miscellaneous Works".
[21] The recriminations that passed between Newton and Flamsteed reveals an intense antipathy from both men. Flamsteed was extremely reticent to make his observations available to Newton. After Newton and Halley had published a pirated edition of Flamsteed's observations all civilised communication ceased. The *Historia Coelestis Britannica* or the *British Catalogue of the Heavens* was finally published in 1725 some six years after Flamsteed's demise. In his introduction to the 1982 republication of the *Preface* Allan Chapman states that it was one of the major landmarks in the development of practical astronomy. National Maritime Museum, Maritime Monographs and Reports, No. 52, 1982.
[22] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works" pp. 17 – 41.

[23] Jacques Maraldi, the cousin of Jacques Cassini, was the first to inform Eustachio Manfredi of the anomalous motions of many stars in 1707. This led to Manfredi observing the "aberrations" in right ascension from 1707 to 1719. His results were published in "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, May 1729.
[24] Chapman, "Dividing the Circle: The Development of Critical Angular Measurement in Astronomy 1500 – 1850", 2nd edition, p.147.
[25] Delambre, "Histoire de l'Astronomie au xviii siecle", p. 244.
[26] Working as the City Architect during the rebuilding of London after the great fire of 1666, ensuring that new buildings and structures conformed to the new building regulations, introduced in an attempt to avoid a repetition of that great conflagration.
[27] It was the only attempt specifically designed to attempt to measure annual stellar parallax during the 17th century. Both illness and irreparable damage to his object glass led to the termination of the attempt after just three and a half months. It was entirely characteristic of the "great claimant" that he should make his claim after only four observations.
[28] In part this was a product of the new scientific instrument trade which made precise and accurate observations possible, and the stimulus this gave to observers to observe to ever exacting standards. It was his concern with increasing precision and accuracy that clearly characterised Bradley as a "Newtonian" astronomer.
[29] I do not in any sense wish to claim that the science was in any sense objective, that is, free of observational or theoretical bias. There are too many counter-instances in the history of science to demolish any such pretensions. What I mean by this term is that it led to the gathering of mathematised data. This data could be, and often was, interpreted in different ways.
[30] In April 1724. The observations at Kew began in December 1725.
[31] Bodleian Library, Bradley MS 18, *Notes by James Bradley about annuities, interest and income from professorship in 1724.*
[32] As I have already discussed, there is a profound difference between observing a star in order to "attempt a measure of annual parallax" in order to confirm Copernican theory, and observing a star in order to "confirm Hooke's result" which would definitely have posed a real difficulty for the validation of Newtonian theory.
[33] Bodleian Library, Bradley MS. 14, *The Kew Observation Book,* K14.
[34] The implications of a confirmation of Hooke's result and the claim that he had discovered an annual parallax of some 30 or 40 seconds of arc, implying that the stars were all comparatively close to the solar system, with the implication that their gravitational influences would disrupt the stability of the planetary orbits, posed a distinct threat to the validity of Newtonian theory.
[35] Newton formed his opinion after reading James Gregory's *Geometriæ pars universalis,* Padua, 1668. p. 147, where Gregory discusses the derivation of stellar distances using photometric methods. Bradley found no evidence of annual parallax of even one second, so the nearest stars could be no nearer than 400,000 times the distance between the Sun and the Earth. This fully conformed to Newton's expectations.
[36] For almost ten years Bradley had been observing the satellites of Jupiter, observing the variations from the predicted eclipses, occultations and elongations. Bradley, in conformity with Rømer's hypothesis, accepted that these variations were the result of the differential motions of the Earth and Jupiter in their respective orbits around the Sun and the finite or "progressive" motion of light.
[37] Maskelyne planned an attempt to measure annual parallax when he was at St. Helena to observe the transit of Venus in 1761. The attempt came to nought.
[38] Bradley, the "Miscellaneous Works and Correspondence of James Bradley", p. 15.
[39] Fisher, "James Bradley and the New Discovered Motion: The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars", Appendix 3, "The Third Unpublished Version of James Bradley's Paper on the 'New Discovered Motion', "Major Textual Variations between the Published Account and the Third Unpublished Account", Bradley MS 20* fol. 29v, line 2 to fol. 30r, line 3.
[40] Bodleian Library, Bradley MS. 20*, *Drafts of Papers by James Bradley on Aberration,* fol. 29v.
[41] F. W. Bessel chose a star with a large annual proper motion indicating a proximity to the solar system. He was able to determine the annual parallax of 61 Cygni in 1837. This claim was quickly

followed by that of Friedrich von Struve's measurement of the annual parallax of *Vega* and Thomas Henderson's observation of the annual parallax of α Centauri in 1838. The α Centauri system (with its gravitationally associated star α Centauri C known as "Proxima") has an annual parallax of 0.753".

[42] Bradley, "Miscellaneous Works and Correspondence of James Bradley", p. 15.

[43] Bradley's "self-fashioning" as a "gentleman" would have required overlooking the slight inferred by his friend and mentor, though no doubt it caused him concern.

[44] It is difficult to determine why Halley held the opinion that it was impossible to detect motions as small as 1" or less. I suspect it may have been because he accepted Newton's opinion that because of chromatic and spherical aberration it was impossible to resolve objects with a diameter of less than 5". However there is a difference between resolving an object and detecting a shift in its position. The stellar images that passed through Bradley's field of vision were often 4 or 5 arc seconds in apparent diameter but by careful centring and the use of multiple lines at the focus it was possible to detect very small angles of shift provided the instrument was carefully prepared and checked for accuracy etc.

[45] Bradley to Maupertuis, Oxford, 27th October, 1737. "Miscellaneous Works", pp. 407 – 409.

[46] Bradley, "A Letter to the Right Honourable George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in the "Miscellaneous Works" pp. 17 – 41.

[47] Bodleian Library, Bradley MS. 25. *Calculations and determinations of the orbital elements etc., of the Comets of 1723, 1742, 1748 and 1757.* Bradley MS. 26. *Computations relating to the Sun 1743 to 1758,* and Bradley MS. 27. *Computations relating to the Moon 1722 to 1759.* This may also account for the paucity of correspondence with his assistants. The gathered papers incorporating cometary, solar and lunar calculations are certainly incomplete.

[48] Rigaud is unjustified in this complaint. With so much of the Bradley archive apparently missing it is not possible to make such an assertion except to note that such conduct would contradict the other aspects of his implied character.

[49] The concept of the Hanoverian "gentleman" will be discussed later in this chapter.

[50] Bodleian Library, Bradley MS. 28, *Determination of Longitude by Observations at sea by Captain Campbell, 1757 to 1759.*

[51] Bradley, "Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII". Vol. I, MDCCL to MDCCLV, ed. Thomas Hornsby, Oxford 1798 and Vol. II, MDCCLVI to MDCCLXII, ed. Abram Robertson, Oxford 1805.

[52] Bodleian Library, Bradley MS. 27. *Computations relating to the Moon 1722 to 1759.*

[53] Bodleian Library, Bradley MS. 35. *Tables of Jupiter's Satellites. Copies of some foreign observations.* These included those of Johann Tobias Mayer.

[54] In his function as the Astronomer Royal.

[55] Hadley, "An Account of Observations made on Board the Chatham-Yacht, August 30th and 31st, and September 1st , 1732, in pursuance of an Order made by the Right Honourable the Lords Commissioners of the Admiralty, for the Trial of an Instrument for taking Angles, described in Philosophical Transactions, Number 420, by John Hadley, Esq; Vice-President of the Royal Society." *Philosophical Transactions,* Vol. 37, pp. 341 – 352.

[56] Hadley, "The Description of a new Instrument for taking Angles. Communicated to the Society on May 13, 1731. *Philosophical Transactions*, Vol. 37, pp. 147 – 157.

[57] Bradley to Hadley, in the "Miscellaneous Works", pp. 505 – 506. The letter is undated but following on from the discussion contained in the *Philosophical Transactions* it must date from the latter part of 1732.

[58] Ibid. p. 505.

[59] Ibid. p. 505.

[60] Bodleian Library, Bradley MSS. 3 to 13. *Lectures delivered by James Bradley at Oxford as Savilian Professor of Astronomy and as Lecturer in Experimental Philosophy.*

[61] This is very clear in his letter to George Graham prior to writing to Maupertuis on the 27th October, 1737.

Bradley is critical of Maupertuis's failure to reverse the sector to acquire the double distance of the objective star from the zenith, but otherwise concedes that the procedures followed acceptable

practice. He then asks Graham's advice as to the propriety of informing the Paris Academy of his ongoing observations of nutation to further refine the results of the Lapland expedition.
[62] Bradley saw the resolution of the problem of the longitude in terms of its utility, as an important contribution that astronomers might contribute to the saving of lives, as evidenced by his letter to Hadley on the double reflecting octant, and to the advancement of commerce and the improvement of naval efficiency.
[63] Bradley, "A Letter to Dr. Edmond Halley, Astrom Reg &c. giving an account of a new-discovered Motion of the Fixed Stars, in the "Miscellaneous Works", pp. 1 – 16.
[64] Bradley, "A letter to George, Earl of Macclesfield, concerning an apparent motion observed in some of the fixed stars", in the "Miscellaneous Works", pp. 17 – 41.
[65] Bradley, "Miscellaneous Works", pp. 13 to 14.
[66] Ibid, p. 15.
[67] I am not suggesting that the imperatives of Newtonian natural philosophy can be reduced to the values of Hanoverian society, merely that there were many commonalities which established an easy acceptance of those forms of natural philosophy called Newtonianism.
[68] A good recent example is included in Terrall's recent study of the work of Maupertuis, "The Man Who Flattened the Earth". On p. 159 Terrall wrote, "the text played down Bradley's discovery of aberration." But I have argued that he did not *discover* aberration, it was *constructed* out of his astronomical practice.
[69] Bessel, "Fundamenta Astronomiæ", Königsberg, 1818.
[70] Bradley, "Miscellaneous Works and Correspondence of James Bradley", Oxford, 1832.
[71] Rigaud, "Memoirs", Oxford, 1832.
[72] Rigaud was more aware of the theoretical and philosophical contexts within which Bradley worked but his perceptions were affected by the common distinctions made in his own age. By way of example Rigaud appeared to confuse the "law" with the "hypothesis" of the new discovered motion, which later was reified into the theory of aberration. His inclusion of the Thomson account of the "boating party" also underlines the common view that Bradley "discovered" the aberration of light, whereas a careful analysis reveals that it was "constructed".
[73] W. H. McCrea, "The Significance of the Discovery of Aberration", in the Quarterly Journal of the Royal Astronomical Society, March 1963, pp. 41 – 43.
[74] I have the highest regard for the work of Professor McCrea, but I do think he pushes irony too far at this point, for it is difficult to accept that the existence of an "unseen Sun" could be determined by the observations of the "visible stars". If the Sun were to be "unseen" I might justly conjecture that so would be the stars, and without the visibility of the stars it is difficult to determine where a theory of annual aberration could stem from.
[75] Ibid. p.42.
[76] Bradley published his paper on the 9th and the 16th January, 1728/9 OS at two readings given at weekly meetings of the Royal Society.
[77] Martin Harwit, *Cosmic Discovery: The Search, Scope and Heritage of Astronomy.* Brighton, 1981. p.63.
[78] Bradley, "A Letter to Dr. Edmond Halley, Astrom Reg &c. giving an account of a new-discovered Motion of the Fixed Stars, in the "Miscellaneous Works", p. 15.
[79] Manfredi was in regular correspondence with the Catholic exile Sir Thomas Dereham, who also regularly corresponded with the Royal Society. Although I have not been able to confirm whether Dereham translated Bradley's paper for Manfredi, there is no reason to suppose he didn't, given that he regularly translated for Manfredi in his correspondence with the Royal Society.
[80] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Bologna, 30th May, 1729.
[81] Ibid. p.64.
[82] Peter Dear, "Discipline and Experience", p. 18.
[83] Manfredi, "De Annuis Inerrantium Stellarum Aberrationibus", Caput IX, Sections 154 – 155. The Latin original reads, "sateri cogimur ambiguum esse judicium, utri Stellarum, illine, cujus aberrationes quaerimus, anne alteri, cum qua hanc ipsam comparamus, aut forte utrique, error fit tribuendus. 155. In hac dubietate nihil mihi fatius occurrit, quam si duas, pluresque ex innerantibus Stellis seligamus, quarum intervalla adventus ad meridianum plurimis observationibus, iisque exquisitis, ac per diverto Stellarum numero multis earum contingere non

dubito, atque ad hasce ipsas, vel ad unum aliquam earum, Stellae illius tempora resenius, cujus aberrationem investigare, ac mensuris praesinare suscepimus." p. 69.

[84] Bradley, "A Letter to Dr. Edmond Halley, Astrom Reg &c. giving an account of a new-discovered Motion of the Fixed Stars, in the "Miscellaneous Works", p. 6.

[85] He was elected FRS in 1728.

[86] The term "progressive motion of light" was a way of describing the finite velocity of light. Many natural philosophers opposed the Cartesian contention that the velocity of light was "instantaneous", that is infinite.

[87] Giorgio Tabarroni, *Eustachio Manfredi,* Dictionary of Scientific Biography, Vol. IX, p.77.

[88] Gian Domenico Cassini, and not his son, Jacques Cassini.

[89] Ibid. p.77.

[90] This stark contrast in the interpretation of observations is sufficient to undermine the contention that the aberration of light was an empirical discovery.

[91] Bodleian Library, Bradley MS. 20*, *Drafts of Papers by James Bradley on Aberration,* fol. 60v.

[92] Bodleian Library, Bradley MS. 20*, *Drafts of Papers by James Bradley on Aberration*, fol. 56r.

[93] Ibid. fol. 56r. This document must date between June 1729, when Bradley was able to hear reports of the Manfredi treatise, and early 1731 by when he was commonly using the term. By the time of the Lapland expedition Maupertuis was applying *aberration* in the reduction of his results.

[94] Manfredi was the principal of the Papal College in Bologna. Heliocentricism had been deemed to be heretical from the placement of Copernicus's *De Revolutionibus Orbium Coelestium* on the Index of Prohibited Books by the Holy Office in 1618. Whether Manfredi was really committed to geocentricity is at least questionable. He was a Fellow of the Royal Society with which he corresponded regularly. He was an early propagator of Newtonian optics in Italy.

[95] His uncle's career was initially advanced by the patronage of the Child family.

[96] This is clearly reflected in the positive responses Bradley elicited in his social affairs from people as diverse as Thomas Hearne and Henry Pelham.

[97] Which was received badly by Lacaille and De L'Isle.

[98] There is no direct evidence that Bradley ever discovered that Molyneux had approached Newton prematurely, but the lack of contact after August 1727, and the way Bradley declined to use Molyneux's observations, suggests that Bradley's friendship had cooled. Rigaud suggested that Molyneux was simply too busy to visit Bradley after he became a Lord of the Admiralty, but he wasn't too busy to undertake observations of γ Draconis for three months in 1727.

[99] Bradley, "A Letter to Dr. Edmond Halley, Astrom Reg &c. giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works", p. 1.

[100] Ibid. p.1.

[101] Including Thomson, Rigaud and Robert Grant.

[102] Bradley, "A Letter to Dr. Edmond Halley, Astrom Reg &c. giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works" p. 1 – 16.

[103] Ibid. p.5.

[104] Bradley can refer to these as "Mr Molyneux's observations" because the Kew experiment was undertaken at his direction and with his instrument.

[105] Ibid. p.6.

[106] King's College Library, Cambridge, Conduitt, "Memorabilia" Keynes MS. 130.5, fol. 3.

[107] Newton, "Philosophiæ Naturalis Principia Mathematica", Bk. III, Prop. 21, Theorem, 17. 3rd ed. 1726. Translated by I. Bernard Cohen and Anne Whitman. Newton wrote, "The equinoctial points regress, and the world's axis, by a nutation in every annual revolution, inclines twice toward the ecliptic and twice returns to its former position. This is clear by book I, prop 66, corol. 20. *This motion of nutation, however, must be very small – either scarcely or not at all perceptible.*" An imputed annual nutation of 40 arc seconds was simply irreconcilable with the Principia.

[108] I have already implied that Bradley must have discovered something about Molyneux's approach to Newton, and that this may have led to a distinct cooling of their relationship. If this is so then Bradley is here making his peace.

[109] Bradley, "A Letter to Dr. Edmond Halley, Astrom Reg &c. giving an account of a new-discovered Motion of the Fixed Stars", in the "Miscellaneous Works" . p. 6.

[110] Ibid. p. 6.
[111] The wider audience included many who inhabited the social and political elites who were interested in astronomy or natural philosophy, or anything that might advance society or commerce. Molyneux was not unique for during this period an interest in astronomy was often regarded as a gentlemanly pursuit amongst many in the social elites. This is witnessed by the success of Short in his sale of fine and rather expensive reflecting telescopes.
[112] Rob Iliffe, "Philosophy of Science" in *The Cambridge History of Science,* Vol. 4, *Eighteenth-Century Science,* p. 281.
[113] In 1728 Bradley was in a financially vulnerable position and his commitments involved much more than idle philosophical interest. His "Newtonian" commitments and the manner in which he attempted to exemplify them in his commitment to precision, accuracy, rigour and repetition was as much a political statement as his early commitment to become Bishop Hoadly's personal chaplain and secretary.
[114] Steven Shapin, "The Image of the Man of Science" in *The Cambridge History of Science,* Vol. 4, *Eighteenth-Century Science,* p. 172.
[115] Philip Dormer Stanhope, 4th Earl of Chesterfield, *Letters to his Son,* 1752, Vol. 1, p. 206. Quoted in Steven Shapin, "The Image of the Man of Science" in *The Cambridge History of Science,* Vol. 4, *Eighteenth-Century Science,* p. 172.
[116] Ibid. p.172.
[117] Joseph Addison, *The Spectator,* 21st June, 1712. Quoted in Steven Shapin, ibid. p.172.
[118] G. J. Barker-Benfield, *The Culture of Sensibility: Sex and Society in Eighteenth-Century Britain*. London, 1992, p. 60.
[119] Philip Dormer Stanhope, 4th Earl of Chesterfield, *Letters to his Son,* London, 1752, Vol. 1, p.394. Chesterfield rather despised Macclesfield despite his grasp of mathematics and philosophy because it was so easily bested by his own rhetorical skills.
[120] Rigaud, "Memoirs", p. lxxi.
[121] V. L. Chenakal, "The Astronomical Instruments of the Seventeenth and Eighteenth Centuries in the Museums of the U. S. S. R." in *Vistas in Astronomy*, Vol. 9, (1968), pp. 53 – 77.
[122] Beginning with Pound's association with the Child family, the connections with the Capel family and also with the Wymondesoldes. All were associated with the City.
[123] G. J. Barker-Benfield, "The Culture of Sensibility" p. 86.
[124] Ibid. p.86.
[125] John Brewer, "Commercialization and Politics," pp. 197 – 262, in Neil McKendrick, John Brewer, and J. H. Plumb, *The Birth of a Consumer Society: The Commercialization of Eighteenth-Century England.* Indiana University Press, 1982. p. 214.
[126] They remained ideals, but society was realistic in its apprehensions of human nature and its foibles.
[127] There can be little doubt that his work on aberration gained him some degree of "celebrity". Whiteside had given up giving lectures in experimental philosophy because he was unable to attract sufficient numbers of subscribers to make it worth the effort. The numbers attending Bradley's lectures after he took over from Whiteside were sufficient to offer him a substantial annual income.
[128] Sir Robert Walpole manipulated patronage as a means of managing Parliament. In return for offering offices and benefices he acquired obligations. His opponents often characterised it as corruption.
[129] Lord Macclesfield to Sir William Jones, Shirburne, 14th January, 1741/42 OS.
[130] After resigning, Sir Robert Walpole remained Henry Pelham's friend and ally in his conflict with George II's favourite, the foreign secretary John Carteret, created Earl Granville. Pelham became Prime Minister in 1743 and was eventually to dismiss Granville in 1744.
[131] Philip Dormer Stanhope, 4th Earl of Chesterfield, "Letters to his Son", Vol. 1, p.394.
[132] Rigaud, "Memoirs", p. ciii.
[133] Rigaud wrote a detailed footnote on Bradley's portraits. It reads "there are four paintings of him: one, by Hudson, in the picture gallery at Oxford, which was given to the University by his daughter, in 1769; a second, probably by the same artist at Shirburn Castle; a third, painted by Richardson, for his mother, which was given by the Rev. J. Dallaway to the Royal Society: all

these three were taken when he was in middle life; a fourth, drawn at a more at a more advanced age, had been reserved for herself by Mrs Peach, and after her death was given by her executor. Sam. Lysons, Esq. to the Royal Society for the observatory at Greenwich. The Rev. Dan. Lysons has also a neatly finished miniature of him, drawn in Indian ink, by Ferguson." "Memoirs", p. civ. footnote m.

[134] Mario Biagioli, *Galileo Courtier: The Practice of Science in the Culture of Absolutism.* University of Chicago Press, 1993, p. 313.

[135] Richard Westfall, "Patronage and the Publication of the *Dialogue,* in *Essays on the Trial of Galileo,* Vatican, Vatican Observatory, 1989. pp. 58 – 83, and *Galileo Heretic:* Problems, As They Appear to Me, with Redondi's Thesis, pp. 84 – 104, in Mario Biagioli, "Galileo Courtier", p. 313.

[136] Pietro Redondi, *Galileo Heretic (Galileo eretico)*, translated by Raymond Rosenthal, Penguin Books, London, 1989.

[137] Mario Biagioli, "Galileo Courtier", p. 340.

[138] Including that of George Parker, the 2nd Earl of Macclesfield, Martin Foulkes and Henry Pelham.

[139] So although Macclesfield doubted his ability to influence the choice of Astronomer Royal following the death of Halley, Bradley was able to use his friendship with Pelham. Later he was able to influence Lord Chesterfield who was politically opposed to Macclesfield in order to reform the calendar.

[140] Who both personally admired his work and character.

[141] From the return of James Pound from the East Indies both he and his nephew had been driven by the ambition of developing an astronomical solution to the problem of the longitude. Their work went a long way to making this possible.

[142] Bradley's acquisition of Newtonian values and methods, which were often in empathy with social values that were often concerned with management and control, and with the acquisition of power and influence, meant that his work was reflective of these ruling imperatives.

[143] Mason to Hornsby, 27th July, 1774. Mason was one of Bradley's assistants at Greenwich and Hornsby his successor as Savilian Professor of Astronomy.

[144] Which was abandoned in 1776.

[145] Bradley, *Observations with Transit and Mural Quadrant from 1750 to 1756,* ed. Hornsby, Oxford ,1798.

[146] Bradley, *Observations with Transit and Mural Quadrant from 1756 to 1762,* ed. Robertson, Oxford, 1805.

[147] Bessel, "Fundamenta Astronomiæ", Königsberg, 1818.

[148] Not enough work has been undertaken on the work of "Newtonian" practitioners as opposed to the arguments of post-Newtonian polemicists.

[149] Agnes Clerke, *James Bradley,* Dictionary of National Biography, Vol. II, London, 1882. pp. 1074 – 1079.

[150] Freeman J. Dyson, *Nature,* Vol. 400, July 1999, p. 27.

[151] Bessel, "Fundamenta Astronomiæ", Königsberg, 1818.

[152] Bodleian Library, Bradley MS. 16, *Tables of Astronomical Data – Sun, Moon, Jupiter and its Satellites,* and Bradley MS. 35, *Tables of Jupiter's Satellites.*

[153] Rigaud, "Analysis of Calculations made by Bradley when investigating a Rule for Refraction, in Miscellaneous Works and Correspondence of James Bradley", Supplement, 1833.

[154] Bodleian Library, Bradley MS. 27, *Computations relating to the Moon 1722 to 1759.*

[155] Rigaud, "Memoirs", p. xl.

[156] Bradley, "A letter to Dr. Edmond Halley, giving an account of a new-discovered motion in the fixed stars, in The Miscellaneous Works and Correspondence of James Bradley", pp. 1 – 16. See also Fisher, "James Bradley and the New Discovered Motion: The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars", MSc dissertation, University of London, 1994.

[157] Bradley, "A letter to George, Earl of Macclesfield, concerning an apparent motion observed in some of the fixed stars, in The Miscellaneous Works and Correspondence of James Bradley", pp. 17 – 41.

[158] Barker-Benfield, "The Culture of Sensibility". p. 87.

# A Bibliography of Primary and Secondary Printed Sources.


'Espinasse, Margaret — *Robert Hooke,* William Heinemann, London, 1956.

Aiton, Eric — *The Cartesian Theories of the Planetary Motions,* PhD Thesis, University of London, 1958.

Aiton, Eric — *The Vortex Theory of Planetary Motions,* Macdonald, London, 1972.

Alder, Ken — "French Engineers Become Professionals: or, How Meritocracy Made Knowledge Objective Enlightenment" in William Clark, Jan Golinski and Simon Schaffer (eds.) *The Sciences in Enlightened Europe,* University of Chicago Press, London, 1999, pp. 94 – 125.

Alexander, A. F. O'D. — *The Planet Saturn: A History of Observation, Theory and Discovery,* Dover Publications, New York, 1962.

Alexander, H. G. (ed.) — *The Leibniz-Clarke Correspondence: together with extracts from Newton's Principia and Opticks*, edited with introduction and notes by H. G. Alexander, Manchester University Press, Manchester, 1956.

Allen, Richard H. — *Star Names their Lore and Meaning* London, 1899. reprinted Dover Publications, New York, 1963.

Aristotle — *Physics,* with an English translation by Philip H. Wicksteed and Francis S. Cornford, Loeb Edition, William Heinemann, London, 1980.

Ashworth, William J. — "The calculating eye: Baily, Herschel, Babbage and the business of astronomy", BJHS, 1994, Vol. 27, pp. 409 – 441.

Bacon, Francis — *Novum Organum: With Other Parts of the Great Instauration,* translated and edited by Peter Urbach and John Gibson, Open Court, Chicago, 1994.

Baily, Francis — *An Account of the Rev. John Flamsteed, the First Astronomer Royal; compiled from his Manuscripts and other authentic Documents, never before published,* London, 1835.

Barker-Benfield, G. J. — *The Culture of Sensibility: Sex and Society in Eighteenth-Century Britain,* University of Chicago Press, London, 1992.

Beeson, David — *Maupertuis: An Intellectual Biography,* The Voltaire Foundation, Oxford, 1992.

Bennett, J. A. — "Hooke's Instruments" in J. A. Bennett, Michael Cooper, Michael Hunter and Lisa Jardine, *London's Leonardo: The Life and Work of Robert Hooke,* Oxford University Press, Oxford, 2003.

Bennett, J. A. — "Hooke's Instruments for Astronomy and Navigation" in Michael Hunter and Simon Schaffer (eds.), *Robert Hooke: New Studies,* The Boydell Press, Woodbridge, 1989, pp. 21 – 32.

Bennett, J. A. et al — *London's Leonardo: The Life and Work of Robert Hooke,* with contributions by J. A. Bennett, Michael Cooper, Michael Hunter and Lisa Jardine, Oxford University Press, Oxford, 2003.

Bennett, J. A. — *The Mathematical Science of Christopher Wren,* Cambridge University Press, Cambridge, 1982.

Berkeley, George — *A Treatise concerning the Principles of Human Knowledge,* J. M. Dent, London, 1980.

Berlinski, David — *Newton's Gift: How Sir Isaac Newton Unlocked the System of the World,* Duckworth, London, 2000.

Berry, Arthur — *A Short History of Astronomy: From Earliest Times Through the Nineteenth Century,* John Murray, London, 1898.

Bertoloni Meli, D. — *Equivalence and Priority: Newton versus Leibniz,* Clarendon Press, Oxford, 1993.

Bertoloni Meli, D. — "The Relativization of Centrifugal Force" in Isis, Vol. 81, No. 306, 1990, pp. 23 – 43.

Bessel, F. W. — *Fundamenta Astronomiæ pro anno MDCCLV deducta ex observationibus viri incomparabilis James Bradley in specula Grenovicensi per annos 1750 – 1762 institutis*, Konigsberg, 1818.

Biagioli, Mario — *Galileo, Courtier: The Practice of Science in the Culture of Absolutism,* University of Chicago Press, London, 1993.

Biagioli, M. & Galison, P. — *Scientific Authorship: Credit and Intellectual Property in Science,* edited by Mario Biagioli and Peter Galison, Routledge, London, 2000.

Birks, John L. — *John Flamsteed: the First Astronomer Royal at Greenwich,* Avon Books, London, 1999.

Black, J. & Porter, R. — *A Dictionary of Eighteenth-Century World History,* edited by Jeremy Black and Roy Porter, Blackwell Publishers, Oxford, 1994.

Blay, Michel — *Reasoning with the Infinite: From the Closed World to the Mathematical Universe,* translated by M. B. DeBevoise, University of Chicago Press, London, 1998.

Boyer, Carl B. — *The History of the Calculus and its Conceptual Development:* Dover Publications, New York, 1959.

Brackenridge, J. Bruce — *The Key to Newton's Dynamics: The Kepler Problem and the Principia,* University of California Press, London, 1995.

Bradley, James — *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832.

Bradley, James — *Supplement to the Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1833.

Bradley, James — "An Account of some Observations made in London, by Mr. George Graham, FRS; and at Black-River in Jamaica, by Colin Campbell, Esq. FRS concerning the Going of a Clock, in order to determine the Difference between the Lengths of Isochronal Pendulums in those places", in *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 62 – 69, and *Philosophical Transactions,* No. 432, Vol. XXXVIII, 1734, pp. 302 – 313.

Bradley, James — *Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII.* Vol. I, MDCCL to MDCCLV, ed. Thomas Hornsby, Clarendon Press, Oxford, 1798.

Bradley, James — *Astronomical Observations made at Greenwich from the year MDCCL to the year MDCCLXII.* Vol. II, MDCCLVI to MDCCLXII, ed. Abram Robertson, Clarendon Press, Oxford, 1805.

Bradley, James — "Correspondence of James Bradley", in *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 391 – 510.

Bradley, James — "Demonstration of the Rules relating to the Apparent Motion of the Fixed Stars upon account of the Motion of Light", in *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 287 – 301.

Bradley, James — "Directions for using the common Micrometer", in *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 70 – 74, and *Philosophical Transactions,* Vol. LXII, pp. 46 – 52.

Bradley, James — "Experiments to determine the Length of the Pendulum vibrating seconds at Greenwich", in *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 384 – 387.

Bradley, James — "A Letter to Dr. Edmund Halley, Astronomer Royal and etc., giving an account of a new-discovered Motion of the Fixed Stars", in *Miscellaneous Works and Correspondence of James Bradley,* edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 1 – 16, and *Philosophical Transactions,* No. 406, Vol. XXXV, 1729, pp. 637 – 661.

Bradley, James — "A Letter to the Rt. Hon. George Earl of Macclesfield, concerning an apparent Motion observed in some of the Fixed Stars", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 17 – 41, and *Philosophical Transactions,* No. 485, Vol. XLV, 1748, pp.1 – 36.

Bradley, James — "The Longitude of Lisbon and the Fort of New York from Wansted and London, determined by Eclipses of the First Satellite of Jupiter" , in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 58 – 61, and *Philosophical Transactions,* No. 394, Vol. XXXIV, 1724, pp. 85 – 89.

Bradley, James — "Memoranda respecting the instructions that Dr. Bradley drew up at the desire of the Council of the Royal Society, relating to Charles Mason observing the transit of Venus in the East Indies", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 388 – 390.

Bradley, James — "Memoranda respecting the instrument at Wansted", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 194 – 200.

Bradley, James — "Miscellaneous Astronomical Observations", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 339 – 380.

Bradley, James — "Observations made at Kew", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 116 – 193.

Bradley, James — "Observations on the Fixed Stars made at Wansted in Essex by J. Bradley" , in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 201 – 286.

Bradley, James — "Observations upon the Comet that appeared in the months of October, November, and December, 1723", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 42 – 48, and *Philosophical Transactions,* No. 382, Vol. XXXIII, 1724, pp. 41 – 50.

Bradley, James — "Observations upon the Comet that appeared in the months of January, February, and March, 1737", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 49 – 52, and *Philosophical Transactions,* No. 446, Vol. XL, 1737, pp. 111 – 117.

Bradley, James — "Observations upon the Comet that appeared in the months of September and October 1757, made at the Royal Observatory", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 53 – 57, and *Philosophical Transactions,* Vol. L, 1758, pp. 408 – 413.

Bradley, James — "Reduction of the Wansted Observations", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 302 – 338.

Bradley, James — "The reverend Mr. James Bradley's Observations on his Tables of Jupiter Satellites", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 81 – 83.

Bradley, James — "State of the Instruments at the Greenwich Observatory when Dr. Bradley became Astronomer Royal", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 381 – 383.

Brewer, John — "Commercialization and Politics", in Neil McKendrick, John Brewer, and J. H. Plumb, *The Birth of a Consumer Society: The*

*Commercialization of Eighteenth-Century England.* Indiana University Press, London, 1982. pp. 197 – 262.

Brewer, John — *The Pleasures of the Imagination: English Culture in the Eighteenth Century,* HarperCollins, London, 1997.

Buchdahl, Gerd — "Explanation and Gravity" in *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham,* edited by Mikuláš Teich and Robert Young, Heinemann, London, 1973, pp. 167 – 203.

Buchwald, Jed Z. (ed.) — *Scientific Practice: Theories and Stories of Doing Physics,* University of Chicago Press, London, 1995.

Burke, John G. (ed.) — *The Uses of Science in the Age of Newton,* University of California Press, London, 1983.

Cannon, John — *Aristocratic Century: The peerage of eighteenth-century England,* Cambridge University Press, Cambridge, 1984.

Cassini, Jacques — "Réflexions sur une letter 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. pp 177 – 183.

Cassirer, Ernst — *The Philosophy of the Enlightenment,* Princeton University, Princeton, 1957.

Chapman, Allan — *Dividing the Circle: The Development of Critical Angular Measurement in Astronomy, 1500 – 1850,* 2nd ed. John Wiley & Sons, Chichester, 1995.

Chenakal, V. L. — "The Astronomical Instruments of the Seventeenth and Eighteenth Centuries in the Museums of the U. S. S. R". in *Vistas in Astronomy*, Vol. 9, (1968), pp. 53 – 77.

Clark, George — *Science and Social Welfare in the Age of Newton,* 2nd edition, Clarendon Press, Oxford, 1970.

Clark, W. et al (eds) — *The Sciences in Enlightened Europe,* edited by William Clark, Jan Golinski and Simon Schaffer, University of Chicago Press, London, 1999.

Clarke, Samuel — *The Works of Samuel Clarke, DD,* edited by Benjamin Hoadly, 1738. Reprinted in 4 volumes by Thoemmes Press, October, 2002.

Clarke, Agnes — "James Bradley", in the *Dictionary of National Biography,* Vol.2, London, 1882, pp. 1074 – 1079.

Cohen, I. Bernard — *Introduction to Newton's 'Principia',* Harvard University Press, Cambridge, Mass., 1978.

Cohen, I. B. & Smith G.E. — *The Cambridge Companion to Newton,* edited by I. Bernard Cohen and George E. Smith, Cambridge University Library, 2002.

Cohen, I. Bernard — *The Newtonian Revolution: With illustrations of the transformation of scientific ideas,* Cambridge University Press, Cambridge, 1980.

Collins, H. M. — *Changing Order: Replication and Induction in Scientific Practice,* Sage Publications, London, 1985.

Cook, Alan — *Edmond Halley: Charting the Heavens and the Seas,* Clarendon Press, Oxford, 1998.

Daston, Lorraine — "The Ethos of Enlightenment" in William Clark, Jan Golinski and Simon Schaffer (eds.) *The Sciences in Enlightened Europe,* University of Chicago Press, London, 1999, pp. 495 – 504.

Dear, Peter — *Discipline and Experience: The Mathematical Way in the Scientific Revolution,* University of Chicago Press, London, 1995.

Dear, Peter — *Revolutionizing the Sciences: European Knowledge and its Ambitions, 1500 – 1700,* Palgrave, Basingstoke, 2001.

Derham, William — *Astro-Theology: Or a Demonstration of the Being and Attributes of God, from a Survey of the Heavens*, London, 1714.

Derham, William — *Physico-Theology (The Boyle Lectures),* London, 1713.

Desaguliers, J. T. — *A Course of Experimental Philosophy,* London, 1734.

Dobbs, Betty Jo Teeter — *The Foundations of Newton's Alchemy, or "the Hunting of the Greene Lyon"*, Cambridge University Press, 1975.

Dobbs, B. J. & Jacob, MC. *Newton and the Culture of Newtonianism,* Humanity Books, New York, 1998.

Dobbs, Betty Jo Teeter "Newton as Final Cause and First Mover", in *Rethinking the Scientific Revolution,* University of Cambridge Press, Cambridge, 2000, pp. 25 – 39.

Dupré, John *The Disorder of Things: Metaphysical Foundations of the Disunity of Science,* Harvard University Press, 1993.

Euler, Leonhard *Letters of Euler to a German Princess,* translated by Henry Hunter, London, 1795.

Euler, Leonhard "Letter XVIII, Difficulties attending the System of Emanation" in *Letters of Euler to a German Princess,* translated by Henry Hunter, London, 1795, pp. 79 – 83.

Feyerabend, Paul *Against Method: Outline of an Anarchistic Theory of Knowledge,* Verso Edition, London, 1975.

Fisher, John Roger *James Bradley and the New Discovered Motion: The Origins, Development and Reification of James Bradley's Hypothesis of the New Discovered Motion of the Fixed Stars,* MSc Dissertation, University of London, 1994.

Flamsteed, John *The Preface to John Flamsteed's Historia Coelestis Britannica,* edited by Allan Chapman, National Maritime Museum, Greenwich, 1982.

Fleck, Ludwik *Genesis and Development of a Scientific Fact,* edited by Thaddeus J. Trenn and Robert K. Merton, translated from *Enstehung unt Entwicklung einer wissenschaftlichen Tatsache: Einfuhrung in die Lehre vom Denkstil und Denkkollectiv,* by Fred Bradley and Thaddeus J. Trenn, Chicago University Press, London, 1979.

Fontenelle, B. l. B. s. d. Excerpts from "Conversations on the Plurality of Worlds" in *Science in Europe, 1500 – 1800, Primary Sources Reader,* edited by Malcolm Oster, Palgrave, Basingstoke, 2002.

Fontenelle, Bernard Le Bovier, sieur de "The Elogium of Sir Isaac Newton: By Monsieur Fontenelle, Perpetual Secretary of the Royal Academy of Sciences at Paris" in *Isaac Newton's Papers and Letters on Natural Philosophy and Related Documents,* edited by I. Bernard Cohen assisted by Robert E. Schofield, Cambridge University Press, Cambridge, 1958, pp. 444 – 474.

Fontenelle, Bernard Le Bovier, sieur de *Théorie des Tourbillons,* Imprimerie Royale, Paris, 1752.

Forbes, Eric *Greenwich Observatory: The Royal Observatory at Greenwich and Herstmonceux 1675 – 1975 :* Vol. 1 : *Origins and Early History (1675 – 1835).* London, 1975.

Force, James E. *William Whiston: Honest Newtonian,* Cambridge University Press, Cambridge, 1985.

Frasca-Spada, Marina "The Science and Conversation of Human Nature Enlightenment" in William Clark, Jan Golinski and Simon Schaffer (eds.) *The Sciences in Enlightened Europe,* University of Chicago Press, London, 1999, pp. 218 – 245.

Gabbey, Alan *Newton and Natural Philosophy,* in R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge (eds.), *Companion to the History of Modern Science,* Routledge, London, 1990, pp. 243 – 263.

Garber, Daniel *Descartes' Metaphysical Physics,* University of Chicago Press, London, 1992.

Gillispie, Charles C. *Fontenelle and Newton* in *Isaac Newton's Papers and Letters on Natural Philosophy and Related Documents,* edited by I. Bernard Cohen assisted by Robert E. Schofield, Cambridge University Press, Cambridge, 1958. pp. 427 – 443.

Gillispie, Charles C. *Pierre-Simon Laplace1749 – 1827: A Life in Exact Science,* with the collaboration of Robert Fox and Ivor Grattan Guinness, Princeton University, Princeton, 2000.

Golinski, Jan "Barometers of Change: Meteorological Instruments as Machines of Enlightenment" in William Clark, Jan Golinski and Simon Schaffer (eds.) *The Sciences in Enlightened Europe,* University of Chicago Press, London, 1999, pp. 69 – 93.

Gooding, David "Putting Agency Back into Experiment" in *Science as Practice and Culture,* edited by Andrew Pickering, University of Chicago Press, London, 1992, pp. 65 – 112.

Gooding, D. et al (eds.) *The Uses of Experiment: Studies in the Natural Sciences,* edited by David Gooding, Trevor Pinch and Simon Schaffer, Cambridge University Press, 1989.

Grant, Robert *History of Physical Astronomy: From the Earliest Ages to the Middle of the Nineteenth Century,* Henry G. Bohn, London, 1852.

Grasshoff, Gerd *The History of Ptolemy's Star Catalogue,* Springer Verlag, London, 1990.

Greenberg, John L. *The Problem of the Earth's Shape from Newton to Clairaut: The rise of mathematical science in eighteenth-century Paris and the fall of "normal" science,* Cambridge University Press, Cambridge, 1995.

Gregory, J & Stevenson J. *The Longman Companion to Britain in the Eighteenth Century, 1688 – 1820,* Longman, London, 2000.

Grosholz, Emily R. *Cartesian Method and the Problem of Reduction,* Clarendon Press, Oxford, 1991.

Gross, Alan G. *The Rhetoric of Science,* Harvard University Press, London, 1990.

Hahn, Roger *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666 – 1803,* University of California Press, London, 1971.

Hakfoort, Casper *Optics in the Age of Euler: Conceptions of the Nature of Light, 1700-1795,* Cambridge University Press, Cambridge, 1995.

Hall, A. Rupert *All Was Light: An Introduction to Newton's Opticks,* Clarendon Press, Oxford, 1993.

Hall, A. Rupert *From Galileo to Newton,* Dover Publications, New York, 1981.

Hall, A. Rupert *Isaac Newton: Adventurer in Thought,* Blackwell Publications, Oxford, 1992.

Hall, A. Rupert *Isaac Newton: Eighteenth-century Perspectives,* Oxford University Press, Oxford, 1999.

Hankins, Thomas L. *Science and the Enlightenment,* Cambridge University Press, Cambridge, 1985.

Harvey, A. D. *Sex in Georgian England: Attitudes and Prejudices from the 1720s to the 1820s,* Duckworth, London, 1994.

Harwit, Martin *Cosmic Discovery: The Search, Scope and Heritage of Astronomy,* Harvester Press, Brighton, 1981.

Hatton, Ragnhild *George I,* Yale University Press, London, 2001.

Heaps, Leo *Log of the Centurion,* London, 1974.

Heilbron, J. L. *Elements of Early Modern Physics,* University of California Press, London, 1982.

Henry, John *The Scientific Revolution and the Origins of Modern Science,* 2nd ed. Palgrave, Basingstoke, 2002.

Hesse, Mary B. *Forces and Fields: A Study of Action at a Distance in the History of Physics,* Thomas Nelson, London.

Hewitt, K. V. *The Astronomical Work of the Reverend James Bradley, Third Astronomer Royal,* MSc Dissertation, University of London, 1953.

Hirshfeld, Alan W. *Parallax: The Race to Measure the Cosmos,* W. H. Freeman & Sons, New York, 2001.

Hoadly, Benjamin *The Nature of the Kingdom or Church of Christ*, by Command of His Britannic Majesty King George I of Great Britain, London, 1717.

Hoadly, Benjamin — *Preservative against the Principles and Practices of the Nonjurors both in Church and State,* London, 1716.

Hooke, Robert — "An Attempt to Prove the Motion of the Earth from Observations" in R. T. Gunther, *Early Science in Oxford,* Vol. 8, pp. 17 – 25.

Hoskin, Michael (ed.) — *The Cambridge Concise History of Astronomy,* Cambridge University Press, Cambridge, 1999.

Howse, Derek, — *Greenwich Observatory: Volume 3: The Buildings and Instruments,* National Maritime Museum, London, 1975.

Hunter, Michael & Schaffer, Simon — *Robert Hooke: New Studies,* The Boydell Press, Woodbridge, 1989.

Hunter, Michael — *Science and Society in Restoration England,* Cambridge University Press, Cambridge, 1981.

Hutchinson, John — *Moses' Principia,* London, 1724.

Iliffe, Rob — "'Aplatisseur du Monde et de Cassini': Maupertuis, Precision Measurement, and the Shape of the Earth in the 1730s" in *History of Science,* vol. xxxi (1993), pp. 335 – 375.

Iliffe, Rob — "Butter for Parsnips: Authorship, Audience, and the Incomprehensibility of the Principia" in *Scientific Authorship: Credit and Intellectual Property in Science,* edited by Mario Biagioli and Peter Galison, Routledge, London, 2000, pp. 33 – 65.

Iliffe, Rob — "Material doubts: Hooke, artisan culture and the exchange of information in 1670s" London, BJHS, 1995, Vol. 28, pp. 283 – 318.

Iliffe, Rob — "Philosophy of Science" in Roy Porter (ed) *The Cambridge History of Science: Vol. 4: Eighteenth-Century Science,* Cambridge University Press, 2003. pp. 267 – 284.

Jardine, Lisa — *On a Grander Scale: The Outstanding Career of Sir Christopher Wren,* HarperCollins, London, 2002.

Jacob, Margaret C. — *The Newtonians and the English Revolution, 1689 – 1720,* Ithaca, Cornell University Press, 1976.

Jacob, Margaret C. — "The Truth of Newton's Science and the Truth of Science's History: Heroic Science at Its Eighteenth-Century Formulation" in Osler, Margaret (ed.) *Rethinking the Scientific Revolution,* University of Cambridge Press, Cambridge, 2000, pp. 315 – 332.

Johns, Adrian — "The Ambivalence of Authorship in Early Modern Natural Philosophy Principia" in *Scientific Authorship: Credit and Intellectual Property in Science,* edited by Mario Biagioli and Peter Galison, Routledge, London, 2000, pp. 67 – 90.

King, Henry C. — *The History of the Telescope,* Dover Publications, New York, 1979.

Knorr-Cetina, Karin D. — "The Couch, the Cathedral, and the Laboratory: On the Relationship between Experiment and Laboratory in Science", in *Science as Practice and Culture,* edited by Andrew Pickering, University of Chicago Press, London, 1992. pp. 113 – 138.

Knorr-Cetina, Karin D. — *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science,* Pergamon Press, Oxford, 1981.

Koyré, Alexandre — *Metaphysics and Measurement: Essays in the Scientific Revolution,* Chapman and Hall, London, 1968.

Koyré, Alexandre — *Newtonian Studies,* University of Chicago Press, London, 1965.

Langford, Paul — *A Polite and Commercial People: England 1727 – 1783,* Oxford University Press, 1992.

Latour, B. & Woolgar, S. — *Laboratory Life: The Construction of Scientific Facts,* Princeton University Press, Princeton, 1986.

Latour, Bruno — *Science in Action,* Harvard University Press, London, 1988.

Leighton, C. D. A. — "Hutchinsonianism": A Counter-Enlightenment Reform Movement" in *Journal of Religious History,* Vol. 23, Part 2, pp. 168 – 184.

Longhurst, Sybil et al — Sherborne: A Cotswold Village, by Sybil Longhurst, Walter and Alice Tufnell, Alan Sutton, Stroud, 1992.

McCrea, W. H. "The Significance of the Discovery of Aberration", QJRAS, March, 1963, pp. 41 – 43.

McGuire, J. E. "The Fate of the Date: The Theology of Newton's Principia Revisited" in *Rethinking the Scientific Revolution,* University of Cambridge Press, Cambridge, 2000, pp. 271 – 295.

Malebranche, Nicolas *De la recherché de la verité*. 3 vols. Paris, 1674 – 75.

Malebranche, Nicolas *Traité de la nature et de la grace*, Paris, 1680.

Manfredi, Eustachio *De Annuis Inerrantium Stellarum Aberrationibus,* Bologna, 1729.

Martin, Julian *Francis Bacon, the State, and the Reform of Natural Philosophy,* Cambridge University Press, Cambridge, 1992.

Maskelyne, Nevil "Analysis of Calculations made by Bradley when investigating a Rule for Refraction", in *Supplement to the Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 3 – 16.

Maskelyne, Nevil "Extracts from Dr. Maskelyne's Observations on the Latitude and Longitude of the Royal Observatory at Greenwich", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 75 – 80, and *Philosophical Transactions,* Vol. LXXVII, pp. 154 – 161.

Maskelyne, Nevil *Astronomical Observations, 1766 – 1774, Explanation and use of tables.* London, 1776.

Midgley, Graham *University Life in Eighteenth-Century Oxford,* Yale University Press, London, 1996.

Molieres, J. P. de *Leçons de physique*, Paris, 4 vols. 1734 – 1739.

Molyneux, Samuel "A Description of an Instrument set up at Kew, in Surrey, for investigating the annual Parallax of the Fixed Stars, with an Account of the Observations made therewith", in *Miscellaneous Works and Correspondence of James Bradley*, edited by Stephen Peter Rigaud, Clarendon Press, Oxford, 1832, pp. 93 – 115.

Montgomery, Scott L. *The Scientific Voice,* The Guilford Press, London, 1996.

Newton, Isaac *Isaac Newton's Papers and Letters on Natural Philosophy and Related Documents,* edited by I. Bernard Cohen assisted by Robert E. Schofield, Cambridge University Press, Cambridge, 1958.

Newton, Isaac *Opticks, or a Treatise of the Reflections, Refractions, Inflections and Colours of Light,* with a Preface by I. Bernard Cohen, Dover Publications, New York, 1979.

Newton, Isaac *Philosophiæ Naturalis Principia Mathematica,* London, 1687 (facsimile of 1st ed.) published by Culture et Civilisation, Brussels, 1965.

Newton, Isaac *The Principia: Mathematical Principles of Natural Philosophy,* A new translation of *Philosophiæ Naturalis Principia Mathematica by I. Bernard Cohen and Anne Whitman.* University of Chicago Press, London, 1999.

Newton, Isaac *Unpublished Scientific Papers of Isaac Newton: A Selection from the Portsmouth Collection in the University Library, Cambridge,* chosen, edited and translated by A. Rupert Hall and Marie Boas Hall, Cambridge University Press, Cambridge, 1962.

Newton-Smith, W. H. ed. *A Companion to the Philosophy of Science,* Blackwell Publishers, Oxford, 2000.

Olby, R. C. et al (eds.) *Companion to the History of Modern Science,* edited by R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge, Routledge, London, 1990.

Olesko, Kathryn M. "The Meaning of Precision: The Exact Sensibility in Early Nineteenth-Century Germany" in M. Norton Wise (ed.) *The Values of Precision,* Princeton University Press, Princeton, 1995, pp. 103 – 134.

Olson, Richard — "Tory-High Church Opposition to Science and Scientism in the Eighteenth Century: The Works of John Arbuthnot, Jonathan Swift, and Samuel Johnson" in John G. Burke (ed.) *The Uses of Science in the Age of Newton,* University of California Press, London, 1983, pp. 171 – 204.

Osler, Margaret (ed.) — *Rethinking the Scientific Revolution,* University of Cambridge Press, Cambridge, 2000.

Oster, Malcolm (ed.) — *Science in Europe, 1500 – 1800, A Primary Sources Reader,* edited by Malcolm Oster, Palgrave, Basingstoke, 2002.

Oster, Malcolm (ed.) — *Science in Europe, 1500 – 1800, A Secondary Sources Reader,* edited by Malcolm Oster, Palgrave, Basingstoke, 2002.

Pannekoek, Anton — *A History of Astronomy,* George Allen & Unwin, London, 1961. Reprinted by Dover Books, New York, 1989.

Picard, Jean — *Voyage d'Uranibourg,* Paris, 1671.

Pickering, Andrew — *From Science as Knowledge to Science as Practice,* in *Science as Practice and Culture,* edited by Andrew Pickering, University of Chicago Press, London, 1992, pp. 1 to 26.

Pickering, Andrew — *The Mangle of Practice: Time, Agency and Science,* University of Chicago Press, London, 1995.

Pickering, Andrew (ed.) — *Science as Practice and Culture,* University of Chicago Press, London, 1992.

Picolet, G. (ed.) — *Jean Picard et les debuts de l'astronomie de precision au XVIIe siecle: actes du colloque du tricentenaire.* Paris, 1987.

Pocock, J. G. A. — *Virtue, Commerce and History: Essays on Political Thought and History, Chiefly in the Eighteenth Century,* Cambridge University Press, Cambridge, 1985.

Porter, Roy (ed) — *The Cambridge History of Science: Vol. 4: Eighteenth-Century Science,* Cambridge University Press, 2003.

Porter, Roy — *English Society in the Eighteenth Century,* Revised edition, Penguin Books, London, 1990.

Porter, Roy — *Enlightenment: Britain and the Creation of the Modern World,* Allen Lane, London, 2000.

Porter, R. & Teich, M. — *The Scientific Revolution in National Context,* Cambridge University Press, Cambridge, 1992.

Pugliese, Patri Jones — *The Scientific Achievement of Robert Hooke: Method and Mechanics,* PhD Thesis, Department of the History of Science, Harvard University, 1982.

Pyle, Andrew — *Atomism and its Critics: From Democritus to Newton,* Thoemmes Press, Bristol, 1997.

Radner, Daisie — *Malebranche: A Study of a Cartesian System,* London, 1978.

Rattansi, P. & McGuire, J. — "Newton and the 'Pipes of Pan'" in *Notes and Records of the Royal Society of London,* Vol. 21 (1966), pp. 108 – 143.

Rattansi, P. M. — "Some Evaluations of Reason in Sixteenth- and Seventeenth-Century Natural Philosophy" in *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham,* edited by Mikuláš Teich and Robert Young, Heinemann, London, 1973, pp. 148 – 166.

Redondi, Pietro — *Galileo: Heretic,* translated by Raymond Rosenthal, Penguin Books, London, 1989.

Rigaud, Stephen Peter — *Correspondence of Scientific Men of the Seventeenth Century,* Clarendon, Oxford, 1841.

Rigaud, Stephen Peter — *Memoirs of Bradley,* bound with James Bradley, *Miscellaneous Works and Correspondence of James Bradley,* (ed. by Stephen Peter Rigaud), Clarendon Press, Oxford, 1832.

Rossi, Paolo — *The Birth of Modern Science,* Blackwell Publishers, Oxford, 2001.

Rusnock, Andrea — *Correspondence networks and the Royal Society, 1700 – 1750, BJHS,* Vol. 32, pp. 155 – 169, (1999).

Sabra, A. I. — *Theories of Light from Descartes to Newton,* Cambridge University Press, Cambridge, 1981.

Schaffer, Simon — "Accurate Measurement Is an English Science" in M. Norton Wise (ed.) *The Values of Precision,* Princeton University Press, Princeton, 1995, pp. 135 – 172.

Schaffer, Simon — "Astronomers Mark Time: Discipline and the Personal Equation." in *Science in Context,* Vol. 2.1 (1988), pp. 115 – 145, p. 115.

Schaffer, Simon — "Glass Works: Newton's prisms and the uses of experiment" in David Goodman, Trevor Pinch and Simon Schaffer (eds.) *The Uses of Experiment: Studies in the Natural Sciences,* Cambridge University Press, 1989, pp. 67 – 104.

Schaffer, Simon — "Natural Philosophy" in G. Rousseau and R. Porter (eds.), *The Ferment of Knowledge,* Cambridge University Press, 1980.

Schaffer, Simon — "Newtonianism" in R. C. Olby, G. N. Cantor, J. R. R. Christie and M. J. S. Hodge (eds.), *Companion to the History of Modern Science,* Routledge, London, 1990, pp. 610 – 626.

Schaffer, Simon — "Where Experiments End: Tabletop Trials in Victorian Astronomy", in Jed Z. Buchwald (ed) *Scientific Practice: Theories and Stories of Doing Physics,* University of Chicago Press, London, 1995, pp. 257 – 299.

Schechner, Sara J. — *Comets, Popular Culture, and the Birth of Modern Cosmology,* Princeton University Press, Princeton, 1997.

Shapin, Steven — *The Image of the Man of Science,* in Roy Porter (ed) *The Cambridge History of Science: Vol. 4: Eighteenth-Century Science,* Cambridge University Press, 2003, pp. 159 – 183.

Shapin, S. & Schaffer, S — *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life,* Princeton University Press, Princeton, 1985

Shapin, Steven — *A Social History of Truth: Civility and Science in Seventeenth-Century England,* University of Chicago Press, London, 1994.

Simcock, A. V. — *The Ashmolean Museum and Oxford Science, 1683 – 1983.* Museum of the History of Science, Oxford, 1984.

Simpson, A. D. C. — "Robert Hooke and Practical Optics: Technical Support at a Scientific Frontier", in Michael Hunter and Simon Schaffer (eds.), *Robert Hooke: New Studies,* The Boydell Press, Woodbridge, 1989, pp. 33 – 61.

Sorell, Tom — *Hobbes,* Routledge, London, 1986.

Stanhope, Philip D. — *Letters to his Son,* London, 1752.

Stewart, Larry — *The Rise of Public Science: Rhetoric, Technology, and Natural Philosophy in Newtonian Britain, 1660-1750,* Cambridge University Press, Cambridge, 1992.

Stewart, Larry — "The Selling of Newton", in *Journal of British Studies*, Vol. 25, 1986, pp. 178 – 192.

Struik, D. J. — *A Source Book in Mathematics 1200 – 1800,* edited by D. J. Struik, Princeton University Press, Princeton, 1986.

Sutton, Geoffrey V. — *Science for a Polite Society: Gender, Culture and the Demonstration of Enlightenment,* Westview Press, Oxford, 1995.

Tabarroni, Giorgio — "Eustachio Manfredi" in the *Dictionary of Scientific Biography,* Vol. 9, pp. 77 – 78.

Taton, Rene — "Jacques Cassini", in the *Dictionary of Scientific Biography,* Vol. 3, pp. 104 – 106.

Taton, Rene — "Sur la diffusion des théories Newtoniennes en France: Clairaut et le problème de la figure de la terre", in *Vistas in Astronomy,* Vol. 22: pp. 485 – 509, 1978.

Teich, M. & Young. R. — *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham,* edited by Mikuláš Teich and Robert Young, Heinemann, London, 1973.

Terrall, Mary — *The Man Who Flattened the Earth: Maupertuis and the Sciences in the Enlightenment,* University of Chicago Press, London, 2002.

Terrall, Mary — "Representing the Earth's Shape: The Polemics Surrounding Maupertuis's Expedition to Lapland" in Isis, Vol. 83, No. 2, 1992, pp. 218 – 237.

Terrall, Mary — "The Uses of Anonymity in the Age of Reason" in *Scientific Authorship: Credit and Intellectual Property in Science,* edited by Mario Biagioli and Peter Galison, Routledge, London, 2000, pp. pp. 91 – 112.

Thomson, Thomas — *A History of the Royal Society,* London, 1812.

Tirion, Wil et al — *Uranometria 2000.0,* in 2 Vols. Willmann-Bell, Richmond, 1989.

Toomer, G. J. — *Ptolemy's Almagest,* Duckworth, London, 1984.

Torrance, John (ed.) — *The Concept of Nature: The Herbert Spencer Lectures,* edited by John Torrance, Clarendon Press, Oxford, 1992.

Turner, G. L'E. — "Eighteenth-Century Scientific Instruments and their Makers" in Roy Porter (ed) *The Cambridge History of Science: Vol. 4: Eighteenth-Century Science,* Cambridge University Press, 2003, pp. 511 – 535.

Urbach, Peter — *Francis Bacon's Philosophy of Science: An Account and a Reappraisal,* Open Court, Chicago, 1987.

Van Helden, Albert — "The Birth of the Modern Scientific Instrument, 1550 – 1700", in John G. Burke (ed.) *The Uses of Science in the Age of Newton,* University of California Press, London, 1983, pp. 49 – 84.

Van Helden, Albert — *Measuring the Universe: Cosmic Dimensions from Aristarchus to Halley,* Chicago University Press, 1985.

Vickers, Brian (ed.) — *English Science, Bacon to Newton,* Cambridge University Press, Cambridge, 1987.

Vickery, Amanda — *The Gentleman's Daughter: Women's Lives in Georgian England,* Yale University Press, London, 1998.

Voltaire — *Lettres Philosophique.* Paris, 1733.

Wallis, John — *Opera Mathematica,* Oxford, 1699.

Waters, David W. — "Nautical Astronomy and the Problem of Longitude", in John G. Burke (ed.) *The Uses of Science in the Age of Newton,* University of California Press, London, 1983, pp. 143 – 169.

Westfall, Richard S. — *Never at Rest: A Biography of Isaac Newton,* Cambridge University Press, Cambridge, 1981.

Westfall, Richard S. — "Robert Hooke, Mechanical Technology and Scientific Investigation" in John G. Burke (ed.) *The Uses of Science in the Age of Newton,* University of California Press, London, 1983, pp. 85 – 110.

Westfall, Richard S. — "The scientific revolution of the seventeenth century: the construction of a new world view" in *The Concept of Nature: The Herbert Spencer Lectures,* edited by John Torrance, Clarendon Press, Oxford, 1992, pp. 63 – 93.

Williams, M. E. W. — *Attempts to Measure Annual Stellar Parallax: Hooke to Bessel,* PhD Thesis, University of London, 1981.

Williams, M. E. W. — "Flamsteed's alleged measurement of annual parallax for the Pole Star", JHA, Vol. X (1979), pp. 102 – 116.

Wilson, Curtis — "Astronomy and Cosmology" in Roy Porter (ed) *The Cambridge History of Science: Vol. 4: Eighteenth-Century Science,* Cambridge University Press, 2003, pp. 328 – 353.

Wilson, Margaret D. — *Ideas and Mechanism: Essays on Early Modern Philosophy,* Princeton University Press, Princeton, 1999.

Wise, M. Norton (ed). — *The Values of Precision,* Princeton University Press, Princeton, 1995.

Woolf, Harry — *The Transits of Venus: A Study of Eighteenth-Century Science.* Princeton University Press, Princeton, 1959.

Wren, S. — *Parentalia; or memoirs of the family of the Wrens,* London, 1750.

Yeomans, Donald K. *Comets: A Chronological History of Observation, Science, Myth and Folklore,* John Wiley, Chichester, 1991.

## A List of Primary Sources associated with James Bradley and held at the Department of Western Manuscripts at the Bodleian Library at Oxford.

Bradley MS. 1
Bradley's notebook for lectures at Oxford.
46 folios. Written 1730 to 1750. In English.
Was formerly designated 16404.

Bradley MS. 2
An Account of James Bradley taken from the *Eloge*.
32 folios. Written soon after 1762. Trans. from French into English.
Was formerly designated 16405.

Bradley MS. 3
Lectures on "The more known properties of the Air established..." Plus notes on members attending his lectures.
198 folios. Written 1745 to 1760. In English.
Was formerly designated 16406.

Bradley MS. 4
Lectures on Specific Gravity, Optics, etc...
64 folios. Written 1745 to 1760. In English.
Was formerly designated 16407.

Bradley MS. 5
Lectures on Pneumatics, Tides, Optics and Astronomy.
95 folios. Written 1745 to 1760. In English.
Was formerly designated 16408.

Bradley MS. 6
Lectures on Optics.
30 folios. Written 1745 to 1760. In English.
Was formerly designated 16409.

Bradley MS. 7
Lectures on Mechanics.
38 folios. Written 1745 to 1760. In English.
Was formerly designated 16410.

Bradley MS. 8
Lectures on Motion.
39 folios. Written 1745 to 1760. In English.
Was formerly designated 16411.

Bradley MS. 9
Lectures on the Density of Fluids, and on Imparted Motion.
37 folios. Written 1745 to 1760. In English.
Was formerly designated 16412.

Bradley MS. 10
On Hydrostatics, partly "by Mr. Saunderson", and on the Magnet.
100 folios. Written 1745 to 1760. In English.
Was formerly designated 16413.

Bradley MS. 11
On Natural Phenomena.
85 folios. Written 1745 to 1760. In English.
Was formerly designated 16414.

Bradley MS. 12
An abridgement of Lectures 1 to 5 (part) on the Properties of Bodies and Mechanics, delivered in Oxford in 1747. (Not in James Bradley's hand).
36 folios. Written 1745 to 1747. In English.
Was formerly designated 16415.

Bradley MS. 13
An abridgement of Lectures 10 to 19 on Optics and Hydrostatics, and for Lecture 5 (in part). (for Lectures 6 to 9 see Bradley MS. 17).
36 folios. Written 1745 to 1760. In English.
Was formerly designated 16416.

Bradley MS. 14
The Kew Observation Book. K14.
141 folios. Written 1720 to 1727. In English and Latin.
Was formerly designated 16417.

Bradley MS. 15
Observations on the fixed stars made at Wanstead from 1727 to 1747.
111 folios. Written 1727 to 1747. In English.
Was formerly designated 16418.

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| Bradley MS. 16 | Tables of Astronomical Data – Sun, Moon, Jupiter and its Satellites.<br>184 folios. Written 1720 to 1721. In English and Latin.<br>Was formerly designated 16419. |
| Bradley MS. 17 | Papers relating to James Bradley's lectures on natural philosophy at Oxford from 1740 to 1760. (Not complete).<br>166 folios. Written 1740 to 1760. In English.<br>Was formerly designated 16420. |
| Bradley MS. 18 | Notes by James Bradley about annuities, interest and income from professorship in 1724. Lectures etc. Miscellaneous astronomical observations.<br>45 folios. Written 1717 to 1762. In English.<br>Was formerly designated 16421. |
| Bradley MS. 19 | Latin Lectures delivered at Oxford by James Bradley. It includes his "*Oratio inaugurales*", 26th April, 1722.<br>191 folios. Written 1722 to 1733. In Latin.<br>Was formerly designated 16422. |
| Bradley MS. 20 | Tables of aberration and angles of position.<br>29 folios. Written 1730 to 1755. In English.<br>Was formerly designated 16423. |
| Bradley MS. 20* | Drafts of Papers by James Bradley on Aberration.<br>104 folios. Written 1729 to 1750. In English and Latin.<br>Was formerly designated 16424. |
| Bradley MS. 21 | A book of observations made at Wanstead by James Pound and James Bradley. It includes a rough "Oxford Observation Book".<br>190 folios. Written 1718 to 1740. In English.<br>Was formerly designated 16425. |
| Bradley MS. 22 | Dr. Bradley's papers including those belonging to Dr. Pound. Astronomy, Natural Philosophy and Mathematics. Includes James Bradley's early work at Wanstead.<br>108 folios. Written 1715 to 1724. In English and Latin.<br>Was formerly designated 16426. |
| Bradley MS. 23 | James Pound's Account Book 1715 to 1724, with some receipts and bills.<br>110 folios. Written 1715 to 1724. In English.<br>Was formerly designated 16427. |
| Bradley MS. 24 | James Pound's reports of his voyages in the Indies, etc.<br>181 folios. Written 1705. In English.<br>Was formerly designated 16428. |
| Bradley MS. 25 | Calculations and determinations of the orbital elements etc., of the Comets of 1723, 1742, 1748 and 1757.<br>323 folios. Written 1723 to 1759. In English.<br>Was formerly designated 16429. |
| Bradley MS. 26 | Computations relating to the Sun 1743 to 1758.<br>197 folios. Written 1743 to 1758. In English.<br>Was formerly designated 16430. |
| Bradley MS. 27 | Computations relating to the Moon 1722 to 1759.<br>653 folios. Written 1722 to 1759. In English.<br>Was formerly designated 16431. |
| Bradley MS. 28 | Determination of Longitude by Observations at sea by Captain Campbell, 1757 to 1759.<br>73 folios. Written 1756 to 1760. In English.<br>Was formerly designated 16432. |
| Bradley MS. 28* | Calculations concerning the Precession of the Equinoxes. Absolute and Relative Motion of Certain Fixed Stars compared with Flamsteed's Observations.<br>54 folios. Written 1746 to 1760. In English.<br>Was formerly designated 16433. |

Bradley MS. 29
Observations and Calculations by James Bradley of the Fixed Stars.
441 folios. Written 1746 to 1760. In English.
Was formerly designated 16434.

Bradley MS. 30
Catalogus Stellarum. Not in James Bradley's hand.
34 folios. Written 1720. In Latin.
Was formerly designated 16435.

Bradley MS. 31
James Bradley's Observations at Greenwich with two Transit Instruments from 25th July, 1742 to January 1742/43. Lord Macclesfield's Similar Observations at Shirburne, September to December 1742, and March 1743.
44 folios. Written 1742 to 1743. In English.
Was formerly designated 16436.

Bradley MS. 32
Miscellaneous Astronomical Observations by James Bradley, including copies of some by Edmond Halley, and Observations by Charles Green of an Eclipse, May 1762, and of an Eclipse in Oxford in 1732.
73 folios. Written 1740 to 1760. In English.
Was formerly designated 16437.

Bradley MS. 33
Miscellaneous Astronomical Observations and Computations.
91 folios. Written 1740 to 1760. In English.
Was formerly designated 16438.

Bradley MS. 34
Reduction of the Wanstead Observations.
22 folios. Written 1740 to 1747. In English.
Was formerly designated 16439.

Bradley MS. 35
Tables of Jupiter's Satellites. Copies of some foreign observations.
102 folios. Written 1735 to 1760. In English and Latin.
Was formerly designated 16440.

Bradley MS. 36
Early Observations and Calculations 1719 to 1721. The Parallax of the Sun and Mars.
33 folios. Written 1719 to 1721. In English.
Was formerly designated 16441.

Bradley MS. 37
Complete Observations of Pendulums. In Wanstead in 1719. At Jamaica by Mr Campbell and Mr Harris in 1732 and at Greenwich from 1743 to 1744. Drafts of James Bradley's paper to the Royal Society in 1733.
113 folios. Written 1719 to 1750. In English.
Was formerly designated 16442.

Bradley MS. 38
A few observations and calculations by James Bradley. Aurora Borealis. Variations of the Compass 1729 to 1731.
43 folios. Written 1727 to 1731. In English.
Was formerly designated 16443.

Bradley MS. 38*
A description of an instrument set up at Kew. Samuel Molyneux's original draft.
43 folios. Written 1725 to 1727. In English.
Was formerly designated 16444.

Bradley MS. 39
A description of an instrument set up at Kew. James Bradley's copy of Samuel Molyneux's draft.
93 folios. Written 1725 to 1727. In English.
Was formerly designated 16445.

Bradley MS. 40
Papers related to Nutation. Observations, calculations and drafts of the letter to Lord Macclesfield.
110 folios. Written 1725 to 1750. In English.
Was formerly designated 16446.

Bradley MS. 41
The Printed Bill and Act (24 Geo.2, cap 23: AD1751) for regulating the commencement of the year. Notes and tables on the subject by James Bradley.
35 folios. Printed and written 1750 to 1751. In English.
Was formerly designated 16447.

Bradley MS. 42
Observations and calculations on refractions. Observed refractions.
56 folios. Written 1750 to 1754. In English.
Was formerly designated 16448.

Bradley MS. 43 — Miscellaneous Astronomical Tables by James Bradley. Tables of the Sun's Declination. Mean Precession in Longitude, etc.
93 folios. Written 1730 to 1760. In English and Latin.
Was formerly designated 16449.

Bradley MS. 44 — Correspondence to James Bradley and his assistants.
151 folios. Written 1732 to 1761. In English, Latin and French.
Was formerly designated 16450.

Bradley MS. 45 — Correspondence to James Bradley and his assistants.
72 folios. Written 1732 to 1761. In English, Latin and French.
Was formerly designated 16451.

Bradley MS. 46 — Papers in print prior to 1829. Not written by James Bradley.
56 folios. Written 1740 to 1760. In English.
Was formerly designated 16452.

Bradley MS. 47 — Miscellaneous papers. Not written by James Bradley.
155 folios. Written 1732 to 1766. In English, Latin and French.
Was formerly designated 16453.

Bradley MS. 48 — Notes of John Whiteside's Opticks. The 17$^{th}$, 18$^{th}$ and 19$^{th}$ of a course of mathematical lectures and experiments.
64 folios. Written 1720 to 1725. In English.
Was formerly designated 16454.

## The Archives of the Royal Greenwich Observatory: The Papers of James Bradley, Astronomer Royal, 1742 – 1762, held at the Department of Manuscripts at the Cambridge University Library.

RGO 3 MS. 1 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1743. Working copy.

RGO 3 MS. 2 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1744 to 1746. Working copy.

RGO 3 MS. 3 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1746 to 1749. Working copy.

RGO 3 MS. 4 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1749 to 1750. Working copy.

RGO 3 MS. 5 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1750 to 1755. Working copy.

RGO 3 MS. 6 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1755 to 1758. Working copy.

RGO 3 MS. 7 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1758 to 1762. Working copy.

RGO 3 MS. 8 Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1743 to 1744. Working copy.

RGO 3 MS. 9 Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1744 to 1747. Working copy.

RGO 3 MS. 10 Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1747 to 1753. Working copy.

RGO 3 MS. 11 Observations, using the Quadrants, of major stars; including adjustments to the telescope and observational method.
Written 1750 to 1757. Working copy.

RGO 3 MS. 12 Observations, using the New Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1753 to 1757. Working copy.

RGO 3 MS. 13 Observations, using the New Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1758 to 1762. Working copy.

RGO 3 MS. 14 Observations, using the New Quadrant, of transits of major stars, 1762 – 1765, also transits of major stars, 1764 – 1765, as observed by Charles Green, assistant to Nathaniel Bliss. Included are notes on adjustments to the telescope and observational method and compiled tables for the following items,

1. Daily rate of the transit clock, deduced from the transits of the fixed stars, 1762 – 1764.
2. Calculated mean times of the moon's passing the meridians, 1762 – 1765 (added to the volume in 1861).
3. Apparent right ascensions of the planets, 1762 – 1765.
4. The lunar eclipse of 17th March, 1764.
5. Quadrant observations of the major stars by Charles Green, 1764 – 1765.

Written 1762 to 1765. Working copy.

RGO 3 MS. 15 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1743. Fair Copy of RGO 3 MS. 1.

RGO 3 MS. 16 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1744 to 1746. Fair Copy of RGO 3 MS. 2.

RGO 3 MS. 17 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1746 to 1749. Fair Copy of RGO 3 MS. 3.

RGO 3 MS. 18 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1749 to 1750. Fair Copy of RGO 3 MS. 4.

RGO 3 MS. 19 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1750 to 1755. Fair Copy of RGO 3 MS. 5.

RGO 3 MS. 20 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1755 to 1758. Fair Copy of RGO 3 MS. 6.

RGO 3 MS. 21 Observations: Transits of major stars including adjustments to the telescope and observational method.
Written 1758 to 1762. Fair Copy of RGO 3 MS. 7.

RGO 3 MS. 22 Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1743 to 1744. Fair Copy of RGO 3 MS. 8.

RGO 3 MS. 23 Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1744 to 1747. Fair Copy of RGO 3 MS. 9.

RGO 3 MS. 24 Observations, using a Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1747 to 1753. Fair Copy of RGO 3 MS. 10.

RGO 3 MS. 25 Observations, using the Quadrants, of major stars; including adjustments to the telescope and observational method.
Written 1750 to 1757. Fair Copy of RGO 3 MS. 11.

RGO 3 MS. 26 Observations, using the New Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1753 to 1757. Fair Copy of RGO 3 MS. 12.

RGO 3 MS. 27 Observations, using the New Quadrant, of major stars; including adjustments to the telescope and observational method.
Written 1758 to 1762. Fair Copy of RGO 3 MS. 13.

RGO 3 MS. 28 Observations: transits of major stars 1762 – 1765, including adjustments to the telescope and observational method and also transits of major stars observed by Charles Green, 1764 – 1765.
Written 1762 to 1765. Fair Copy of RGO 3 MS. 14.

RGO 3 MS. 29 Observations, using the New Quadrant, of major stars, 1762 – 1765; including adjustments to the telescope and observational method, and also Quadrant observations of major stars, 1764 – 1765, taken by Charles Green.
Written 1762 to 1765. Fair Copy of RGO 3 MS. 14.

RGO 3 MS. 30 Observations made with the Zenith Sector, received by Nevil Maskelyne, 11th March, 1770 from William Wales. It includes a Latin piece on the orbits of Jupiter's satellites and contains adjustments to the telescope and observational method.
Written 1749 to 1760. Working copy.

RGO 3 MS. 31 A miscellaneous collection of handwritten notes; they include:

1. A note dated 28th June, 1734 on the error's of Halley's Quadrant from experiments performed by Bradley.
2. A note dated 26th July, 1735 correcting the errors of the Quadrant and the previous year's over-estimated calculations.
3. A note dated 6th July, 1742 on correcting errors in the Quadrant and repairs to the telescope.
4. A note dated 8th July, 1742 comparing the Quadrant's present position with its adjustments in 1726.
5. An undated note probably 1742. Describes Halley's observational technique. Explains improvements made to aid observations in difficult weather and refine the results taken.
6. Notes on the alterations to the line of collimation, 1744 – 45.
7. A Bound Manuscript. It contains:
   a. Observations of *Polaris* and other stars.
   b. Tests on the new quadrant.
   c. Observations made with the New Transit Telescope and a description of its size and location.
   d. A law of refraction and its calculations.
   e. A note on the replacement of instruments from a £1,000 grant.
   f. Quadrant and Zenith Sector calculations.
8. Errors of solar tables, 1743 – 1756.

Written 1734 to 1756.

RGO 3 MS. 32 Various loose papers in a file. They include:

1. Bradley's table of aberration of certain fixed stars, 1746 – 1753.
2. Adjustments made to the telescope, 18th July 1745.
3. Observations made of Wanstead Tower and St. Paul's, 1746.
4. Various unprovenanced calculations, some for the period 1689 – 1691.
5. Calculations written on a Royal Society invitation to Bradley, 20th June, 1757.
6. A table of observations of the moon in Gemini made by the New Quadrant, 1753 – 1756.
7. Tables and observations relating to the Quadrant, 1745 – 1746.
8. Tables relating to the Transit Telescope, 1743.
9. Papers referring to the occultations of Jupiter, 6th June, 1744 observed at Oxford and Shirburne. Observations of Jupiter and Mercury from 1743 – 1749.
10. Notes establishing the index from the Quadrant, 1745 – 1753.
11. Calculations and tables to ascertain the zenith from the Old and New Quadrants, 1746 – 1753.
12. Collimation error of the Quadrant 1742 – 1752.
13. Unprovenanced chart giving the positions of indeterminable stars.

14. Errors of the planes of the Quadrant at various times from 1742 to 1745. A bound copy of "Comparisons between the arc of the sector and Quadrants", with diagrams and equations, 1750 – 1758.
15. Errors and rates of clocks, 1743 – 1748. There is part of a letter concerning the building of an observatory.
16. Various data relating to adjustments taken on the Quadrant.
17. Observations with the Zenith Sector, 1749 – 1758.

Written 1689 – 1758.

RGO 3 MS. 33 Calculations relating to observations of the Moon.
Written 1752 – 1762.

RGO 3 MS. 34 Ecliptic tables of Jupiter's satellites from 1700 to 1739 compiled by Bradley.
Written 1719 – 1739.

RGO 3 MS. 35 Sun's mean right ascension and longitude compared with tables, 1743 – 1758.
A table of mean right ascension of stars in 1758.
Written 1743 – 1758.

RGO 3 MS. 36 A Catalogue of Stars as observed by John Flamsteed reduced to 1744. A Table of Lunar Parallaxes.
Written 1744. Fine Copy.

RGO 3 MS. 37 "Index to the days on which every observation of each star, with the Quadrants, was made by Bradley" or Working Catalogue marked off for the Quadrant observation 0hrs to 12hrs.
Written 1753 to 1757.

RGO 3 MS. 38 "Index to the days on which every observation of each star, with the Quadrants, was made by Bradley" or Working Catalogue marked off for the Quadrant observation 12hrs to 24hrs.
Written 1753 to 1757.

RGO 3 MS. 39 "Index to the days on which every observation of each star, with the Transit, was made by Bradley" or Working Catalogue marked off for the Transit observation 0hrs to 12hrs.
Written 1750 to 1756.

RGO 3 MS. 40 "Index to the days on which every observation of each star, with the Transit, was made by Bradley" or Working Catalogue marked off for the Transit observation 12hrs to 24hrs.
Written 1750 to 1756.

RGO 3 MS. 41 Computations to ascertain the Moon's position by observations and to find the apparent diameter of the Moon. The Rules to Compute the Aberration of any Star.
Written 1742 – 1744.

RGO 3 MS. 42 A collection of tables:

1. Refraction tables; Lord Macclesfield's observations.
2. Reduction of micrometer.
3. Tables of Jupiter's satellites (in Latin).
4. Lunar Tables (in Latin).
5. Solar Tables.
6. Lunar Tables.
7. Tables of the nonagesimal (on the Quadrant).

Written 1745.

RGO 3 MS. 43 Miscellaneous Observations: this is a bound volume of calculations, manuscripts and letters from 1723 to 1804, divided up into eight sections.

1. Notes on the handwriting of Bradley's manuscripts.
    a. Explanation of observation code for 1743. A – Other observers. B – Bradley.
    b. A list of observations, 1743 – 1760.
2. Quadrant observations for 1750.
3. Deductions from Bliss's observations and the Board of Longitude.
    a. Mean time of the Moon's passage with latitude and longitude from the 30th of August 1762 to the 2nd of March 1765.
    b. Apparent right ascensions of the planets from the 29th of August 1762 to the 2nd of March 1765.
    c. Daily rate of the Transit Clock form the 27th of August 1762 to the 2nd of March 1765.
    d. Minutes of the Board of Longitude, the 1st of March 1804 concerning the acquisition of the above data, which was observed by Nathaniel Bliss and Charles Green.
4. Observations of eclipses 1748 – 1751.
    a. Observations of a lunar eclipse on the 9th of March, 1751.
    b. Observations of an eclipse on the 14th July, 1748
    c. Observations of a lunar eclipse on the 12th of December, 1749.
    d. Observations of a lunar eclipse on the 8th of June, 1750.
5. Miscellaneous Observations and Calculations 1742 – 1757.
    a. Mathematical equation to find the relative position of the Sun and the Moon with sketches and calculations.
    b. Rule and equation concerning the Sun's declination.
    c. Observation of an eclipse, 14th July, 1748, and calculations relating to that observation.
    d. Adjustments to the line of collimation, with a detailed explanation of the procedure, 1745.
    e. Improvements in illuminating the wires of the Quadrant and Transit telescopes by Bradley as well as other general alterations. Undated but probably 1742. See RGO 3 MS. 31. 5.
    f. Invitation to attend the Royal Society, 20th June, 1757, with unprovenanced calculations on the reverse.
    g. Establishing the distances of meridians at Shirburne and Oxford from Greenwich 6th June, 1744.
    h. Observations of Jupiter and Mercury, 1744 – 1745.
    i. Observations of *Polaris.* 1745 – 1746.
    j. Observations of Jupiter and Mars at Oxford, 21st May, 1743. See RGO 3 MS. 8.
    k. Unknown column of monthly figures in "Time for 13 1/3 in Azimuth".
    l. Column of figures for "The Difference in Time for 13 1/3 in Azimuth".
    m. Tables for reducing 96/90 of Quadrant to degrees, 1750 – 1755.
    n. Transit and zenith observations, southwards, 1751 – 1752.
    o. Unknown rough calculations.
    p. Computation of Sun's error of right ascension, September 1749.
    q. Transit observations, 1754.
    r. Transit observations, 1752.
    s. Computation of the Sun's passage for transit observations.
    t. Part of a letter from Francis Rowden concerning the building of an observatory. See RGO 3 MS. 32. MS. 15.
    u. Transit observations, 1743 – 1746, to establish the zenith.
    v. A bill for food. 1743 – 1746.

w. A table of stars passing through the threads of the Transit Telescope.
x. Quadrant observations, 1743.
y. Measurement of the Sun by clocks, 1743 – 1744.
z. Quadrant observations, 1743 – 1745.
aa. Tables relating to the zenith, 1743.
bb. Observations of stars with regular daily intervals between recording observations, 1744 – 1748.
cc. Table of zenith distances and errors, 1742.
dd. Mean difference in right ascension between Orion and Aquila, 1st January, 1743.
ee. Positions of major stars, 1743.
ff. Observations of stars with regular daily intervals between recording observations, 1743 – 1748.

6. Papers principally on comets, 1723 – 1759.
   a. "Observations upon the Comets that appeared in the months of September and October 1757" with reference to unnamed stars and observational technique including an acknowledgement to Dr Smith's three volumes on optics.
   b. Observations to ascertain the 1757 comet's right ascension, its position relative to the Earth and other stars and its declination, with attended calculations.
   c. Table of observations of the 1759 comet.
   d. Table of observations of the 1759 – 1760 comet, fixing its relative position with reference to the 1744 comet.
   e. Notes on the position of the 1744 comet within the constellation Pegasus.
   f. Notes on the position of the 1748 comet.
   g. Preface for a paper on the comets of 1743 – 1744. It mentions the observations at Shirburn.
   h. Two identical diagrams of planes.
   i. Large drawing of arcs and planes.
   j. Observations of the 1743 comet establishing its right ascension and position relative to the constellation Pegasus.
   k. Chart of the position of the comets of 1743 – 1744.
   l. Comments on the position of the comets of 1743 – 1744.
   m. Computations on the comet by Mr Gael Morris, 1743.
   n. A letter from Mr John Bradley on his computations of the comet at Greenwich, 1744.
   o. Observations of the comet by Earl Macclesfield, 1744.
   p. Observations of the comet at Shirburne with associated tables, 1744.
   q. Comparative table of observations between Gael Morris and John Bradley.
   r. Notes on the orbit of the comet of 1744.
   s. Hypothesis in the handwriting of Nevil Maskelyne on the orbit of the 1759 comet, with calculations.
   t. Problem to find the position of a ship, with diagram.
   u. Position of the 1719 comet from appearance to disappearance.
   v. Short note on the position of the 1723 comet with reference to the comet of 1707 – 1708, as observed at Bologna.
   w. A theory in Latin with an equation on the orbit of the planets, including calculations.
   x. Observations of the 1757 comet.

y. Calculations and equations to find the trajectory of a comet and find its presumed positions in 1742.
z. Observations of a comet 1727.
aa. Observations of a comet in 1742 by Gael Morris.
bb. Observations of a comet in 1742 by Mr. Wright.
cc. Observations of a comet, 1738 – 1739.
dd. A letter from Gael Morris amending his computations of the comet.
ee. Kepler's problem resolved by John Machin and probably addressed to "Mr. Jones". Copied by James Bradley. See *Philosophical Transactions,* Vol. 40, p.205.
ff. Letter from an astronomer, Julius Ascetto of Turin, written in English, enquiring into his observations of the Moon, 1750.
gg. Letter from Bradley to an unknown person stating he will be in Oxford, 1742.
hh. Calculations on a letter to Bradley.
ii. Memoranda on alterations and repairs to the telescope, 1742 – 1743.
jj. The plotted eclipse of the Sun, 1737.
kk. Observations of a comet, 1748.
ll. Various unprovenanced calculations.
mm. Explanation of the generation of the parabola, with calculations.

7. Early computations, mainly cometary.
   a. Chart of cometary observations, 1723.
   b. Several pages of calculations.
   c. "The Places of the Telescopic Stars from which the Comet was Observed"; a chart, 1723.
   d. An equation to solve a problem on parabolas.
   e. An account of the observations of a comet by Maraldi at Paris, 1723. It mentions the 1707 cometary sighting.
   f. Several pages of calculations.
   g. Equation and method to solve a problem involving parabolas.

8. Letters from various persons to Bradley.
   a. Letter to Bradley on atmospheric pressure with two examples of evidence provided by the author for his argument. Also, a piece on the origins of springs and rainfall by an unknown admirer "Academicus".
   b. Equation on a letter to Mrs Paine of Wanstead, Essex.
   c. Letter to Mrs Woodam from Matthew Wymondesolde on travelling arrangements, 1729. On the reverse is a note on gravity.
   d. Letter from Gael Morris requesting information on the latitude of the Moon whilst in occultation with Jupiter. See *Philosophical Transactions* Vol. p.674.
   e. Letter from Matthew Wymondesolde on travel arrangements to Dorchester, Dorset from Wanstead, Essex, 1741.
   f. Letter from Gael Morris on the position of the Moon by observing two stars, 1742.
   g. Letter from Thomas Corbett requesting the pleasure of Dr Bradley's company.
   h. Letter from John Bradley explaining his lack of success in observing the comet, 1743.
   i. Various unprovenanced calculations.

j. Letter from John Bradley on general conditions for his observations, 1743.
k. Letter from Mr Bevis wishing to compare observations of Mercury's late transit, 1742. It includes his observational technique and his findings.
l. Letter from John Periam of Somerset. It makes several notes on comets deduced from his observations, and their religious connotations.
m. Letter from Richard Bates of Cardiff asking for information to improve his line of collimation, 1743.
n. Another letter from Richard Bates of Cardiff on his observations of Mercury and asking for Bradley's advice on the construction of a grid iron pendulum, 1746.
o. Request for figures of the Moon's distance from the Greenwich Meridian by Joseph Pollock on behalf of the 2nd Earl of Macclesfield, 1749. There is an equation on light rays on the reverse.
p. Letter in Italian from Julius Ascetto of Turin on the recent lunar eclipse, 1750.
q. Letter covering the enclosed papers on astronomy researched by Mr Rogers of Bolton, Lancashire, 1753.
r. A long letter from Richard Heaton on the position of the poles and the implications of his computations on lines of longitude and latitude for the benefit of navigators, 1752.
s. Another lengthy letter from Richard Heaton concerning his invention of the planisphere with its implications for the safety of sailors and a direct comparison with the state of science in the mid 18th century with as it was in classical Greece, 1752.
t. Covering letter from Emanuel Mendes de Costa, London, on meteorological observations and lunar and solar eclipses. 1753.
u. Letter from Gael Morris concerning zenith distances, 1754.
v. Letter from Thomas Cooke who analyses the differences between physical laws and the natural order as laid down by God, 1754.
w. Piece on the exact time for the Earth to orbit the Sun.
x. Correspondence from Thomas Cooke of Oxford on the conflict between astronomy and theology, 1756.
y. Letter from John Kennedy requesting information on the exact time the Earth orbits the Sun, 1755.
z. Letter from Mr M. Roper requesting observations before June 1743 and highlighting the errors in his own observations at that time, 1755.
aa. Equation and method to find the Earth's orbit around the Sun.
bb. Correspondence from James Stewart Mackenzie of Ham, Surrey, disputing Bradley's calculations for measuring the altitude of the Sun, 1757.
cc. Letter from Contianus Liber on observations and comments on the Milky Way, 1759.
dd. Letter from Thomas Stevenson on the observations of Halley's Comet from Barbados in 1759. It provides details of the comet's orbit and predicts its re-appearance for 1833. There is a table of returns of Halley's Comet, 1305 – 1833.
ee. Further observations of Halley's Comet by Thomas Stevenson from Barbados in 1759 with details of its position and the technique employed to trace the comet's path. It mentions the comet in relation to an "eclipse", really an occultation, of Jupiter by the Sun.

ff. A request from John Fletcher of Halling, Gloucestershire, to discuss with Bradley the calorific efficiency of fires, 1760.
gg. Letter from Christopher Irwin concerning the Sun's orbit in conjunction with an eclipse, 1760.
hh. Letter from Stephen Hales of Teddington requesting that Bradley act as an intermediary between him and a rich heiress, Mrs Mathews of Petersham, Richmond.
ii. Letter from Samuel Smedhurst of Manchester on the transit of Venus and the difference in longitude of Manchester and London, 1762.
jj. Comparative study of observations of an eclipse of Jupiter taken at London and Manchester, 1761.

RGO 3 MS. 44 Various works and correspondence for the period 1764 – 1774. These are as follows:-

1. Eclipse of the Moon, 1st March 1764.
2. Eclipse of the Sun, 1st April 1764.
3. Observations on the transits of Venus 1769; including an eclipse of the Sun observed at Gibraltar 1769, and a bill for crockery.
4. An eclipse of the Sun, 4th June, 1764.
5. Eclipses of Jupiter's satellites, 1772 – 1774.
6. Eclipse of the Moon 12th December. (Undated; List of eclipses gives 13th December 1769).
7. Drawing of a solar eclipse.
8. Table of observations of the comet of Mr Napier 1766. See *Philosophical Transactions*, Vol. 56, p.62, with computations and sketches.
9. Eclipses of the Sun and the Moon 1769 – 1770, as observed at Hawkhill. Scotland.
10. Latitudes of Hawkhill from the meridian altitudes of the Sun. 1770.
11. Transit observations at Hawkhill.
12. Meridian altitude of Cassiopeia for finding the error of the Quadrant.
13. Observations and calculations on the orbit of comets, 1769 – 1770.
14. Observations of an eclipse of Jupiter.
15. Calculations to find the distance of Ben Lomond from Hawkhill.
16. Short note on reflecting telescopes.
17. Reference on behalf of Thomas Roy.

RGO 3 MS. 45* Miscellaneous Observations 1755 – 1763.

1. Observations made with the moveable brass Quadrant, 5th December 1755 to 27th December 1760. Notes on the observational technique and the clocks employed.
2. Observations with the Transit Instrument from 31st July 1760 to 4th December 1763. Notes on the observational technique and the clocks employed.
3. Rough calculations.

*These papers were placed in RGO 3 MS. 45 after being presented to the Royal Greenwich Observatory by the Radcliffe Trustees in 1934.

## Miscellaneous Manuscript Sources.

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|---|---|
| Keynes MS. 130.5 | King's College Library, Cambridge, John Conduitt, *Memorabilia.* |
| | Cambridge University Library, Microfilm MS. 659. |
| | Gloucestershire Records Office, Gloucester. Sherborne Parish Records, |
| | Gloucestershire Records Office, Gloucester. Rendcomb Parish Records, |
| FISh. | Royal Society Library, Sharp Letters. |
| | Christ Church, Oxford. William Wake Letters, |
| | Bodleian Library, Department of Western Manuscripts, Oxford. Thomas Hearne Papers. |
| | Hampshire County Records Office, Southampton. The William Molyneux and Samuel Molyneux Papers. |
| Add. MSS. 4478D, | British Library, *Dr Birch's Common Place Book.* |