Edmond Halley

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Preface

EDMOND HALLEY has often been esteemed second only to Isaac Newton among the British men of science of his time. His long life of public service, his bold and wide-ranging speculations, his profound scholarship, his heroic adventures and the impact of his robust personality upon his contemporaries have all combined to stir the imagination of posterity and to invest his career with dramatic interest unsurpassed in the annals of discovery. Yet no adequate biography of Halley has ever been published. In the two centuries and over that have elapsed since his death, not a few have taken in hand to relate the full story of his life and labours, but none has been spared to complete the task.

The present volume does not claim to be the definitive biography which must surely be written some day and which will take full account of every scrap of relevant manuscript preserved in a dozen libraries. What has been attempted in the following pages is essentially an historical evaluation (more elaborate than any previously undertaken) of Halley's scientific researches, particularly as these are presented in the papers (numbering more than eighty) that he contributed to the *Philosophical Transactions*, and with some account of his life story.

In organizing this material into a book, one is faced with the problem of how best to bring the life story and the researches into perspective. Sometimes a scientist's classic investigations represent self-contained episodes that can be caught up into his biography at the appropriate points. Thus Newton's scientific career has been conceived as falling into successive decades of preoccupation with optics, fluxions, gravitation. Or, as with Herschel, the life story may be almost completely irrelevant to the researches and can be presented as a continuous narrative while the researches receive separate and systematic treatment. With Halley, no such simplifications are possible. For over sixty years he poured out discourses, papers, and books on an indiscriminate variety of topics, and to discuss these productions in the order of their dates of publication would only create confusion. On the other hand, his communications were generally inspired by what he was doing at the time, and they cannot be understood if merely grouped according to subject and studied in isolation. Some sort of compromise is necessary. Now, as we pass from Halley's youth into his later years, information about his personal life becomes less abundant (or at least I have discovered less of it). It has therefore seemed best to begin this book in a conventional way but to lay progressively more stress upon the researches, and to group these with some regard both to date and to subject-matter, inserting biographical particulars where these could throw most light on the researches.

I would here offer my grateful acknowledgments to the various authorities who have granted me permission to reproduce copyright material: in particular, to the Royal Society, for allowing me to quote or summarize passages from the *Philosophical Transactions*, from the *Journal Book* and from unpublished papers in their possession; to the Trustees of the National Maritime Museum for permission to examine and make use of a file of documents relating to Halley's nautical activities, and to Messrs Taylor and Francis Ltd, of London, for permission to quote or summarize material contained in E. F. MacPike's *Correspondence and Papers of Edmond Halley*, published by them in 1937. Crown copyright material in the Public Record Office relating to Halley's voyages has been reproduced by permission of the Controller of H.M. Stationery Office. I am also indebted for permission to reproduce illustrations (as indicated) to the Royal Society, the Science Museum, the Trustees of the British Museum and of the National Maritime Museum, and the Ronan Picture Library. I am indebted to Lt.-Comdr. J. Dickson, R.N.R., for valuable information on Halley's nautical activities.

I desire to thank the Editor of the Series, Sir Gavin de Beer, F.R.S., for his advice and encouragement, and the executives of the Publishers for their courtesy.

A.A.

Chapter 1

Introduction

1 *Halley and his Age*

This book is an attempt to sketch the career and to explain the scientific achievements of Edmond Halley, whose genius, exercised in many different fields of thought and action, added lustre to the age that saw modern science launched upon its momentous course. Halley claims a high place in the roll of British men of science. He made notable contributions to astronomy and physics; he was an accomplished mathematician, a pioneer in geophysics and in demography, a learned Arabist, and an intrepid navigator. He was a friend and counsellor of Isaac Newton, whose immortal *Principia* he edited; and he rendered long and faithful service to the Royal Society during its formative years. Born in the Protectorate of Oliver Cromwell, he passed his youth and early manhood under the restored monarchy of Charles II and prolonged his days into the Hanoverian period, dying at a good old age in the reign of George II. His active career thus spanned the period during which physical science in Britain developed from a Baconian adventure into a scheme of interlocking concepts and theories destined to enjoy unchallenged acceptance up to the beginning of the present century. Halley lived, in fact, through one of the crises of what is usually called the Scientific Revolution. This was the movement that transformed the prevailing outlook of medieval Christendom upon man and the Universe into something approaching the set of presuppositions with which the modern mind confronts the course of nature. In reviewing Halley's particular contributions to the many problems that attracted his interest, we shall have to take account of how things stood at that period with each of the branches of science concerned. This introductory chapter, however, may afford an opportunity for setting the scene with some more general remarks on the state of scientific knowledge and speculation at the time when Halley was entering upon his labours.

Science, the quest for 'systematized positive knowledge' (in George Sarton's phrase), did not begin with the modern world; the scientific attitude was fully realized in some of the Greek investigators, though it was no more typical of their society than it is universal in ours. The natural fruition of Greek science was arrested by factors that have been variously assessed; but when, centuries later, the quest was taken up again in western Christendom, the pioneers had to start out, as best they could, from where the Greek adventure had ended. The Greek scientific classics were still the best available guides to the subjects with which they dealt, and progress at first depended upon the recovery of these texts and the presentation of them in scholarly editions. This was the essential contribution of the Humanists of the Renaissance, though several mathematical classics remained to claim the editorial attention of Halley and his contemporaries. However, Halley was born into an age which had already passed beyond the Greek achievement to create a new world-view of its own; this had been largely delineated by the little band of 'giants' upon whose shoulders Newton confessed himself to have stood to see further than other men. Chief among these were Copernicus, Kepler, Galileo, and Descartes; these men established the broad cosmic setting within which the operations of nature were conceived to proceed.

It was in 1543 that Nicolaus Copernicus had proposed to invert the traditional Earth-centred world-picture by making the Sun (instead of the Earth) the stationary body round which the planets revolved. Thereby planetary theory gained greatly in simplicity and geometrical elegance; but, despite the efforts of astronomers, no physical phenomenon could be found to give a decisive indication in favour of the Copernican hypothesis until Halley's years had nearly run their course. Meanwhile, early in the seventeenth century, the Copernican scheme of the solar system had afforded Johannes Kepler the clue to the discovery of his classic Laws of planetary motion, which were to serve as the text for Halley's first published paper. Kepler's third Law links the well-determined periods of revolution of the several planets about the Sun with their respective distances from that

luminary. Hence the dimensions of the solar system can be arrived at if we know the distance of a single planet from the Sun, or even the distance between any two of the planets at some stated time. The distance of Mars from the Earth at the planet's opposition of 1672 was determined from concerted observations made in Paris by Jean-Dominique Cassini and at Cayenne by Jean Richer, thus establishing the earliest reasonable estimate of the Earth's distance from the Sun. News of Richer's expedition helped to inspire the youthful Halley to undertake his voyage to St Helena; the observation that he made there of a transit of Mercury across the Sun's disc marked the beginning of his lifelong interest in the possibility of employing such transits (preferably of Venus) for securing improved estimates of the Earth's distance from the Sun. The specification of this important quantity required an accurate knowledge of the Earth's radius; and here, too, Halley followed the French astronomers in undertaking the measurement of a degree of the meridian.

In Continental Europe the Copernican theory encountered determined opposition on doctrinal grounds. It was the great Italian physicist Galileo Galilei who bore the brunt of this opposition and who, by his discoveries with the newly-invented telescope and his reformation of the principles of mechanics, did most to neutralize the traditional arguments against the possibility of the Earth's motion. Galileo's telescope revealed surface irregularities on the Moon and spots on the Sun, in contradiction to the 'perfection' that had long been claimed for celestial bodies. He also discovered the four principal satellites of Jupiter, which served to show that the Earth is not the only centre of revolution in the Universe, and the moon-like phases of Venus, which established that this planet revolved round the Sun and not round the Earth.

About the middle of the seventeenth century the Copernican scheme of the solar system was embodied in the widely accepted cosmology propounded by the French philosopher René Descartes. His doctrine of celestial 'vortices' was taught at Oxford and Cambridge in Halley's student days, and its influence can be traced in the astronomer's early papers. Although highly artificial and unable to stand up to experimental verification, the Cartesian system could claim to provide a plausible explanation of most known phenomena, including those of organic life and human behaviour.

It was in 1628 that Descartes retired into Holland to devote himself to the reformation of philosophy. He proceeded to construct in imagination the whole realm of nature, conceiving it as a mechanism such as God *could* have created in order to produce the observed phenomena. Descartes postulated matter endowed only with the property of extension in unbounded space and the capacity for motion. He conceived the Creator to have arbitrarily divided this continuum into particles (not atoms) and to have set these in motion under laws conserving always the same quantity of what we should call 'momentum'. No 'void', or unoccupied space, was admissible; hence all movement was of the nature of a circulation; and the Universe was to be conceived as a congeries of revolving systems of particles, or *vortices*. The smallest particles, constituting the element fire, collected at the centre of these vortices to form the stars, among which the Sun was to be numbered. Like all luminous bodies, the stars exerted an outward pressure which, transmitted to our eyes through the intervening medium, constituted the external physical cause of our sensation of light. Gross particles tended to form dark spots upon the stars: sunspots were already familiar. A star could become completely 'obfuscated' in this manner; it then ceased to exert an outward pressure, and its vortex collapsed under the pressures of the neighbouring vortices, one of which captured the defunct star and set it in orbit as a planet. The Earth and its planetary companions Descartes supposed to have been introduced into the Sun's vortex in this manner. The Earth was still the centre of a secondary vortex maintaining the revolution of the Moon and producing the force of gravity by a mechanical action. The Cartesian system thus embodied, and helped to establish, the Copernican conception of a Sun-centred planetary scheme.

Within this general world-view the individual sciences were being actively pursued in Halley's early years. Mathematics was beginning to assume the form in which the student encounters the subject today. Algebra was gradually taking over problems that had previously been treated much more cumbrously by geometry; logarithms were coming to be regarded as indices; rule-of-thumb methods of measuring curves or of dealing with continuously-varying quantities were about to be systematized into techniques foreshadowing the calculus. Unlike most of the other sciences, mechanics had been notably enriched by the critical discussion to which it had been subjected by the Schoolmen of the Middle Ages. This medieval tradition reached the youthful Galileo, who introduced the important conception of a uniformly accelerated motion and applied it to the case of a body falling freely under gravity. Galileo made some considerable advance towards the doctrine of inertia, first clearly stated by Descartes but given its classic formulation in Newton's first Law of Motion. It was thereby laid down that a body once set in motion would, in the absence of external forces, continue moving uniformly in a straight line. This principle served Torricelli, a disciple of Galileo, as the foundation for the science of projectiles; it also showed the problem of the planetary motions in a clear light, leading to its rapid solution at the hands of Newton. Halley, as we shall see, was concerned with both these developments. The mechanics of gases had been brought under investigation through the invention of the air pump, while the barometer had cleared up the mystery of suction phenomena and had established the role of atmospheric pressure. Physical principles could thus be brought to bear upon the problems of meteorology; Halley was to apply them to interpret phenomena occurring upon a scale commensurate with the Earth itself, and thereby he helped to create geophysics.

In the late seventeenth century, heat was still widely believed to consist of 'calorific' particles pervading a hot body; but this view was giving place to the conception of heat as a vibration affecting the atoms of which the body was composed. The invention of the thermometer by Galileo about 1590 had given precision to the conception of 'degree of heat', or temperature. Halley was to stress the importance of standardizing the instrument. The laws of reflection of light had been known from antiquity; the sine law of refraction, established only at the beginning of the seventeenth century, enabled the theory of the telescope to be placed upon a sound footing, though the process of tracing the course of the rays through the instrument was rendered tedious by the multiplicity of special cases. There was no satisfactory explanation of colours; and serious discussion of the physical nature of light was only just beginning. The velocity and the medium of propagation of sound were both investigated by Halley and his contemporaries. The science of magnetism was far advanced when Halley made it one of his principal interests. The properties of magnets were known to the ancients; the mariner's compass had since been introduced and its vagaries noted, and the analogy of the Earth to a huge magnet was widely accepted. The study of electrical phenomena was far less advanced, and Halley's interests barely touched it. Peripheral, too, was his contact with chemistry, geology, and the biological sciences, though these were not excluded from his interests.

Astronomers of the sixteenth and seventeenth centuries usually limited the scope of their observation and theorizing to the solar system, content to regard the stars as luminaries attached to an immense crystal sphere having the Sun as its centre. However, the diversity in brightness of the stars suggested their distribution in depth at various distances from us, while the yet bolder surmise that space might be infinite, with no assignable centre, prepared the way for Halley's discovery that the stars are moving freely, and observably, in what he conceived to be a boundless void. Interest in this little-explored field of stellar astronomy had been quickened since the latter part of the sixteenth century by the occasional apparition of 'new' stars and by the detection of variable stars and of nebulae, among which Halley classed the two fine star clusters that he discovered.

However, Halley's day-to-day concern as an astronomer was not with cosmological mysteries but with practical and instrumental problems; and he was fortunate in being born just when the art of observing the heavens was being revolutionized by the somewhat belated application of the telescope to the measurement of celestial angles. The substitution of telescopic for open sights in astronomical measuring-instruments was established by the Parisian astronomers about 1668. The new sights were not introduced without some opposition; and Halley became involved in the resulting controversy. Another indispensable adjunct to the modern observatory, the pendulum clock, had been patented by Christiaan Huygens about the year of Halley's birth.

The reformation of astronomy in the sixteenth century had owed something of its vigour to the urgent requirements of navigators, now at length venturing into uncharted oceans for trade or exploration. It was of vital importance for the mariner to be able to fix his position at sea by determining his latitude and longitude. The latitude presented no special difficulty, requiring only an instrument for measuring the meridian altitude of the Sun, with tables of the Sun's apparent annual motion round the heavens. Several instruments were available for the purpose; Halley concerned himself with their improvement, and he lived to see the introduction of the ideal device, the nautical sextant. The determination of longitude presented greater difficulties; it requires a comparison of the mariner's local time with that of some standard meridian. No problem held a deeper fascination for Halley, who was involved with it both as astronomer and as ocean navigator. By his time hopes had become fixed upon the possibility of utilizing the Moon, in its monthly motion, as the hand of a clock measuring out standard time against the background of stars. Before this method could be put to practical use it was necessary to reduce the Moon's complicated motion to a rule and to determine and catalogue the exact positions of the stars in the belt of the heavens traversed by the Moon. It was to carry out this twofold programme that King Charles II founded Greenwich Observatory, where Halley was to labour as Astronomer Royal for the last twenty years of his life.

Close upon three hundred years have now passed since Halley began his career of discovery. Scientists have long since perfected a well-tried procedure for investigating any unfamiliar phenomenon; and throughout 'Western' societies the scientific assessment of experience is generally shared even by those who make no claim to be scientists. But in Halley's England superstition was widespread; and even among men of science much uncertainty prevailed as to the correct procedure for reaching valid conclusions about the world around us. Broadly speaking, there were three competing prescriptions for the advancement of natural knowledge, commended respectively by the authority of Francis Bacon, of Descartes, and of Galileo.

All three philosophers revolted against the claim of the medieval Schoolmen to be able to discover truths about nature through the logical analysis of Aristotle's scientific treatises. Bacon appealed from ancient authority to experience. He proposed that the circumstances of the occurrence, or the non-occurrence, of each type of natural phenomenon should be tabulated; analysis of the results should enable the underlying cause of the phenomenon to be tracked down. Standing at the opposite extreme from Bacon's almost mechanical interrogation of experience, Descartes formed the ideal of building up science by valid inferences from axioms clearly apprehended to be true, so that scientific theories should carry the same conviction as do mathematical propositions. Experiment played but a subordinate role in his system, being used to investigate particular details which, within the same general scheme of things, might have been different if the Creator had so willed. Lastly, the seventeenth century saw the adoption, in physical science, of the practice of operating with concepts capable of being specified in quantitative terms (as a velocity is expressible in feet per second). These concepts were woven into hypotheses from which could be inferred (often by mathematical deduction) consequences capable of being verified (or falsified) by experiment. This type of procedure is exemplified in the work of Galileo and of Newton, though Baconian and Cartesian influences are also manifest.

Bacon dreamed of an institution for organized scientific research; and he was one of the first to recognize the immense contribution that scientific knowledge might make to improving the lot of mankind. His vision was consciously realized, in some degree, in the establishment of the Royal Society, whose founders liked to think of themselves as his disciples, and with the activities of which Halley was to be associated for more than sixty years. The Royal Society was one of several such academies that arose in seventeenth-century Europe, partly in revolt against the scholastic discipline of the medieval Universities, partly to enhance the prestige of their parent states.

It seems to have been about the year 1645 that a small group of those interested in the new 'experimental philosophy' began to make a practice of meeting together once a week at Gresham College, in the City of London, or at some other convenient rendezvous (Pl. 4a). Banning political and theological controversy, they would discuss recent discoveries and current scientific problems. Close relations seem to have existed between the Gresham College circle and outside practitioners, particularly in the fields of navigation and naval administration. After three or four years, the little company was divided into two groups through the removal of some of its members to Oxford, where they formed an independent society. Following the Restoration of King Charles II to the throne, and with his encouragement, steps were taken to constitute the London group as a formal Academy. The Royal Society of London for improving Natural Knowledge was, in fact, virtually constituted on 28 November 1660, though its title and privileges under its Royal founder were first established by Charters granted in 1662 and 1663. In the spirit of Francis Bacon, the founders sought to promote a fruitful interchange between pure science and current technical practice, though the Society's 'Baconian' period was to be succeeded, in Halley's later years, by a 'Newtonian' one.

So much, then, by way of a general survey of Halley's scientific inheritance. How he entered into it and enlarged its borders will be related in the following pages.

2 A Note on Sources

Primary sources for the life of Edmond Halley are:

The article in *Biographia Britannica*, iv (1757), 2494ff., probably written by Thomas Birch, or perhaps John Machin, which contains some information communicated by Halley's son-in-law, Price.

A manuscript memoir of Halley, probably by Martin Folkes, found in the Bodleian Library at Oxford and transcribed by S. P. Rigaud, which may have been intended to supply information for Mairan's 'Éloge de M. Halley', published in the *Mémoires de l'Académie Royale des Sciences (Histoire)*, Paris, année 1742, 172ff.

Important letters and papers by Halley included, together with much valuable information relating to the astronomer's life and work, in E. F. MacPike's *Correspondence and Papers of Edmond Halley* (Oxford, 1932; London, 1937), in which the above-mentioned manuscript memoir was also first printed, together with Mairan's Éloge.

Valuable secondary sources are, E. F. MacPike, *Hevelius, Flamsteed and Halley* (London, 1937), and the article by Agnes M. Clerke in the *Dictionary of National Biography*. Other sources will be indicated in the following pages.

Chapter 2

The Boy Astronomer

1 *Birth, Parentage, and Education*

THE boroughs and wards of London preserve many names of what were once villages and hamlets lying beyond the old city boundaries. What we know today as Haggerston, a densely-populated district of East London, formerly consisted of little more than a country house, situated in the parish of St Leonard, Shoreditch. About the middle of the seventeenth century this residence became the family home of one Edmond Halley, a prosperous soap-boiler with a town house and business premises in Winchester Street. He was the son of Humphrey Halley, a citizen of London variously described as a haberdasher and a vintner. Of his more remote ancestry nothing is known with certainty, though John Aubrey connects the Halleys with a Derbyshire family of the same name. In 1656 Edmond Halley married Anne Robinson, and at least three children were born of the marriage. A son, named Humphrey, died abroad about 1684; his sister Katherine probably died in infancy. Another son was called Edmond after his father; and it is with his life and achievements that we shall be concerned in the following pages.

There is some uncertainty as to the precise date of the boy's birth. He seems to have grown up in the belief that it fell upon 29 October 1656 (8 November, according to the New Style); but this date is not confirmed by any known baptismal record, and it follows so closely upon the date of his parents' marriage, on 9 September 1656, at the fashionable church of St Margaret's, Westminster, as to prompt the surmise that his birth may have occurred exactly a year later, in 1657. But perhaps his parents reckoned their married life from an earlier, civil ceremony.

Edmond was a promising boy; and his father, anxious that he should have the benefit of a good education, sent him to St Paul's

School, which Dean Colet had founded in the City of London in 1509 with the aim of fostering the ideals of the New Learning. We do not know in what year Halley entered St Paul's. It may well have been in the time of Samuel Cromleholme, who held the office of High Master from 1657 to 1672. The problem is complicated by the consideration that the old School was completely destroyed in the Great Fire of 1666, and the new building which arose upon the same site was not opened until April 1671. It is not clear what happened to the scholars in the meantime, though Cromleholme is known to have opened a school at Wandsworth during the interval. On the other hand, we are told that Halley became Captain of the School at the age of 15, a rank which even he could hardly have attained immediately upon his arrival as a new boy. So that it seems probable, especially if he was in fact born in 1656, that he entered the school in the days of Cromleholme.[1] However, Halley's biographers link his name with that of Thomas Gale, who succeeded to the office of High Master in 1672. Born in 1636 at Scruton in Yorkshire, Gale was educated at Westminster and proceeded thence to Cambridge, where, in due course, he was appointed Regius Professor of Greek. A few months later he exchanged his Chair for a schoolmaster's desk and ruled St Paul's for 25 years, becoming Dean of York in 1697. He died in 1702. Gale was one of the most distinguished scholars of his day, with wider interests which especially fitted him to tend the ripening genius of his most famous pupil. The passing years were to bring them together again: in 1677 Gale was elected a Fellow of the Royal Society; he later served on the Council of the Society, and when he and Sir John Hoskyns were appointed to be its Secretaries, they chose Halley as their Clerk.

As a scholar at St Paul's, Edmond excelled in the study of classics and of mathematics (which in those days included some acquaintance with astronomy and navigation); and it was there that he made his earliest recorded scientific observation, measuring, in 1672, the variation of the magnetic compass; this result he later published in a list of such determinations. His interests had already turned to astronomy when, in the summer of 1673,

[1] See M. F. J. McDonnell, *A History of St. Paul's School*, London, 1909, and *The Annals of St. Paul's School*, privately printed, 1959.

he entered Queen's College, Oxford, as a Commoner, taking with him a 'curious apparatus of instruments' provided by his father.

2 *First Scientific Paper*

Halley was still an Oxford undergraduate when his first scientific paper appeared in the *Philosophical Transactions*, of which Henry Oldenburg, the Secretary of the Royal Society, was the editor and publisher. It was a contribution to the problem of determining what are called the elements of the orbit of a planet.

Johannes Kepler had announced in 1609 that the planet Mars

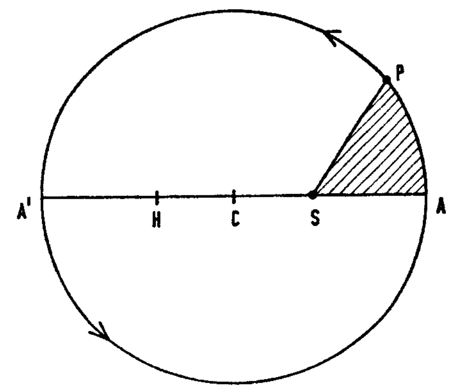


Fig. 1 A planet's elliptic orbit.

revolved in an ellipse, the Sun occupying one of the two foci of the curve. He went on to establish that the other planets, including the Earth, also describe elliptic orbits having the Sun as a common focus. Now ellipses differ in size and in shape; and the astronomer needs to be able to specify the length of what is called the major axis AA^1 of a planet's elliptic orbit, and the eccentricity (CS:CA) which measures the proportional displacement of the focus S from the centre C of the curve (Fig. 1). In determining these quantities, astronomers who strictly followed the ideas of Kepler had to take account of his second law of planetary motion, that the radius vector SP, joining the Sun S to a planet P, sweeps out equal areas in equal times. However, by the middle of the seventeenth century it had been found easier to substitute some other condition for Kepler's second law, as, for example, that a planet revolves with a uniform angular velocity about the vacant focus H of its elliptic orbit—the focus *not* occupied by the Sun. This is very nearly true when the orbit is of small eccentricity; and in 1656, the reputed year of Halley's birth, Seth Ward, an Oxford astronomer, had published a book on geometrical astronomy in which he adopted this simplifying assumption as the basis of his method for determining a planet's orbital elements. The object of Halley's paper was to show how three determinations of a planet's position at noted

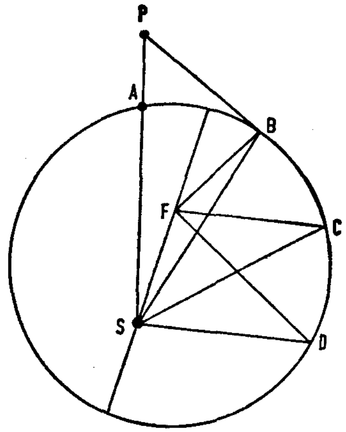


Fig. 2 Determining the Earth's orbit.

times would suffice to solve the problem without any physical assumptions except that the planets described ellipses about the Sun in a common focus and in known periods of rotation (*Phil. Trans.* (1676), *11*, 683ff.).

Halley's method was based upon the principle that, as the Earth revolves in its annual course round the Sun, the terrestrial observer sees his motion reflected in the apparent motion of a neighbouring planet. He first shows how to establish the Earth's orbit. Choose a time when the Sun S and the Earth A lie in a straight line with one of the outer planets, preferably Mars, or, more strictly, with the foot P of the perpendicular drawn from the planet to the plane of the Earth's orbit (Fig. 2). Determine the planet's apparent place in the sky. Allow an interval of time

to elapse equal to the mean period required for the planet to complete a circuit of the heavens (its sidereal period); for Mars this is 687 days. The planet will by then have returned to its initial position at P while the Earth, which completes nearly two annual revolutions in that time, will have reached the position B. Observation serves to determine the three angles of the triangle PBS (in particular the angle BSP), and therefore the ratio (SB: SP). The same procedure, twice repeated, after further intervals of 687 days, when the Earth is at C and D respectively, defines the distances SC and SD in terms of SP, and the corresponding angles CSP and DSP.

All this is very much on the lines of Kepler's technique for determining the elements of a planetary orbit by reference to another planet which is 'kept stationary' by spacing out the observations at intervals equal to its sidereal period. Halley's problem, however, was to determine the major axis and the eccentricity of the Earth's orbit from three focal radii SB, SC, SD. Assuming $SB > SC > SD$, take B and C as the two foci of a hyperbola having its transverse axis equal to $SB-SC$; and take C and D as the foci of a second hyperbola with transverse axis equal to $SC-SD$. Let the branch of the first hyperbola having B as its internal focus intersect the branch of the second having C as its internal focus, in F. Join FB, FC, FD. Then (from well-known properties of the hyperbola and the ellipse)

$$FC-FB=SB-SC \text{ (the transverse axis), and}$$
$$FD-FC=SC-SD$$

Hence, $SB+FB=SC+FC=SD+FD$, which establishes F as the second focus of the Earth's orbital ellipse and defines the length of its major axis, while SF, the separation of the foci, defines the eccentricity.

Halley now employs a corresponding procedure to establish the elements of a planet's orbit. Let S be the Sun, ASB the major axis of the Earth's orbit, and P the projection of Mars, as before (Fig. 3). Observe the positions of S and P from the Earth at K, and again from the Earth at L after the lapse of 687 days when the planet has returned to the same position. Knowing now the direction of the axis ASB, Halley obtains the angles ASK, ASL, and he calculates SK and SL, using what we now call the 'polar equation of a conic'. Solve the triangle KSL for KL, and the angles SKL, SLK; thence solve the triangle KPL for LP, and the triangle SPL for SP, which is then to be corrected for the inclination of the orbit of Mars to give the planet's true distance from the Sun in length and direction. Two other such pairs of observations are needed to give, in all, *three* positions of the planet in its orbit, to which Halley's former procedure can then be applied to find the major axis and the eccentricity.

Halley concludes his paper with an algebraic treatment of the

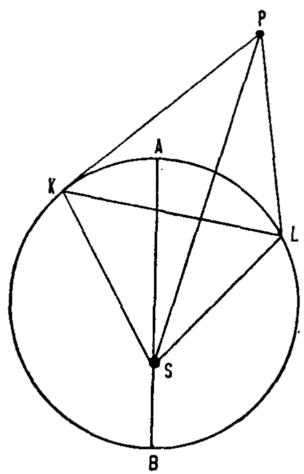


Fig. 3 Determining a planet's orbit.

problem under discussion. His undergraduate effort may perhaps be classed as brilliant geometry rather than useful astronomy.

3 *Friendship with Flamsteed; early observations*

While he was still at Oxford, Halley exchanged letters with another young astronomer, John Flamsteed (Pl. 3). The careers and the interests of the two men were to be closely linked in after years, though the promise of their early friendship was unfortunately not to be fulfilled. Born in Derbyshire in 1646, Flamsteed was about ten years older than Halley. His education was interrupted and he was thrown on to his own intellectual

resources by the constitutional ill-health which was to dog him through life. His interests set strongly towards astronomy; and he introduced himself to the circle of the Royal Society by computing an almanac of celestial events for the year 1670 and sending it to Lord Brouncker, the Society's first President. Invited to London, Flamsteed was introduced to Sir Jonas Moore, Master of the Ordnance and a mathematician, who lived at the Tower of London and who furnished him with instruments to equip his little north-country observatory and spoke often of him to King Charles II.

It was therefore natural that when, in 1674, the Royal Society set about founding an observatory to match the one lately established in Paris, Moore should summon Flamsteed to London to take charge of the project. However, while the merits of various sites (including Chelsea and Hyde Park) were under discussion, the course of events was changed through a Frenchman's claim to have solved the long-standing problem of finding the longitude at sea. The proposal involved utilizing the Moon as the hand of a great clock measuring out standard time as it traces out its monthly course against the background of the fixed stars. Flamsteed was appointed a member of the commission set up to consider the feasibility of the plan. He argued that it was impracticable both because the complicated motion of the Moon was not correctly shown by the current lunar tables, and because the places of the stars, to which the Moon's position was to be referred, were not accurately known either. When this report was shown to Charles II, he 'startled', in Flamsteed's phrase, and commanded that the deficiencies should be made good in the interests of navigation. By a Royal warrant issued on 4 March 1675, the King appointed Flamsteed 'Our Astronomical Observator' at a salary of £100 a year. On Sir Christopher Wren's recommendation, Greenwich Hill was chosen as the site of the new Royal Observatory, the construction of which was sanctioned by a warrant dated 22 June 1675. The foundations were laid in August of that year, and the building was completed and Flamsteed installed by the following July. (Pls. 5 and 6.)

There is evidence that during the summer of 1675 Halley spent much time with Flamsteed at the Tower, where Moore resided, and at Greenwich, where the new Observatory was under construction; also they were seen together at various coffee-houses. Flamsteed acknowledged Halley's co-operation in observing two lunar eclipses which occurred in 1675, one on 27 June ('the ingenious youth, Edmond Halley, an Oxonian, was present at these observations and greatly assisted them by his diligence'), and the other on 21 December ('Edmond Halley observed it in London in Winchester Street'—*in vico Wintoniensi*).[1]

During the summer of 1676, Halley at Oxford and Flamsteed at Greenwich independently observed the course of a large sunspot across the Sun's disc, making daily measurements of its position by means of micrometers. Such observations help to determine the period in which the Sun completes one rotation upon its axis, and the direction in which, and the amount by which, this axis is inclined to the plane of the Earth's orbit. The two sets of measurements showed good agreement, and they were published by Flamsteed. They indicated a rotation period of 25 days, 9½ hours; but more confidence could have been placed in them if the sunspot had not unfortunately broken up in mid-passage (*Phil. Trans.* (1676), *11*, 687f.).

In the same volume of the *Transactions*, Halley published his observations of an occultation of the planet Mars by the Moon on 21 August 1676 (ibid., 724). On such occasions the Moon, in its orbital motion round the Earth, passes between us and the planet, or other such celestial object, and temporarily hides it from our view. If the instant of occurrence of such a 'celestial signal' is determined at two different stations, the difference in the local times gives the difference of longitude of the two stations. Halley timed the instants of the planet's disappearance and reappearance in Oxford time; a subsequent comparison with the corresponding local times of these phases recorded at Greenwich and at Danzig showed (when allowance was made for lunar parallax as between the three observing-stations) that the longitudes of Greenwich and Oxford differed by 1° 15′ and those of Greenwich and Danzig by 18° 41′ (the modern estimated differences of longitude are 1° 16′ and 18° 40′ respectively).

Halley's three papers, on planetary orbits, on the sunspot, and on the occultation of Mars, were all published in the *Transactions*

[1] See *Phil. Trans.* (1675–6), *10*, 371, 498.

for 1676. Before the year was out, he had left Oxford without a degree and had set off for St Helena, the lonely island in the south Atlantic, with no less ambitious intention than that of cataloguing and charting the stars of the southern celestial hemisphere which are invisible from Europe.

Chapter 3

The Southern Tycho

1 The Heavens Surveyed

THE construction of a star catalogue is bound up with certain conventions about representing the positions of celestial objects on the sphere of the heavens; and it depends upon a certain technique of astronomical observation. Some brief explanation of these matters is called for at this point by way of introduction to Halley's expedition as well as to the more mature achievements of his lifelong endeavour for astronomy.

The stars scattered over the night sky show a tendency to form irregular groups which catch the eye of the observer. In pre-scientific ages these star groups, or constellations, were woven into the web of mythology and received the names of heroes, monsters, and familiar objects which their outlines were thought to resemble. Such a celestial nomenclature, associated with Greek legend, is still of service to astronomers. The constellation figures show so little change with the lapse of the ages that the Greeks could conceive the stars as fixed like studs to a solid sphere. And they sought to define and record the positions of the principal stars on this sphere in much the same way as geographers fix the positions of places upon the Earth by specifying their longitudes and latitudes. The doctrine of a solid celestial sphere was to prove physically untenable : it was Halley, in fact, who, early in the eighteenth century, finally shattered it by establishing the independent motions of certain stars in space. However, it has proved convenient to retain the *fiction* of a celestial sphere, of indefinite radius, upon which the stars, whatever their actual distribution in space, are represented by their projections as viewed by a centrally-situated observer. And on this convention astronomers have continued the practice, begun by the Greeks, of recording the positions of selectedclasses of stars.

Briefly, this procedure consists in selecting some great circle OMN of the celestial sphere (Fig. 4), choosing an origin O upon it, graduating this *primary* circle in angular measure, and locating its two poles, P and P^1 (which are 90° from every point on the great circle OMN). To locate the star S, a *secondary* great circle PSP^1 is drawn passing through S and cutting the primary circle in M. Then the position of S is determined uniquely by specifying the arcs OM, MS (or, equivalently, the angles OCM, MCS which these arcs subtend at the centre C of the sphere). The primary co-ordinate is measured eastward;

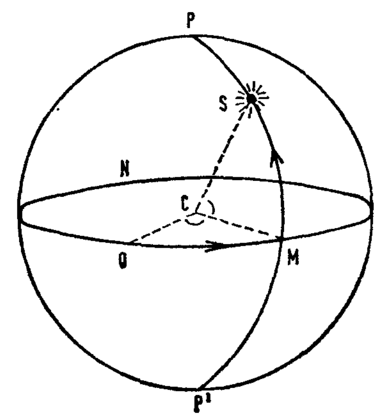


Fig. 4 Defining the position of a star upon the celestial sphere.

secondary co-ordinates, measured towards P or towards P^1, are distinguished by means of a sign convention.

The Greeks selected as their primary circle the ecliptic, the path which the Sun appears annually to trace out through the heavens; they chose as origin the equinoctial point the passage of the Sun through which marks the beginning of spring. Since the middle of the seventeenth century it has been more usual to take the celestial equator (the trace of the Earth's equator upon the celestial sphere) as primary circle, with the equinoctial point again as origin. The latter system is related to the polar axis about which the celestial sphere appears daily to revolve; the measurement of time is therefore involved in the determination of a star's position, and the clock becomes an essential adjunct to the telescope.

Closely bound up with these conventions for defining the positions of celestial bodies, there developed a practical technique for measuring the angles and arcs involved. The Greeks established a type of astronomical instrument which has survived in principle to our own day. The fundamental operation of precise astronomy is to determine the angle PEQ subtended at the observer's eye E by two given points, say two stars, P and Q, or, equivalently, to measure in units of angle the arc P′Q′ on the celestial sphere (Fig. 5). An instrument designed to carry out this operation must consist essentially of a graduated circle

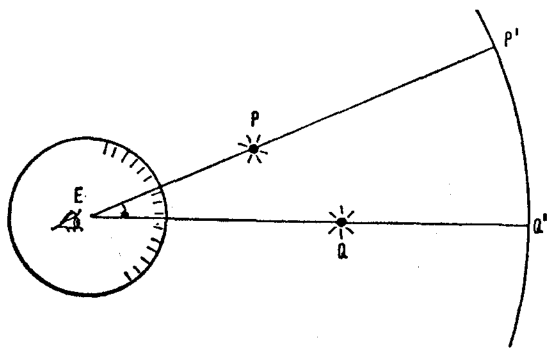


Fig. 5 Measuring a celestial angle.

equipped with a radial pointer turning about the centre and directed towards a star with the aid of sights. The plane of the circle is brought into coincidence with the plane determined by the observer's eye E and the two stars P and Q whose separation is required. The pointer is then directed to each of the two stars in turn, and the difference of the two scale readings gives the required angular separation of the stars. The principal Greek and medieval astronomical instruments worked on this plan, and so did those of the great sixteenth-century Danish observer Tycho Brahe, who, however, introduced great improvements in the graduation, sighting, and mounting of the instruments. His technique was followed in all essentials by Halley's friend and older contemporary, Johannes Hevelius of Danzig.

Now the finite resolving power of the human eye imposes a

natural limit upon the accuracy with which celestial angles can be estimated in this manner. By this is meant that if the angle PEQ is less than about 2 minutes of arc, the two points P and Q will appear one and the same to the normal eye; thus all estimates of celestial angles made in this way are necessarily subject to an uncertainty of about the amount stated. However, about the time when Halley first took up astronomy, the telescope was beginning to be consistently employed to magnify the angles under which the eye sees distant objects, thus proportionately restricting the limits within which those angles could be estimated. Thus a telescope magnifying sixty-fold enabled an uncertainty of 2 minutes to be reduced to one of 2 seconds. This improvement was effected by substituting the telescopic sight for the open sight previously in use; it was usually equipped to define a precise direction by mounting a pair of threads or wires, intersecting at right angles, in the focal plane of the object glass of the instrument, where the images of distant objects are formed. These wires, being in the same optical plane as the images, were in focus for the same adjustment of the eye when viewed through the eyepiece, another advantage which they possessed over the open sight. Alternatively, when the objects whose angular separation was required were simultaneously visible in the field of the telescope, the measurement could be made by means of a micrometer, usually consisting of wires movable in the focal plane of the instrument.

Some early trials of the telescopic sight and the micrometer were made by a young English observer, William Gascoigne, about 1640; but these devices were first established about 1668 by the astronomers of the newly founded Académie des Sciences in Paris. In the course of his youthful travels, Halley was to become acquainted with these Parisian astronomers. Meanwhile, Johannes Hevelius maintained a lifelong opposition to the introduction of telescopic sights; and this gave rise to a controversy in which, again, it was Halley's lot to become involved.

The places of the stars upon the celestial sphere, whether determined with the unaided eye or by means of telescopic sights, are eventually recorded in a star catalogue. This is normally drawn up in parallel columns which respectively contain, for each selected star, a brief indication of its situation in its own constellation (or other identifying label), the two angular measurements which define its position upon the conventional sphere, and some indication of its relative brightness. When the ecliptic is chosen as primary circle, the two spherical co-ordinates (OM and MS in Fig. 4) are known respectively as the celestial longitude and latitude; when the star is referred to the celestial equator they are called the right ascension and declination.

The tradition of historic star catalogues began with that drawn up by Hipparchus of Rhodes in the second century before Christ and edited by Ptolemy of Alexandria some three centuries later. Hipparchus classified his selected stars according to their brightness on a scale which was to provide the basis for the modern system of stellar magnitudes. This catalogue had no rival in antiquity and the Middle Ages except, perhaps, that of the fifteenth-century Tartar astronomer Ulug Begh; but in Halley's student days the best available star catalogue was that compiled towards the end of the sixteenth century by Tycho Brahe and revised by his disciple Johannes Kepler. However, the introduction of telescopic sights rendered all previously-compiled star catalogues obsolete; and the construction of a more accurate catalogue, based upon telescopically determined star places, was one of the principal aims in view when the Royal Observatory was established at Greenwich in 1675. The stars to be observed there were predominantly those of the northern celestial hemisphere. However, following the discovery of the Cape of Good Hope and the establishment of trade routes through southern waters, it had become possible, and necessary for navigation, to list and locate the stars which surround the south celestial pole. This task had indeed been attempted by certain sixteenth-century mariners, but only in crude fashion even when judged by the standards of pre-telescopic astronomy. The deficiency was now to be made good by an Oxford undergraduate only just entering upon manhood.

2 *Expedition to St Helena*

The earliest hint of Halley's project of observing the southern stars is perhaps to be found in a letter that he sent to Henry Oldenburg, Secretary of the Royal Society, in July 1676. He had

heard that the French astronomer Jean Richer was soon to publish a book about his astronomical expedition to Cayenne to observe the planet Mars at its opposition of 1672. He wondered whether the book would contain a catalogue of stars of the southern hemisphere.

For if that work be yet undone, I have some thoughts to undertake it myself, and go to St Helena, or some other convenient place, where the south pole is considerably elevate, by the next East India Fleet, and to carry with me large and accurate instruments, so as to be able to make a most accurate sphere of fixed stars, and complete our globes throughout: nor will that be all; but by comparing observations made there and here, the proportion of the Moon and Earth, with their distance, will be more exactly, than any other way, found. But if it be already done, I would not then meddle with it, though I would very willingly do something to serve my generation; and here I can do nothing but what will be rendered wholly inconsiderable by the greater accurateness of the three great promoters of the astronomical science in our age [he is alluding to Hevelius, Cassini and Flamsteed].

A month later, sending his paper on planetary theory to Oldenburg, he writes: 'Let me understand what you hear from France about the southern stars . . . and if here there be any appearance of encouragement for my friend that will go along with me' (S. P. and S. J. Rigaud, *Correspondence of Scientific Men of the Seventeenth Century*, Oxford, 1841, i, 228, 242).

The results of Halley's expedition were embodied in a catalogue of the southern stars dated 1679. This will be described in due course; but reference is made to it here because the Preface affords the most authentic account we have both of the conception and of the accomplishment of the young astronomer's bold design. We will tell the story largely in his own words, translated freely from the Latin.

From my tender years [he writes] I showed a marked bent towards mathematics; and when, about six years ago, I first devoted myself wholly to astronomy, I derived so much pleasure and delight from its study as anyone inexperienced therein could scarcely believe. I was carried along by such a mental impulse that in a short time I had explored all the intricacies of sidereal science, so remote from the perception of the ordinary run of men.

The science could not be studied in libraries; and Halley equipped himself with instruments and started observing at every opportunity,

whence it became clear that the reckoning of all the tables for representing celestial motions was seriously defective. For instance, Saturn is travelling much more slowly and Jupiter more rapidly than is laid down by the tables hitherto published. From that time I began to think seriously of correcting these tables. But I realised forthwith that I should be wasting time in this great enterprise without a more correct catalogue of the fixed stars. Now it is well known that the illustrious Johannes Hevelius, Consul of Danzig, has devoted many years to this task and has, by his learned writings, greatly enriched astronomy. With his costly and quite amazing outfit of instruments, he appeared to me by far the best equipped for carrying out this correction. Nor was I unaware that this is the principal duty laid upon our Astronomer Royal, John Flamsteed, who I believe will accomplish whatever can be done in this business by means of those most refined instruments with which the Royal Observatory is furnished.

With these two distinguished observers, Halley ranked Jean Dominique Cassini of Paris.

As for me, who, in proportion to my slender resources, have striven to contribute some mite to the use or ornament of Urania, if I were to attempt anything of the same kind, I know I should be as one stupidly quacking among such matchless swans. And so a more favourable prospect was afforded by the restoration of the stars concealed from our view near the south pole of the heavens, which no one, to my knowledge, has hitherto attempted with the requisite instruments.

Halley refers to earlier catalogues of the southern stars, in particular to one published by Kepler in 1627. The position appears to be that the origin of the southern constellation figures is involved in some obscurity. The credit for the earliest European observations of the principal stars invisible from our latitudes was formerly shared among several sixteenth-century navigators; but the researches of E. B. Knobel established that the first substantial catalogue of southern star places was the achievement of the Dutch pilot Pieter Dirckszoon Keyser (latinized as Petrus Theodorus). He seems to have made the observations at Madagascar in 1595, but he died at Sunda in the following year, and his catalogue (comprising the 12 original

southern constellations) was published in 1603 by his fellow-voyager, Frederick de Houtman, who unfairly received the credit for it. Meanwhile, Johannes Bayer had incorporated the new material in his *Uranometria* of 1603.[1] Kepler tacked on to the end of his edition of Tycho Brahe's catalogue a class of 136 southern star places which had been extracted for him by his son-in-law, Jacob Bartsch, from Bayer's charts and latest manuscripts (*Tabulae Rudolphinae*, Ulm, 1627, 118f.). Halley mentions Frederick de Houtman; he also refers to the determinations of star places made a few years earlier by the French astronomers Richer and Meurisse at Cayenne, of which, however, he had no particulars; in any case the south celestial pole would not be visible from their place of observation.

Having resolved upon an expedition to the southern hemisphere, 'I communicated my plans to certain of my friends who were skilled in these matters, whom also I consulted as to a suitable place of observation and as to the instruments required.' Some suggested Rio de Janeiro, others the Cape of Good Hope; 'but neither place attracted me, as I realized that I should have to contend for a long time with the completely unfamiliar local customs and language, and that much time set apart for astronomical pursuits would have to be spent in learning these. Therefore we chose the island of St. Helena, the southernmost of all territories under English rule, where the south celestial pole is moderately elevated above the horizon, and which appeared on many accounts the place most suited to my purpose.' Halley's father willingly promised to pay the expenses of the expedition and allowed him £300 a year. Application was made to the Secretary of State, Sir Joseph Williamson (a Fellow and benefactor of Halley's College and soon to become President of the Royal Society), who countenanced the project. Sir Jonas Moore, Master of the Ordnance and a Royal Society mathematician, who, as we saw, played an active part in establishing the Royal Observatory, brought the matter to the notice of King Charles II; he heartily approved the proposal and commended Halley to the East India Company (which then governed

[1] See E. B. Knobel, 'On Frederick de Houtman's Catalogue of Southern Stars, and the Origin of the Southern Constellations,' *Monthly Notices of the Royal Astronomical Society* (1916-17), 77, 414ff., 580.

St Helena) in a letter which Moore presented to their Court of Committees on 4 October 1676. The Company was bidden to convey Halley, with a friend who was to accompany him, on the first ship bound for St Helena, and, when they arrived there, to afford them all countenance and assistance necessary to the accomplishment of their task. The Court directed that the two observers, with their equipment, should be transported free of charge on the *Unity*, and that the appropriate instructions should be sent to the Governor of the island. The astronomers were to be given free accommodation, in the Governor's house or otherwise, but they were to pay for their own board; and when they had completed their mission they were to be commended to the captain of some homeward-bound vessel of the Company's fleet.[1]

To return to Halley's narrative:

And so, being now certain of going, I had a sextant made with a radius of 5½ London feet; the framework was of iron and the limb and enclosing radii and the scales were of brass. It is equipped with telescopes in place of open sights, and, so that all necessary motions can be conveniently imparted, it is mounted upon two toothed semicircles placed at right angles to each other which, turned by endless (or Archimedean) screws, serve with little trouble to adjust the plane of the sextant to that of any two selected stars. Moreover, I have a quadrant (of which I had made previous use) about 2 feet in radius; I seldom directed it to the heavens except to determine altitudes of the Sun for correcting the clock. I also prepared telescopes of several different lengths, the longest of 24 feet, with two micrometers for measuring arcs within the same field of the telescope.

Halley and his companion, whose name was Clarke, set off in November 1676 and arrived at St Helena after a voyage lasting about three months Halley had been led by travellers' reports to expect clear skies at St Helena,

but, on the contrary, I was much tormented with cloudy and generally rainy conditions of the atmosphere, which was such a hindrance to me that through the whole of August and half of September I could scarcely make a single observation. . . . All too frequently clouds, laden with their own moisture, would descend upon the lofty peaks of the island and blot out everything with the densest fog.

[1] E. F. MacPike, *Correspondence and Papers of Edmond Halley*, London, 1937, 179f.; this book is hereinafter referred to as *Correspondence*.

A south-east wind blew incessantly. Referring years later to his experiences at St Helena, Halley wrote:

In the Night time, on the tops of the Hills about 800 yards above the Sea, there was so strange a condensation or rather precipitation of the Vapours, that it was a great Impediment to my Celestial Observations; for in the clear Sky the Dew would fall so fast as to cover, each half quarter of an hour, my Glass with little drops, so that I was necessitated to wipe them so often, and my Paper on which I wrote my Observations would immediately be so wet with the Dew, that it would not bear Ink. (*Phil. Trans.* (1691), *17*, 471f.)

In a letter addressed to his patron, Sir Jonas Moore, from St Helena and dated 22 November 1677, Halley pleaded the adverse weather in excuse for his having accomplished so little to date; and he paid tribute to his companion: 'Mr. Clarke is a person wonderfully assistant to me, in whose company all the good fortune I have had this Voyage consisteth, to me all other things having been cross' (*Correspondence*, 39ff.). Halley made use of every hour when observation was possible, without thought of rest; and he refrained from spending time upon planetary observations. Even so, he had determined the places of only 341 stars when the time came to return to England. It was early in January 1678 when, after about a year on the island, Halley and Clarke sailed for home on the *Golden Fleece*, arriving in London before the end of May. 'Hally from St. Hellena', wrote Hooke in his diary on 30 May 1678. In a note oddly interpolated in his star catalogue, Halley remarks that he and his friend had embarked for their homeward voyage from the island 'much exhausted by vain watches' during a run of cloudy nights, and smarting from the undeserved insults of 'a certain individual exercising tyranny there'. The Company, indeed, received complaints from several quarters about the 'ill-living' of the Governor, in consequence of which he was removed and an officer 'of sober life and conversation' appointed to succeed him. On the other hand, the court of Committees was certified of the 'civil demeanour' of Halley and his friend while on the island and at sea; and a deposit of £20 was ordered to be returned to the astronomer (ibid., 180f.).

In the course of his voyages to St Helena and back, Halley must have become acquainted with the practice of ocean navigation; and to this phase of his career belongs an improvement which he effected in the design of one of the standard nautical instruments of the period. The art of navigation largely consists in the determination of position at sea, or, more precisely, finding the present longitude and latitude of the navigator's ship. In Halley's day, longitudes could be determined only with difficulty by a comparison of pre-concerted or fortuitously coincident observations of some 'celestial signal' from different points on the Earth's surface; though before his death substantial progress had been made with the design of chronometers destined to solve this problem. It had long been the practice, however, to find the latitude quite simply by measuring the meridian altitude of the Sun or of some other celestial body. Various devices had been employed through the centuries for making this measurement; and in the seventeenth century the favourite instrument was the Davis's Quadrant, or Back-staff (Plate 4b). This consisted essentially of two graduated arcs BG, DE, with a *horizon-vane* C at their common centre, a *shadow-vane* F sliding on the smaller arc and an *eye-vane* A on the larger one. Standing with his back to the sun S, the observer viewed the horizon through slits in the eye-vane and the horizon-vane, and he adjusted the movable vanes so that the shadow-vane cast its shadow upon the slit in the horizon-vane. The altitude of the Sun was obtained by adding the angles indicated by the movable vanes on their respective arcs. Halley improved the instrument by attaching a lens to the shadow-vane; this was designed to collect the Sun's rays and to focus them on to the horizon-vane as a spot of light visible even when the shadow was too faint to be seen.[1]

It is not easy to imagine Sir Thomas Browne, the Norwich physician and master of English prose, living in the bustling world of Edmond Halley, but their life-spans overlapped by some twenty-five years. Writing to his son Edward on 6 July 1678, the author of *Urn Burial* remarked: 'It was very ingeniously done of Mr. Hally to take such a voyage for the observation of the starres about the south pole' (*The Works of Sir Thomas Browne*, ed. Geoffrey Keynes, London, 1928-31, vi, 95).

[1] *Biographia Britannica*, iv, 2496; H. O. Hill and E. W. Paget-Tomlinson, *Instruments of Navigation*, London, 1958, 10, 12.

When David Gill sailed to Ascension Island to observe the planet Mars at its opposition of 1877, he called with his party at St Helena and located the site of Halley's temporary observatory on a mountain overlooking the tomb where the body of Napoleon once rested. His wife described the visit:

> We did not know whether any record of this work remained in stone and lime, and it was a pleasant surprise to find, on the spot that an astronomer's eye at once picked out as the most favourable, a bit of low wall, duly oriented, and overrun with wild pepper. . . . This had been the Observatory, without doubt; and near to it is a quarry from which the stones for its erection had evidently been taken (Isobel Gill, *Six Months in Ascension*, London, 1878, 33).

3 *Catalogue of Southern Stars*

Within a few months of his return from St Helena, Halley published what was in fact the earliest catalogue of telescopically-determined star places to appear in print. It formed a small quarto volume written in Latin and entitled *Catalogus Stellarum Australium sive Supplementum Catalogi Tychonici*, etc., Londini, 1679. Although thus dated, the catalogue seems to have appeared late in 1678; writing to Hevelius on 11 November of that year, Halley describes it as 'paucos ante dies editum'. It was promptly translated into French by Augustin Royer as a companion to his own catalogue of stars visible from Europe (*Catalogue des Estoilles Australes, ou Supplement du Catalogue de Thycho, par Edmond Hallai*, Paris, 1679). Halley's star list may best be studied in the critical edition prepared by Francis Baily (*Memoirs of the Royal Astronomical Society* (1843), *13*, 35ff., 167ff.).

In constructing his Catalogue, Halley proceeded by measuring with his large sextant the angular distances of each selected southern star from at least two others included in Tycho Brahe's catalogue. Knowing the longitudes and latitudes of these reference stars, he was then able to calculate the corresponding co-ordinates of the selected southern star. In the resulting catalogue (which shows the positions of the stars at the epoch 1677), each star listed is identified by means of a brief description of its place in the constellation to which it belongs, e.g. *In* *humero dextro Sagittarii* (In the Archer's right arm). Adjoining each such descriptive label there are set out, in parallel columns, (i) the designations of the two or more known reference stars, (ii) the angular distances of these from the unknown star, (iii) the computed co-ordinates of the latter, (iv) the same according to the Rudolphine Tables of Kepler, and (v) the star's magnitude. Halley gives also the right ascensions and the north polar distances of the principal southern stars for use in navigation, together with a table of corrections for the accumulated effects of the 'precession of the equinoxes' which affects the co-ordinates of celestial objects.

Halley enhanced the value of the catalogue by including his actual measurements so that when the places of the reference stars should have been re-determined by Flamsteed or Hevelius the positions of the southern stars could be re-computed with a corresponding gain in accuracy. And, in fact, the places of 265 stars observed by Halley but invisible at Greenwich were incorporated in the catalogue forming part of the third volume of Flamsteed's *Historia Cœlestis Britannica* of 1725. They were brought up to the year 1726 by the editor, Abraham Sharp. The place of observation, St Helena, is mentioned, but Halley is not named, probably in obedience to the directions of Flamsteed, whose relations with the younger astronomer had long ceased to be cordial (*The Observatory* (1885), 8, 429f.). To the list of southern constellations which had become traditional, Halley added a new one in honour of his Royal patron—*Robur Carolinum*, or 'Charles's Oak, deservedly translated to heaven in perpetual memory of King Charles II of Great Britain etc. preserved under its leafy screen' (after the battle of Worcester). The catalogue was supplemented by a large folding planisphere also dedicated to the King but not found in all copies (Pl. 7).

Inserted here and there in Halley's star list are some interesting notes on observations that he made at St Helena. He noticed that several stars seemed to have grown fainter than the magnitudes assigned to them by Ptolemy or even by Bayer at the beginning of the seventeenth century; other stars seemed to have disappeared altogether. He remarked how empty of stars the sky appeared in the neighbourhood of the south celestial pole. He spotted the two *Nubeculæ*, called then by sailors (as now by

astronomers) the Magellanic Clouds. These appear in the earliest known map of the southern heavens, that of Peter Plancius (teacher of Petrus Theodorus), published in 1592 (E. B. Knobel, loc. cit.). Halley remarked that 'they reproduce exactly the whiteness of the Galaxy, and, examined through a telescope, they exhibit here and there small clouds and a few stars, from the concourse of which their white colour, like that of the Galaxy, is now believed to be produced'. This was a remarkable anticipation of the view that the Magellanic Clouds are systems largely composed of stars and comparable in status to the Galactic system of which the Sun is a member. The Clouds are, in fact, irregular nebulae which form, together with our Galactic system, the Great Nebula in Andromeda and about five others, a local group affording an instructively diverse sample of nebular types.

Halley was also one of the earliest observers to notice what is usually called the shortening of the seconds pendulum in low latitudes. The length l of a simple pendulum and the period T in which it performs one complete vibration are connected with the acceleration of gravity g by the well-known approximate formula $T = 2\pi\sqrt{l/g}$, from which it appears that any diminution in the value of g calls for a proportional decrease in l if T is to remain constant. Now upon approaching the equator from a higher latitude it is found necessary to shorten the pendulum slightly in order to make the clock keep correct time; and this is interpreted to mean that the acceleration g has suffered a small diminution which can be reasonably explained as a consequence of the Earth's rotation, which tends to throw objects off into space by a so-called centrifugal force; also in some degree as a consequence of the Earth's departure from a perfectly spherical figure. This phenomenon had been anticipated on theoretical grounds by Christiaan Huygens, and it had been observed by the French astronomer Jean Richer on his expedition to Cayenne some five years earlier.

Halley makes no mention of this matter in his catalogue; but seven years later in a paper on gravity, he wrote:

> At S. Helena in the Latitude of 16 Degrees South, I found that the Pendulum of my Clock which vibrated seconds, needed to be made

shorter than it had been in England by a very sensible space, (but which at that time I neglected to observe accurately) before it would keep time; and since the like Observations has [*sic*] been made by the French Observers near the Equinoctial; yet I dare not affirm that in mine it proceeded from any other Cause, than the great height of my place of Observation above the Surface of the Sea, whereby the gravity being diminished, the length of the Pendulum vibrating seconds is proportionably shortened (*Phil. Trans.* (1686), *16*, 7).

Again, Robert Hooke, writing to Newton on 6 January 1680, relates how

Mr. Hally when he returned from St. Helena told me that his pendulum at the top of the Hill went slower than at the Bottom which he was much Surprised at, and could not Imagine a reason. But I presently told him that he had solved me a query I had long Desired to be answered, but wanted opportunity, and that was to know whether the gravity did actually Decrease at a greater height from the centre.[1]

Both Halley and Hooke were thus at first mistaken as to the cause of the phenomenon, though Hooke, in the first rough draft of his letter, had added to the passage quoted the words: 'but I had not then thought of this Increase of the centrifuga [*sic*] virtue from the vertiginous motion of the Earth' (ibid., 312).

Newton clearly related the shortening of the seconds pendulum to the rotation of the Earth. Writing to Halley on 14 July 1686, he remarked that he first learned of this phenomenon (which, however, he had anticipated on theoretical grounds) from Robert Hooke, who cited 'ye expt of your Pendulum Clock at St Hellena as an argument of gravities being lessened at ye equator by ye diurnal motion. The expt was new to me but not ye notion' (ibid., 445). For in a memorandum he had made more than 15 years before 'I calculated ye force of ascent at ye Equator arising from ye earth's diurnal motion in order to know what would be ye diminution of gravity thereby' (ibid.). Again, in his *Principia*, Newton writes: 'Our friend Dr. Halley, about the year 1677, arriving at the island of St. Helena, found his pendulum clock to go slower there than at London, without marking the difference' (*Principia*, third edition, Book III, Prop. 20; F. Cajori's

[1] *Correspondence of Isaac Newton*, ed. H. W. Turnbull, Cambridge, 1959 etc., ii, 310; hereinafter cited as Newton, *Correspondence*.

Translation, 430f.). This phenomenon was early linked by Huygens, Hooke, Newton, and others with the question of the shape of the Earth. Since the so-called centrifugal force acts obliquely to normal gravity (except at the equator), it must deflect the plumb-line from passing through the Earth's centre. And since the ocean surface is everywhere perpendicular to the plumb-line, it must assume somewhat the figure of an oblate spheroid, bulging at the equator and having its short axis coinciding with the Earth's axis of rotation. And since the level of the land does not differ markedly from that of the ocean, the whole surface of the globe must approximate to the same figure. Hooke, in 1683, was drawing conclusions as to the figure of the Earth by postulating the equilibrium of two ideal 'canals' of liquid lying respectively along the polar axis and in the equatorial plane and communicating at the Earth's centre (*Posthumous Works*, 456); and this artifice was also employed by Newton and Huygens.

During his stay at St Helena, Halley observed a transit of the planet Mercury across the Sun's disc, which occurred on 28 October 1677. The weather was unusually favourable; he took up his station at the 24-foot telescope before dawn and became, so he claimed, the first astronomer to time both the planet's entry upon the solar disc and its exit. Comparing (in an appendix to the catalogue) his results with some observations of the same phenomenon made at Avignon in the south of France, he arrived, by a rough calculation, at an estimate of the Sun's distance from the Earth. The result was only about one-fifth of the true value. However; the attempt is of historic interest because it suggested to Halley the idea of utilizing, for the same purpose, a transit of the planet Venus (whose parallax is about three times that of the Sun), and it contains the germ of his later proposals in support of this project. Another appendix to the catalogue refers to the Moon's motion and is expressed in the language of the prevailing vortex theory of Descartes. Attention is drawn to a phenomenon which observation seemed to indicate: the lunar orbit is flattened and compressed towards the Earth in the syzygies (about new and full moon) and bulges out in the quadratures (about the first and last quarters).

In recognition of the great contribution he had made to astronomy by his expedition, and on the proposal of Sir Jonas Moore, Halley was elected a Fellow of the Royal Society at the Anniversary meeting of 30 November 1678. And in the Preface to his *Doctrine of the Sphere*, contributed to the first volume of Moore's *New Systeme of the Mathematicks*, Flamsteed acclaimed his friend as 'Our Southern Tycho, Mr. Edmond Halley'.

Halley, as we saw, had left Oxford without a degree; but on 3 December 1678, shortly after returning from his expedition, he was created M.A. *per literas regias*—by virtue of the King's letters—dated 18 November, 'which say that he had received a good account of his learning as to the mathematics and astronomy, whereof he hath gotten a good testimony by the observations he hath made during his abode in the island of St Helena &c.' (Anthony à Wood, *Fasti Oxonienses*, ed. P. Bliss, London, 1815 etc., ii, col. 368).

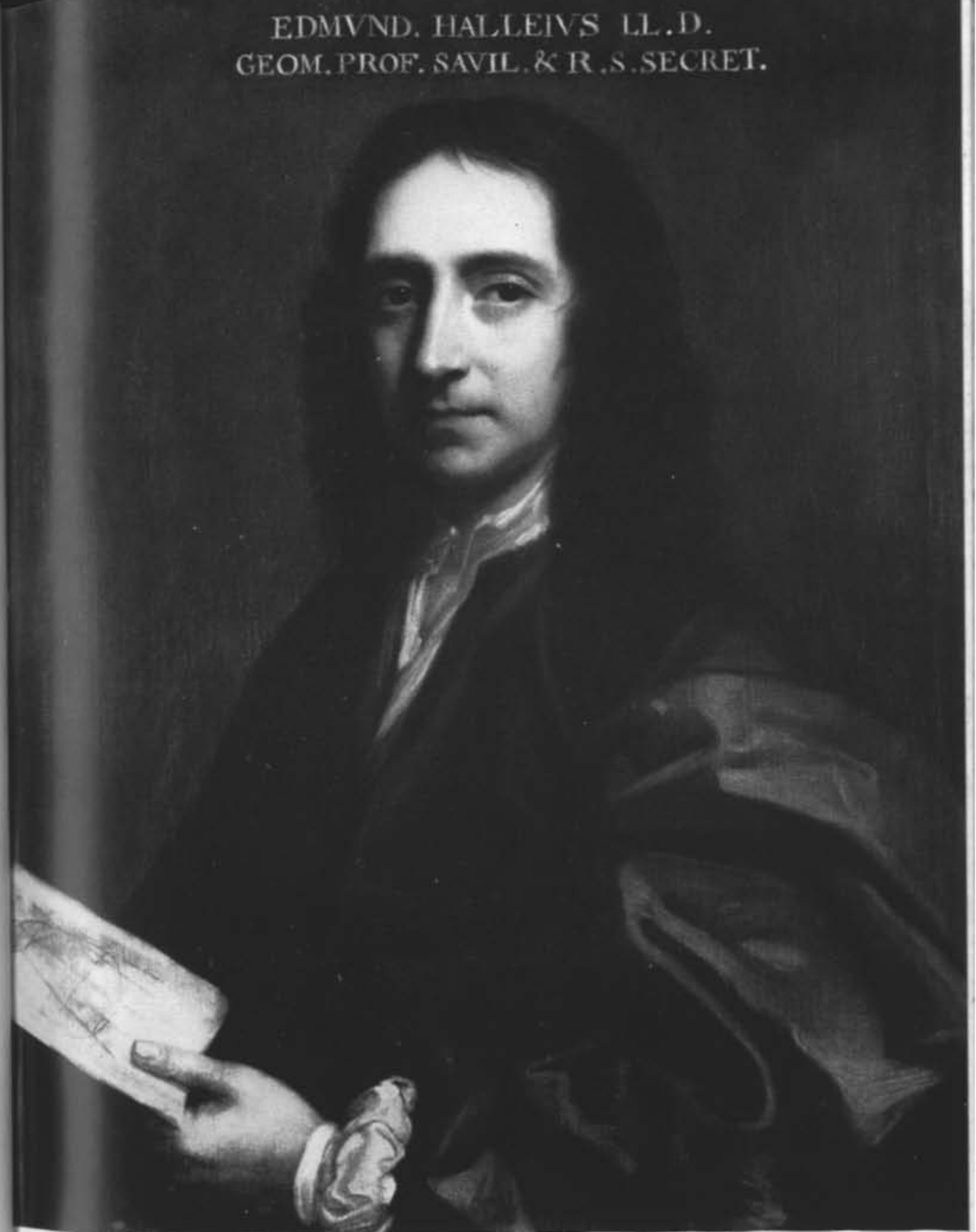


Plate 1 Edmond Halley as a young man, from a portrait in the possession of the Royal Society (*Royal Society*)

Plate 2 Edmond Halley as a naval officer, after a portrait by Sir Godfrey Kneller (*British Museum*)

Chapter 4

Danzig, Rome, Islington

1 *Visit to Hevelius*

Just a year after returning from St Helena, Halley set out on his travels again, this time to Danzig to visit a renowned German astronomer, Johannes Hevelius, as he had styled himself, latinizing his family name (Pl. 9a). Hevelius was born at Danzig in 1611, the son of a prosperous brewer. As a boy he was attracted to the study of astronomy by his mathematics teacher, Peter Krüger. But he also became a skilful craftsman in wood, metal, and glass, so that when, eventually, he settled down to work as an astronomer, he was able to construct his own instruments, the most refined and ornate that had ever been seen. He was also an expert draughtsman, engraver, and printer; and the books in which he described and illustrated his apparatus and observations are among the most beautiful productions of their kind. His career began with an extended tour, taking in Holland, England, and France; and after his return to Danzig he was largely occupied with civic duties and with carrying on the family business. However, he established, on the roofs of three adjoining houses overlooking the Vistula, an observatory which, unrivalled as yet by the royal foundations at Paris and Greenwich, must have been the finest such installation in Europe (Pl. 9b). Hevelius was elected a Fellow of the Royal Society in 1664. He was twice married, his second wife, Elizabetha Koopman, assisting him in his observations and figuring, as we shall see, in the story of his relations with Halley.

The instruments employed by Hevelius were of two principal types. On the one hand he made skilful use of the telescope for examining the Moon, sunspots, and comets; and to such studies he devoted several magnificent illustrated treatises. In the fashion of the time, he constructed telescopes of impressive length, hoping thereby to minimize the optical defects of the image. However, besides his telescopes, Hevelius possessed a battery of instruments of a type established by Tycho Brahe and serving for the measurement of celestial angles on the principle already explained. These instruments generally consisted of a quadrant or some other sector of a circle, stoutly constructed of wood or metal, and having its arc, or *limb*, graduated in angular measure and traversed by a radial pointer, or *alidade*; this was directed in turn to each of the selected stars whose separation was required by means of two sights, one consisting of a narrow slit and the other of a straight edge. The visual ray, passing through the one and grazing the other, defined the direction of the star. Or sometimes provision was made for making simultaneous settings on the two stars by two observers using the same instrument. By the 1660s these open sights were giving place to telescopic ones; but in the preparation of his (posthumously published) star catalogue Hevelius continued to rely entirely upon open sights, mistrusting the use of optical adjuncts as likely to falsify the observer's estimation of such angles. And this attitude he maintained, despite criticism, until his life's end. This policy brought him into conflict with Robert Hooke, one of the most inventive and versatile British men of science of the period, whom we shall have occasion to mention repeatedly in the following pages.

Robert Hooke was born, the son of a clergyman, in 1635, at Freshwater in the Isle of Wight. As a sickly child he amused himself with the construction of mechanical toys; in his schooldays at Westminster he excelled in both mathematics and classics. Going up to Oxford in 1653 he was soon taken up by the brilliant set of experimental philosophers who formed one branch of what was soon to become the Royal Society. He was introduced to astronomy by Seth Ward, the Savilian Professor of that science, and he became technical assistant to Robert Boyle, whom he helped materially in the design and construction of his improved air pump and in the verification of 'Boyle's Law' connecting the pressure and volume of a gas.

In 1668 Hooke, a keen champion of telescopic sights and a pioneer in their use, advised Hevelius to adopt them. Writing to Oldenburg late in 1672, Flamsteed, too, deplored the policy of

Hevelius: 'If M. Hevelius use not Glasses, I fear we shall but be where we were'. And in the following year, in a Latin letter to Cassini, he expressed the same view (*Phil. Trans.* (1672), 7, 5119; 1673, 8, 6000). The publication by Hevelius, in 1673, of the first instalment of his *Machina Cœlestis* prompted Hooke to devote part of his Cutler Lecture of 1674 to a critical review of the Danzig astronomer's observing technique (*Animadversions on the first part of the Machina Cœlestis* etc., London, 1674).

Hooke maintained that the costly instruments of Hevelius were not capable of measuring angles more accurately than those of Tycho Brahe; enlarging the instruments, or refining the methods of graduating them, availed nothing beyond a certain point owing to the limited resolving power of the eye: 'the power of distinguishing by the naked eye is that which bounds and limits all the other niceness'. He believed that Hevelius could have increased tenfold the accuracy of his projected star catalogue had he employed telescopic sights for making the underlying observations. He had formerly communicated something to this purpose to Hevelius, who replied, in a letter to the Royal Society, that these sights appeared to him less reliable than open ones, being more likely to suffer derangement. He seems also to have feared that lenses might produce unsuspected deviations of the rays refracted through them; or they might get broken. Hevelius used to measure the successive angular separations of eight stars distributed round the ecliptic, and he boasted that the intervals in right ascension or longitude added up to exactly 360°. Hooke questioned this claim: even if Hevelius could estimate angles to a single second of arc (the normal limit is two minutes) the effects of refraction should introduce a discrepancy of many seconds. Halley later made the same point in his paper on refraction. Hevelius read Hooke's lecture, and he complained to Oldenburg about its strictures upon methods which had stood the test of long experience. He felt that his good faith had been impugned by Hooke, whom he wrongly supposed to be an official spokesman of the Royal Society. It was accordingly placed on record, in the Society's Journal Book, 'that what Mr. Hooke had published against him [Hevelius] was done without any approbation or countenance from the Society' (T. Birch, *History of the Royal Society*, London, 1756-7, iii, 332).

In October 1678, Flamsteed invited Halley down to Greenwich to assist in the observation of a lunar eclipse; and in November he showed him a letter he had received from Hevelius expressing a desire to see the results of Halley's expedition. Halley wrote to Hevelius from Oxford on 11 November sending him a copy of his newly published catalogue of southern stars (already available though dated 1679). He writes that he would willingly re-calculate it employing fundamental star places determined by Hevelius instead of those derived from Tycho Brahe; and he would be greatly honoured if his star list could be immortalized by inclusion in the great catalogue which Hevelius was preparing to publish. In conclusion, Halley announces his intention of shortly visiting Danzig to become personally acquainted with Hevelius, to study his instruments and observe technique at first hand, and to consult him concerning the further advancement of astronomy (*Correspondence*, 41f.). The idea of such a visit may well have originated with Flamsteed, who was involved in the controversy about telescopic sights and would welcome direct comparison between the two modes of determining celestial angles. The Royal Society, to which, as we saw, Halley was elected just about this time, may have welcomed an opportunity of restoring friendly relations with the Danzig astronomer and of putting his claims to an independent test. Halley, indeed, carried with him a letter written, at the bidding of the Society, by a distinguished Fellow, William Croone, to whom Hevelius replied expressing his delight at the visit. Halley left London in the spring of 1679 and arrived at Danzig on 16 May.

Hevelius gave his own account of the episode and summarized some of the letters bearing upon the original controversy in his *Annus Climactericus* (Gedani, 1685), which dealt with his astronomical work in 1679, the year of Halley's visit and the climacteric, or forty-ninth year, of his own career as an observer of the heavens (a full account of the book was given in *Phil. Trans.* (1685), *15*, 1162ff.). It appears that on the very night of Halley's arrival the two astronomers commenced observations in friendly rivalry, Halley with his 2-foot telescopic quadrant, and his host with his 'great bronze sextant', a sixty-degree graduated arc six feet in radius and equipped with two sights (Pl. 10). Nearly all these observations survived the subsequent disastrous fire which destroyed Hevelius's observatory later in the same year, and they were published in the *Annus Climactericus*. Halley began by testing the consistency with which Hevelius could measure the angular separation of the stars Regulus and Spica. Perhaps (Hevelius suspected) he had chosen a season when the twilight was prolonged and the Moon nearly full to increase the observer's difficulties. However, working with his printer (unskilled in astronomy) Hevelius made successive estimates of the required angle differing by only 5 seconds of arc from a mean of 54° 1′ 50″. Subsequently Halley, taking turns at the fixed and movable sights, observed in co-operation with Hevelius, with his wife Elizabetha, with the printer, or with Johann Olhoff, Secretary of the City of Danzig and a kinsman of his host; and, although unskilled in the use of open sights, he was hardly ever as much as a minute out.

Halley described his experiences and conclusions in several letters. His earliest impressions of 'my Lord Hevelkyes Instruments and Observations' were given in a letter to Flamsteed, dated 7 June 1679 (*Correspondence*, 42f.). He was surprised at the consistency with which Hevelius could measure celestial angles with his sextant: 'Verily I have seen the same distance repeated severall times without any fallacy agree to 10″ . . . so that I dare no more doubt of his Veracitye'. Halley appears to have made trial also of the enormously long telescopes (up to 300 feet in length) which figure in the illustrated account of his instruments given by Hevelius. The lenses were fixed to long beams slung from poles; the beams suffered flexure under their own weight so that the lenses could not be brought into line, and Halley concluded that no good work could be done with such instruments. Upon taking leave of Hevelius early in July, Halley addressed to his host a testimonial letter dated the 8th of that month (*Correspondence*, 44f.). He confesses to having formerly doubted whether observations made with open sights might not be several minutes out, and to having wondered what considerations had dissuaded Hevelius from employing telescopic ones. Then came news that the Danzig astronomer was about to publish all his celestial measurements in one volume, affording a basis for a much more accurate and comprehensive star catalogue. Halley had come to Danzig partly to offer congratulations and partly to dispel his doubts. Hevelius had received him cordially and had allowed him to examine the instruments and repeatedly to be present when they were in use. 'I voluntarily offer myself as a witness of the barely credible certainty of your instruments against all who in days to come may seek to call your observations in question. With my own eyes I saw, not one or two but many observations of the fixed stars made with the great bronze sextant, by different observers and sometimes by myself, though little practised therein; and when the setting of the instruments was disturbed and a fresh setting made, the observations showed almost incredible agreement, never differing by more than an insignificant fraction of a minute'. The tone of this letter contrasts strangely with the views which Halley later expressed on the claims of Hevelius. In the debate about telescopic sights his sympathies were with Hooke; and in letters to Molyneux dated 1686 he confessed to having concealed his real opinion of his Danzig host's technique only for fear of hastening the departure of 'an old peevish Gentleman' (*Correspondence*, 60, 65).

Halley had intended to return home by way of Denmark, hoping to observe, in concert with Greenwich, any occultations or near approaches of the Moon to stars such as might serve to determine the longitude of Tycho Brahe's old observatory of Uraniborg, but this plan seems to have come to nothing. About two months after Halley left Danzig, a calamitous fire destroyed the observatory of Hevelius with most of his instruments, books, and recorded observations. However, with the assistance of well-wishers all over Europe, the veteran astronomer re-founded his observatory and continued his astronomical labours there until his death in 1687. Some time before news of the fire reached London the rumour spread that Hevelius was dead. Halley wrote to the aforesaid Johann Olhoff expressing the shock and grief with which he had received the news (*Correspondence*, 45ff.). He still hopes the report may not be true. Meanwhile he is sending Madame Hevelius a silk dress in the latest fashion which he had promised to buy for her; the total expense is £6 8s. 4d. Perhaps

she is not a widow, or she can save the dress until she is out of mourning or sell it profitably. He is sending the dress to Danzig on board the *Charity of Hull*; and he would like to be paid in copies of Hevelius's books, to be addressed 'To Mr. Edmond Halley, at his Father's House in Winchester Street, London'. Later correspondence between Hevelius and the English astronomers mainly related to the re-equipment of the Danzig observatory.

2 The Grand Tour; Marriage

After his return from Danzig, Halley seems to have lived for more than a year in London with his father; his mother had already died, probably in 1672. However, late in 1680 he embarked upon the 'Grand Tour' of France and Italy which customarily completed the education of young University men in easy circumstances. It was on 1 December that he set out for Paris in company with a school friend of his own age, Robert Nelson, son of a wealthy 'Turkey merchant' and already a Fellow of the Royal Society. Nelson was later to become a figure of some note in the religious life of England; he remained Halley's friend to his life's end. Writing to Hooke from Paris, Halley reported his arrival there on 24 December 'after the most unpleasant journey that you can imagine, having been 40 hours between Dover and Calais with wind enough'. During the journey he caught a glimpse of the great comet of 1680; he had already seen it in November, but it now re-appeared and was thought for a time to be a different object. He found it the subject of conversation among the 'Virtuosi' in Paris; and he tried without success to represent the observations by giving the comet a uniform rectilinear motion.

Halley soon made the acquaintance of 'Monsieur Cassini, who has been my very particular good friend', and in company with whom he observed the comet. This was Jean-Dominique Cassini, the founder of a family whose fortunes, through four generations, were to be closely bound up with those of the Paris Observatory. Born near Nice in 1625 and appointed to the Chair of Astronomy at Bologna, Cassini had made a special study of the surface features and rotation periods of the planets Mars and Jupiter, and he had tabulated the revolutions of Jupiter's satellites so that their vicissitudes could serve as celestial signals for the determination of longitudes, as suggested by Galileo. He had come on a temporary visit to Paris in 1669; but he soon assumed French nationality and became the virtual director of the Observatoire de Paris, founded as an offshoot of the Académie Royale des Sciences and completed in 1672. Cassini had collaborated in that year with Jean Richer in the determination of the solar parallax from concerted observations, made at Paris and Cayenne, of Mars in opposition. Further successes awaited him as an observer, but he was not so happy in the theoretical positions which he adopted.

Hooke's letters of introduction also made Halley known to the Protestant Henri Justel, Secretary to Louis XIV but soon to seek refuge from religious persecution by migrating to England, where he eventually became librarian to Charles II and in due course to James II. Justel's abstracts of the Breslau bills of mortality were later to afford the basis for Halley's mortality table. Already the astronomer was taking note of the vital statistics of Paris. He paced the city from north to south and from east to west and found it smaller than London; but it appeared to be more populous. The registers showed that half as many were married as were born, suggesting 'that it is necessary for each married Couple to have 4 Children one with another to keep Mankinde at a stand'.

Halley was tireless in his quest for new books desired by Hooke or needed for the Royal Society's library; those he could not buy he sought to inspect, copying out the vital portions. Henry Savile, the English Envoy in Paris, promised to help in procuring the wanted volumes. Robert Nelson bore a letter of introduction to Savile which Dean Tillotson of Canterbury had obtained for him from the Envoy's brother, the Earl of Halifax. Savile put up a plan to Nelson for purchasing a place at the court of Charles II, but Nelson would have none of it (C. F. Secretan, *Memoirs of the Life and Times of the Pious Robert Nelson*, London, 1860, 15f.).

Writing to Hooke from Saumur in May 1681, Halley announced that he and his companion intended to travel by way of La Rochelle, Bordeaux, Toulouse, Narbonne, and Montpellier

to Avignon; they seem also to have visited Lyons. By November they were in Rome, with Halley acknowledging a letter from Hevelius which had allayed his fears of having fallen into the Danzig astronomer's disfavour. While he was in Rome, Halley measured the Roman and the Greek foot at the Campidoglio, finding the Roman foot four-tenths of an inch shorter and the Greek foot one-twentieth of an inch longer than the London foot (*Correspondence*, 166f.). Late in 1681 Halley was called home on business, perhaps connected with the financial difficulties which were beginning to beset his father. He returned by way of Leghorn, Genoa, and Paris. Meanwhile, Robert Nelson stayed on in Rome, where he met the lady whom he was eventually to marry.

Within a few months of his return, Halley, too, was married. His bride was Mary Tooke, whose father was the Auditor of the Exchequer; and they were married at St James's, Duke's Place, on 20 April 1682. There were three surviving children of the marriage, a son, Edmond, who became a naval surgeon and who died about two years before his father, and two daughters, Margaret, who died a spinster, and Catherine, who was 'twice handsomely marryed'. Catherine's second husband, Henry Price, contributed memoirs of the astronomer to the account of his career given in *Biographia Britannica*.

After his marriage Halley set up house at Islington, which then lay on the northern outskirts of London. He established a small private observatory and equipped it with his well-tried collection of instruments. A conjunction of Saturn and Jupiter (when the two planets appeared to pass close to each other in the sky) afforded him occasion repeatedly to view Saturn with his 24-inch telescope and prompted a further contribution to the *Philosophical Transactions* (1683, *13*, 83ff.). Huygens had discovered Saturn's largest satellite, Titan, in 1655, and he had tabulated its revolutions round the planet in his *Systema Saturnium* of 1659. Since then nothing had been done on the theory of the satellite; and Halley found Huygens's tables 'considerably run out' and his estimate of Titan's period of revolution too short. By comparing a series of his own observations of the satellite with two made by Huygens, Halley deduced the period to be 15 days, 22 hours, 41 minutes, with which modern estimates agree, thus supplying the basis for a fresh tabulation. As for Saturn itself ('the most remote of all the Planets of our Vortex' he called it in Cartesian phrase), he thought ('this is but conjecture') that the planet's axis must be oblique to the Ring so that the plane of the latter intersects the planet's parallels of latitude. Otherwise some part of the Saturnian surface would be 'incommoded' by unbroken night for months or years together. Two other satellites had been discovered more recently by Cassini (he was to discover two more the following year), but Halley had not yet succeeded in seeing these.

3 *Longitude; the Royal Society*

In November 1682, Halley embarked upon a series of observations of the Moon and planets which he continued until 1684, working with his $5\frac{1}{2}$-foot sextant at his home observatory in Islington. His principal aim in these observations was, whenever the weather permitted, to determine the true place of the Moon so as to establish the corrections to be applied to the existing tables of the Moon's motion. The underlying motive for this enterprise was the hope of employing the corrected lunar tables for the determination of longitude at sea. The problem of the longitude had been steadily growing more urgent ever since the fifteenth century, when navigators largely abandoned coasting to venture forth upon the trackless ocean. The longitude, referred to a selected prime meridian, of any point on the Earth's surface is given by the constant difference between the local time at that point and the standard time as determined by an observer on the prime meridian. The determination of the local time presented no serious difficulties; the problem consisted in the simultaneous estimation of the standard time, say of Paris, or Greenwich.

Rewards for a practicable solution of this problem were offered from time to time by the governments and academies of the maritime states; and many claims were put forward by inventors. At first, the usual procedure was to rely upon a 'celestial signal' —some phenomenon, such as an eclipse or the occultation of some star by the Moon, which could be predicted in standard time and recorded in the local time of the observer. The practice of utilizing for this purpose the frequent eclipses of Jupiter's

satellites as they pass through the planet's shadow was suggested by Galileo and perfected by J.-D. Cassini. Meanwhile, as above mentioned, the suggestion was repeatedly made that the Moon might be regarded as the hand of a great clock measuring out standard time by its rapid and predictable travel through the constellations (in one hour the Moon moves through an angle roughly equal to its own breadth in the sky). It was a proposal on these lines that afforded occasion for the establishment of the Royal Observatory, as we have seen; and Halley's efforts to rid the current lunar tables of errors were also inspired by his zeal to promote navigation. It was many years later that he published the register of his Islington observations, together with his proposals for determining longitude, as an appendix to the second edition of Thomas Street's *Astronomia Carolina* (London, 1710), a standard collection of astronomical and mathematical tables with instructions for their use.

Experience early convinced Halley of the impracticability of all methods of finding longitude at sea except those depending upon a knowledge of the Moon's monthly motion through the constellations. Eclipses of Jupiter's satellites, though affording excellent signals for the determination of the longitudes of stations on land, could not be utilized for this purpose at sea, as a much larger telescope was needed to observe them than could be employed on a rolling ship. On the other hand, he had found that with only a little practice it was possible, in moderate weather, to manage a telescope 5 or 6 feet in length, which would suffice for observing appulses (near approaches) or occultations of stars by the Moon. And 'were we able perfectly to predict the true Time of the Appulse or Occultation of a Fix'd Star in any known Meridian; we might, by comparing therewith the Time observed on board a Ship at Sea, conclude safely how much the Ship is to the Eastward or Westward of the Meridian of our Calculus'.

It appeared that none of the current tables represented the Moon's motion with the required certainty. But the fault lay with the tables; it did not arise from 'uncertain Wandrings' on the part of the Moon. Evidently the lunar inequalities went through a regular cycle in the 'sarotic' period of 223 lunations (18 years, 11 days) after which the Moon returned to approximately its initial position relative to the Sun, to its node on the ecliptic, and to its apogee. Any error in the tables was repeated at the corresponding phase of the succeeding cycle. Halley accordingly resolved to follow the Moon through such an 18-year period, 'by a sedulous and continued Series of Observations, to be collated with the Calculus, and the Errors noted in an Abacus', from which at all times in a like situation of Sun and Moon the appropriate correction could be extracted. For this purpose, Halley referred to Jeremiah Horrox's tables in preference to Street's. Within 16 months he had observed on 200 days; 'But this Design of mine was soon interrupted by unforeseen Domestick Occasions, which oblig'd me to postpone all other Considerations to that of the Defence of my Patrimony: And, since then, my frequent Avocations have not permitted me to reassume these Thoughts'. So Halley wrote in 1716; but he was destined to observe the Moon through a sarotic period in his later years at Greenwich.

It was the death of his father and the resulting shock to his own fortunes that compelled Halley to break off his course of lunar observations at Islington. The elder Halley had brought up his children in a measure of affluence; but he was hard hit by losses resulting from the Great Fire and aggravated by circumstances connected with an unhappy second marriage which he contracted following the death of his first wife. Early in 1684 he came to a tragic end, his body being recovered from a river near Rochester. A verdict of murder was returned, but the evidence was consistent with suicide. Litigation over the estate ensued between the astronomer and his stepmother.

It may well have been these adversities of fortune that compelled Halley to accept a salaried office under the Royal Society. On 27 January 1686 he was appointed Clerk to the Society following the sudden resignation of Francis Aston from the Secretaryship. This engagement forced him temporarily to resign his Fellowship of the Society. The terms of appointment laid it down that the Clerk should be single and childless; but the requirement was waived in this instance. On 3 March following it was resolved 'that Mr. Halley draw up the *Transactions*; and that when they are so drawn up, they shall be perused and approved by one of the Secretaries'. On 16 June satisfaction was

expressed with Halley's performance of his duties (T. Birch, op. cit., iv, 454f., 462, 489). The *Philosophical Transactions*, which Halley was thus appointed to edit, was a periodical founded in 1665 as the private enterprise of Henry Oldenburg, the Society's first Secretary. Though encouraged and fed with material by the Royal Society and edited from time to time by its Secretaries (Halley among them), the *Transactions* continued to run (with occasional interruptions) as an independent journal up to 1752 (Volume 47), when a Committee of the Society was appointed to direct its publication. Even so, the journal did not assume the title of *Philosophical Transactions of the Royal Society* until 1776 (Volume 66).[1] Halley continued to edit the *Transactions* until 1692; his Clerkship seems to have effectively ended in 1696. To complete the record of his association with the Society: he served as its Secretary (in succession to Sloane) from 1713 to 1721, and he edited the *Transactions* again from 1714 to 1719.[2] Halley was one of the editors of the original (1712) edition of the *Commercium Epistolicum* in which a committee appointed by the Royal Society reported on the dispute between Newton and Leibniz concerning the invention of the calculus.

At one period Halley seems to have undertaken the preparation of an annual ephemeris giving particulars of celestial phenomena due to occur in the ensuing twelvemonth; in December 1686 this pressing duty served him as an excuse for delay in replying to a letter from Wallis, and a month later he presented his ephemeris (for 1687) to the Royal Society (*Correspondence*, 74; T. Birch, op. cit., iv, 521).

To anticipate Halley's subsequent movements: we are told that, upon leaving Islington, he removed to a house in Golden Lion Court, Aldersgate Street. He spent two years (1696-8) at Chester, and, after his appointment to the Savilian Chair at Oxford in 1704, he occupied a house in New College Lane. The Rigaud papers at the Bodleian are stated to contain a letter addressed 'To Dr. Halley at his house the corner of Bridgewater Square in Barbican [in the City of London], July 16, 1716' (*Smithsonian Miscellaneous Collection*, 1905, 48, 229ff.). And he ended his days at the Royal Observatory, Greenwich.

[1] See W. P. D. Wightman's paper in *Nature* (1961), *192*, 23f.
[2] See *Record of the Royal Society of London*, fourth edition, London, 1940, 342, 344.

Chapter 5

Halley and Newton

1 *The Tides at Batsha*

HALLEY's wide interests, extensive travels, and sociable disposition won him the acquaintance and esteem of many of the leading men of science of his day, both at home and abroad. But of all his personal relationships the most momentous for the future of science, and indeed for the destinies of mankind, was his enduring friendship with his greatest contemporary, Isaac Newton (Pl. 8). For it was Halley who first discovered the unsuspected progress that Newton had made towards supplying a long-sought mechanical explanation of the laws of planetary motion; it was Halley who tactfully persuaded the difficult genius to develop this crucial insight into a vast synthesis embracing all the principal phenomena of the solar system, and it was Halley who, when Newton's 'Incomparable Treatise' was completed, saw it through the press and himself paid the printing expenses.

Before describing this vital phase in Halley's career, it will be convenient to discuss his contributions of the same period to several problems already engaging Newton's attention, notably the motion of projectiles and the size of the Earth. First, however, we shall look at one of Halley's earliest papers dealing with a tidal phenomenon which it remained for Newton to relate to mechanical principles.

The *Philosophical Transactions* for 1684 contain an extract from a letter written by one Francis Davenport and describing some observations he had made of the peculiar course of the tides at the far-eastern port of Batsha, in the Gulf of Tong-King (*Phil. Trans.* (1684), *14*, 677ff.). He had noticed that there was only one high tide a day, the flood and the ebb each occupying about 12 hours. Twice in the month, at intervals of about 14 days, the

tides ceased altogether for a day or so, after which they built up to reach a maximum in about 7 days, and then again fell off. Davenport wrote his letter chiefly for the guidance of sea-captains who might intend to call at Batsha; and he drew up detailed instructions indicating at what hours of the tidal cycle large ships might safely be brought across the bar of the river there. However, Halley was struck by the scientific interest of these tidal phenomena, which appeared to have no parallel elsewhere; and in an appendix to Davenport's letter he attempted to reduce them to a rule.

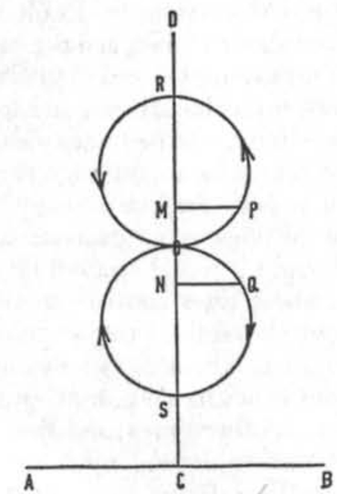


Fig. 6 The tides at Batsha.

Halley noticed that the tides ceased when the Moon was crossing the celestial equator and were greatest when it was farthest from the equator; that high water coincided with moonrise when the Moon was in the southern celestial hemisphere and with moonset when the Moon was in the northern hemisphere. He thought the phenomena could be represented by means of a geometrical scheme on the lines of Fig. 6, in which AB represents the level of the sea-bottom, and CD is a vertical scale on which the depth of the water is to be measured. The mean depth when the tides disappear being given by CO, let two equal circles OR, OS, be drawn, one above and one below O. These circles are to be described by points P, Q which set off from O when the Moon is on the equator and move at twice the Moon's angular velocity (as measured relatively to its node upon the equator). Perpendiculars PM, QN are drawn to CD; and M, N then represent the variable limits between which the tides must alternate during the fortnight that it takes for P and Q to describe their circles. After each fortnight, high tide changes over from moonrise to moonset, or vice versa. Expressed analytically, 'the increase of the waters should be allways proportionate to the Versed sines [one minus the cosine] of the double distances of the Moon from the Equinoctial points'. The depth CO represented about 15 feet, and the radius of each circle was about 4½ feet. This scheme seemed to fit what Davenport had observed or gathered from the natives, and to be confirmed by the reports of Captain Knox, who had since visited the port.

To understand the reason for this strange phenomenon seemed to Halley too much to hope for, particularly when attempts to establish a theory of the tides on our own coasts had met with but little success. He thought it would be useful if navigators would supply information about tides and currents farther along the China coast, down which the flood tide seemed to come. Again, owing to the inclination, about 5°, of the lunar orbit to the ecliptic, the Moon can sometimes recede about 10° more from the celestial equator than at other times; and since the height of the tides at Batsha seemed to depend upon the Moon's angular distance from the equator, Halley thought it would be worth ascertaining whether in some years the high tides were much higher than in others, or whether inundations ever occurred, and, if so, in what years.

Newton later sought to account for these anomalous tides on the principle that

> the tide may be propagated from the ocean through different channels towards the same port, and may pass quicker through some channels than through others; in which case the same tide, divided into two or more succeeding one another, may compound new motions of different kinds. . . . An example of this Dr. Halley has given us, from the observations of seamen in the port of Batsha.

Newton suggested that the tides observed at that port might be compounded of two tides, one passing from the South China Sea

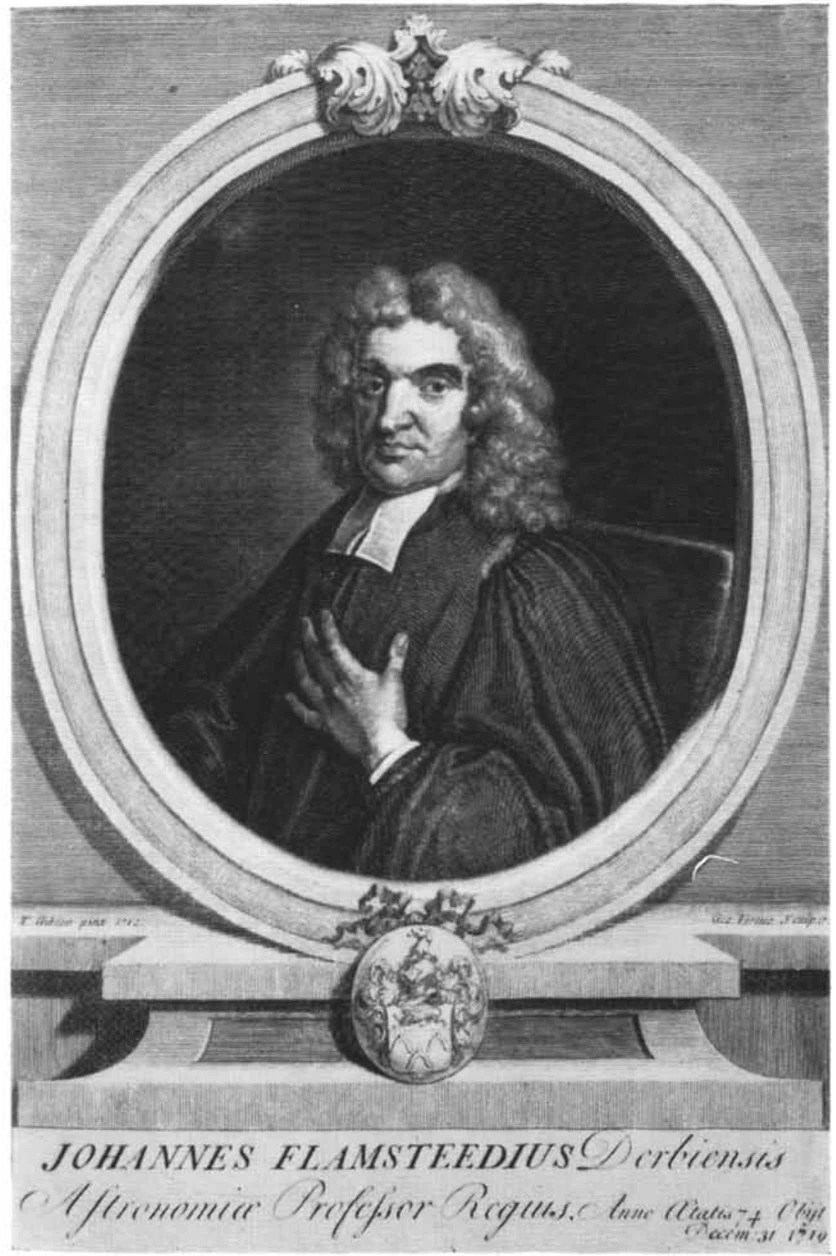


Plate 3 John Flamsteed, after a portrait by Thomas Gibson (*Crown Copyright reserved, Science Museum, London*)

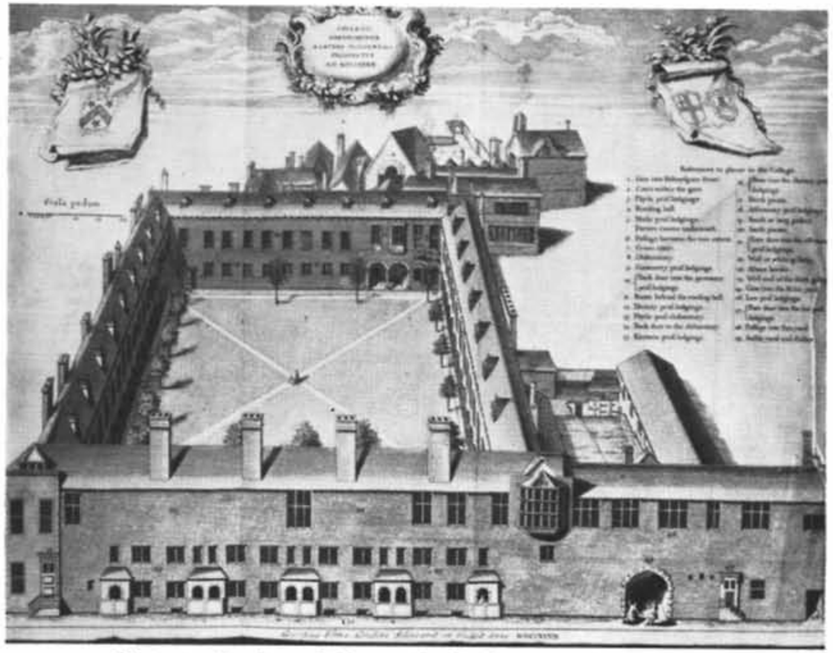


Plate 4a Gresham College, the birthplace and early home of the Royal Society

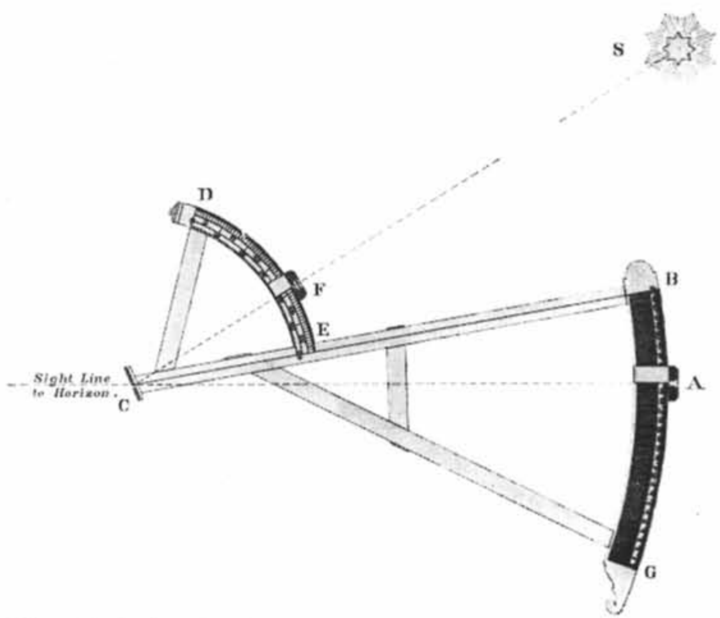


Plate 4b Davis's Quadrant or Back-staff (*Crown Copyright reserved, Science Museum, London*)

through what is now called the Hainan Strait and the other coming up from the Indian Ocean by way of the Malacca Strait.[1]

When, more than a century later, Thomas Young formulated his Principle of the Interference of Light, he claimed that his idea was original apart from some hints that he had received from the writings of Hooke, 'and except the Newtonian explanation of the combination of tides in the port of Batsha' (*Miscellaneous Works*, ed. G. Peacock, London, 1855, i, 202).

2 Gravity and Gunnery

The forthcoming publication of Newton's *Principia* was foreshadowed in a paper on gravity and its properties which Halley contributed to the *Philosophical Transactions* (1686, *16*, 3ff.). He begins with a brief, critical review of the stock explanations of gravity, the cause of which seemed as mysterious as its effects were manifest. Descartes had supposed the Earth to be surrounded by a vortex of revolving particles having the same axis as the Earth. He grasped the principle of inertia, later to be expressed in Newton's first Law of Motion, and he realized that each particle of the vortex would constantly tend to travel away along the tangent to its circuit round the Earth and thereby to recede into space. The more rapidly moving (aethereal) particles, he supposed, showed this tendency in a more marked degree, and they prevailed over the more sluggish (terrestrial) bodies which they accordingly forced down to occupy the space they were vacating, no void being permissible.

The original Cartesian hypothesis seemed to suggest that bodies should fall towards the axis rather than towards the centre of the Earth; but it was amended by Huygens who supposed the particles of the vortex to revolve in every possible plane about the Earth's centre. A rival theory, due to Isaac Vossius, attributed gravity to the diurnal rotation of the Earth; but Halley thought that such rotation should have the opposite tendency, and that, on Vossius's hypothesis, gravity should decrease towards the poles, whereas observation seemed to suggest that it fell off slightly towards the equator. Other writers saw the cause of gravity in the pressure of the air, but this was to mistake the effect for the cause. The classic experiment with a feather falling

[1] See *Principia*, Book III, Proposition 24; F. Cajori's translation.

in an evacuated tube proved that the removal of the air actually increased the speed of fall. Reference to a supposed analogy with magnetic attraction merely explained one mysterious phenomenon by invoking another equally so; and the doctrine of a 'sympathy' existing between the Earth and heavy bodies was little more than a re-statement of the observed facts in other words. But while the 'efficient cause' of gravity was obscured, its purpose, or 'final cause', was clear: it was ordained by the Creator to preserve the Earth and the celestial bodies from disintegration.

As a scientist, Halley turned gladly from such speculations to the task of formulating mathematically the observed behaviour of bodies moving freely under gravity. The rules had already been largely made out by Galileo, Torricelli, and Huygens, 'and now lately by our worthy Country-man, Mr. Isaac Newton, (who has an incomparable Treatise of Motion almost ready for the Press)'. Halley summarizes the well-established properties of gravity. All bodies tend to descend towards the centre of a sphere to which the ocean surface exactly conforms and from which the contour of the land surface departs but little. This centre must have remained fixed throughout historic time: had it suffered even a slight displacement the sea would have flooded low-lying lands in that part of the Earth towards which the displacement was directed. And no such inundations are on record, except for the biblical Deluge which, indeed, may best be explained by supposing the centre to have been temporarily shifted towards the then inhabited parts of the Earth. A displacement of but one two-thousandth part of the Earth's radius would have sufficed to submerge the highest hills.

Gravity, again, appears of equal intensity at all points equidistant from the centre of the Earth, the length of a pendulum which beats out seconds being everywhere nearly the same: the exception to this rule observed by Halley at St Helena has already been noted. If the resisting medium is removed, all bodies fall in the same time; there is no positive quality of levity. The force of attraction varies inversely as the square of the distance from the attracting centre, 'the principle on which Mr. Newton has made out all the Phenomena of the Celestial Motions, so easily and naturally that its truth is past dispute'. This law of attraction

holds for the Sun, the Earth, Jupiter, and Saturn (the bodies then known to possess satellites), 'and thence is reasonably inferred, to be the general principle observed by Nature, in all the rest of the Celestial Bodies'.

Halley concludes this part of his paper with eleven propositions on motion under gravity. The first three of these relate to a free fall from rest and establish the fundamental kinematic relations connecting distance fallen, velocity acquired, and time of fall, the acceleration being supposed known. Geometrical proofs are given of the kind that Galileo had taken over from medieval sources and which are still found in elementary textbooks of mechanics, where, for example, velocity is plotted against time and the area under the curve gives the distance travelled. Proposition IV shows how to estimate the acceleration of gravity by measuring the length of a simple pendulum beating seconds, as had been explained by Huygens.

In considering next the motion of a projectile fired at a given elevation above the horizontal, Halley simplifies the problem by ignoring not only the effects of air resistance upon the course of the projectile, but also the diminution of gravity with increase of height and the fact that the verticals at different points of the trajectory, or arc of flight, are not strictly parallel but converge towards the centre of the Earth. Two further axioms are required, one the equivalent of Newton's first Law, establishing the perseverance of undisturbed motion, the other equivalent to the parallelogram of velocities and asserting that a body subjected to two rectilinear displacements at once arrives at the same terminus as it would do if they acted upon it separately.

The propositions that follow establish the elementary properties of projectile motion, as already analysed by Torricelli: the trajectory is a parabolic arc; the horizontal range is proportional to the sine of twice the angle at which the cannon is elevated and is therefore a maximum when the elevation is 45°; the vertical height attained by the projectile with a given velocity of projection is proportional to the versed sine (1—cosine) of twice the elevation, or, as we should say, to the square of the sine of the elevation; and the time of flight is proportional to the simple sine of the elevation. These propositions (V—VIII) are established from geometrical properties of the parabola.

Of the concluding three propositions (IX—XI), Halley stars the tenth as the heart of his discourse, for the sake of which the rest was chiefly composed. It sets the problem of calculating the elevation at which a projectile must be fired, given the velocity of projection, in order that it shall pass through a point whose horizontal distance and height above (or depression below) the horizontal plane is known. This proposition was of great use in gunnery, and Robert Anderson had solved the problem in his *Genuine Use and Effects of the Gunne demonstrated* (London, 1674); but his method involved much tedious calculation.

Halley had hit upon a simpler procedure in 1678, proceeding somewhat as follows: Let A be the point of discharge of the projectile (Fig. 7), v its velocity of projection, and g the acceleration of gravity. Let C be the point through which it has to pass,

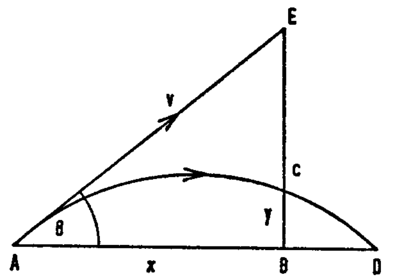


Fig. 7 Aiming a projectile.

the horizontal distance AB (x) and the height BC (y) being given as well as the *parameter* of the parabola, $\frac{2v^2}{g}$ ('half the Parameter is the greatest Random', or range), denoted by p. It is required to find what elevation θ must be given to the gun. Produce BC to meet the tangent at A in E. Then if t is the time of flight to C,

$$x = vt\cos\theta, \text{ and } EC = \tfrac{1}{2}gt^2 = \frac{gx^2}{2v^2\cos^2\theta}$$

But $EC = x\tan\theta - y$

$$\therefore\ x\tan\theta - y = \frac{x^2}{p\cos^2\theta}$$

$$\therefore\ px\tan\theta - py = x^2\sec^2\theta = AE^2 = x^2 + x^2\tan^2\theta$$

$$\therefore\ \tan^2\theta - \frac{p}{x}\tan\theta + \frac{py}{x^2} + 1 = 0$$

This is a quadratic in tan θ, and normally it has two different roots which represent two different elevations at which the projectile may be fired so as to hit C. The two roots may be equal and C represent the extreme range in the direction AC. The condition for equality of the roots leads to the polar equation of the parabola which envelopes all possible trajectories with the given initial velocity, and in that plane of projection. The existence of this so-called 'parabola of security' (whose focus is the point of projection) was known to Torricelli (*De motu gravium*, Florentiae, 1644, Liber II, Prop. 30; *Opere*, ii, 178f.). Halley's analysis takes account of the alternative case where C is depressed below the horizontal AD; and it is supplemented by a geometrical construction. The concluding theorem gives a rule for finding the speed of the projectile at any point on its trajectory.

Halley was well aware that this simple theory of projectiles was not strictly applicable if account were taken of the resistance of the air, which he supposed might be directly proportional either to the speed or to the square of the speed of the projectile (the two cases usually considered in textbooks) and inversely proportional to its specific gravity and diameter, jointly. He was misled by the available experimental evidence into believing that air resistance would exert only a negligible effect upon the flight of a massive missile.

It had formerly been the practice always to use the same quantity of powder in loading a mortar, and then to control the range by adjusting the elevation of the piece. However, in Halley's day it was becoming customary to vary the charge so as always to strike the target at extreme range. Besides saving powder, this had the advantage that the bomb need never fall too nearly vertically, burying itself in the ground before exploding. Moreover, at extreme range, errors in judging the elevation of the piece were of least consequence; it was for this reason that mortars used at sea were usually discharged at an elevation of about 45°. With these considerations in mind, Halley, about ten years later, returned to the problems of gunnery in order to communicate a new ballistic theorem he claimed to have discovered (*Phil. Trans.* (1695), *19*, 68ff.). This stated that, when a projectile is to be fired from a point O to strike an object P (whether above or below the horizontal) at maximum range along the inclined plane OP, then the direction OV in which the gun is pointed must bisect the angle between OP and the vertical OY (Fig. 8). This suggested a practical method for aiming a mortar: fix a mirror to the muzzle at right angles to the axis of the piece, and adjust the elevation of the latter until the reflection of the target is seen upon looking down a plumb-line into the mirror. (A particular case of this theorem is the rule that an elevation of 45° is required to give extreme range on a horizontal plane.) It only remained to determine experimentally how the maximum range depended upon the charge employed and to engrave this information on the gun as a standing direction to bombardiers.

Halley was frequently drawn into the discussions that arose in the Royal Society on problems of practical mechanics, such as the relation between the speed with which a liquid flows out from an

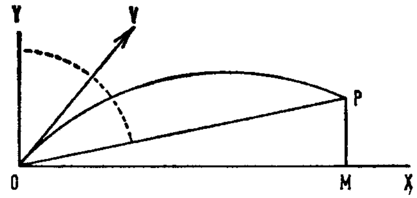


Fig. 8 Halley's ballistic theorem.

opening in a vessel, or the height to which it rises as the jet of a fountain, and the head of pressure acting upon it. Other unpublished papers of his deal with the measurement of the force of running water, or of the wind, and with the flight of birds (*Correspondence*, 147ff.).

3 *The Degree of the Meridian*

Halley's interests extended to the problem of determining the size of the Earth. Attempts to estimate this fundamental specification go back at least to the third century before Christ, when Eratosthenes of Cyrene established the standard procedure followed in principle by subsequent, more refined determinations.

Let Fig. 9 show a section of the Earth containing the polar axis PP' and let QQ' be the trace of the equatorial plane. Along the meridian PQ let a limited arc AB be measured off. From the

stations A, B, respectively, are measured the meridian zenith distances ZAS, Z'BS, of some selected celestial object S. Assuming AS and BS to be parallel, the difference between these two measured angles equals the angle AOB (which is the difference of latitude of A and B); and this bears the same proportion to 360° as the (known) arc AB bears to the circumference of the Earth. The circumference is thus calculable, or equivalently (as it was commonly expressed), 'the length of a degree of the meridian'. Formerly the meridian arc involved was laboriously paced out or otherwise directly measured, but early in the seventeenth century Willebrord Snell, the 'Dutch Eratosthenes', improved upon this

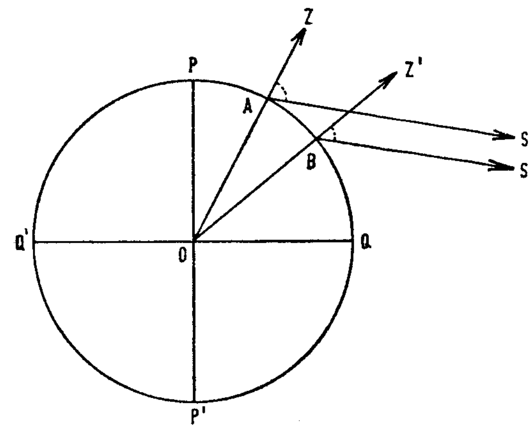


Fig. 9 Measuring a degree of the meridian

procedure, connecting the arc by triangulation with a short, accurately determined base-line. However, it was the Parisian astronomer Jean Picard who carried out the classic seventeenth-century determination of the length of the degree. He was the pioneer in the use of telescopic instruments both to survey his arc (extending 80 miles through the plains north of Paris) and to measure the terminal zenith distances of the selected star. The publication, in 1671, of Picard's greatly improved estimate of the degree marked an epoch in geodesy; it was formerly thought to have had a decisive influence upon Newton's decision to proceed with his gravitational hypothesis, which depended for its verification upon an accurate knowledge of the Earth's radius.

Fifteen years later, the Royal Society, apparently at Halley's instigation, took up again this perennial problem and resolved, under date 23 June 1686, that the Treasurer, 'to encourage the measuring of a degree of the Earth, do give to Mr. Halley fifty pounds or fifty copies of the *History of Fishes*, when he shall have measured a degree to the satisfaction of Sir Christopher Wren, the President, and Sir John Hoskyns' (T. Birch, *History of the Royal Society*, iv, 491). Halley sought the advice of John Caswell, an Oxford mathematician and geodesist, as to the best procedure, complaining that the grant was too small to allow of a triangulation of the meridian arc such as Picard had effected (*Correspondence*, 67). However, he tried this method in August 1686; but the attempt seems to have ended in failure, as he explained to Wallis:

> I found a great and insuperable difficulty to come to the objects I had seen at a great distance, for the country people I observed could tell me nothing of places above 7 or 8 miles off; And that at about 20 miles North from London, the country is very thick of high Woods, and the hills so near of a height, that there were no conspicuous objects to be found (*Correspondence*, 70f.).

The proposal to re-imburse Halley with unsold copies of Francis Willughby's *Historia Piscium* was no isolated instance of economy by the struggling Society. On 6 July 1687 we read: 'Resolved that Halley be given 50 copies of *History of Fishes* instead of £50 salary' (T. Birch, op. cit., iv, 545).

4 *The Preparation of the* Principia

Halley's fruitful co-operation with Newton dates from the year 1684; but the ideas which then began to receive systematic exposition as the *Principia* grew to completion had already largely taken shape, and not alone in the mind of Newton.

When Newton and Halley were students the system of natural philosophy generally taught at the Universities was that elaborated by René Descartes in his *Principia Philosophiæ* of 1644. It was there that he published what appears to have been the earliest general formulation of the Law of Inertia, familiar to us as Newton's first Law of Motion, which asserts that a body once projected tends to continue to move uniformly in a straight line. Descartes also postulated the purely geometrical space, devoid of physically privileged points or directions, in which such inertial motion is ideally possible. This was an important advance from the Aristotelian conception of space, and even from that of Galileo, who never seems to have held the doctrine of inertia in all its generality.

This doctrine was adopted by some who did not accept the whole Cartesian system, and notably by Robert Hooke. By 1666 he had grasped the vital point that what is needed to keep a planet moving in its orbit is not a tangential force pushing it along from behind, but a radial force pulling it in towards the Sun, or other centre of attraction, so that it does not travel off along a straight line into outer space. He identified this force with an attraction exerted by the Sun upon material bodies in its neighbourhood; and by 1670 he had imaginatively linked up this force with terrestrial gravity, supposing that all the heavenly bodies attract to their respective centres not only their own parts but one another. Already in 1662 Hooke had vainly sought to establish experimentally that the force of gravity fell off with increasing height above the Earth's surface; later he sought to prove that it diminished with increasing depth below the surface. And he adopted the hypothesis (perhaps as an easy deduction from Kepler's third Law) that gravitational attraction varied inversely as the square of the distance from the attracting body. Hence the idea of a gravitational force, familiarly manifested in terrestrial gravity but of cosmic range, was not unheard of when Newton first began to direct his attention to celestial mechanics.

Born near Grantham in Lincolnshire in 1642, Newton went up to Cambridge in 1661 and graduated in 1665. It appears that he began to investigate the problem of gravitation in 1666. He was then living at home, the University having been closed on account of the Plague; and the story that his thoughts were turned to the mystery of gravity by the fall of an apple in his orchard is well attested. He wondered whether the power of gravity, observed to extend to the tops of mountains without sensible diminution, might not reach as far as the Moon and influence its motion, or even keep it in its orbit. Starting from Kepler's third Law (that the squares of the periods of revolution of the planets are proportional to the cubes of their mean distances from the Sun), Newton was able to infer that the force which keeps the planets in their (supposedly circular) orbits varies inversely as the square of the distance from the central Sun; and, assuming this law, he calculated what the Earth's attraction upon the Moon must be. When he compared this force with the one actually required to keep the Moon in its roughly circular orbit (as given by a rule familiar to students of mechanics), he found them 'answer pretty nearly'. However, Newton did not at that time pursue the matter, probably because he had not yet established rigorously that, in calculating its attraction upon an external body, the Earth could be treated as a massive particle concentrated at its own centre.

Newton's attention was recalled to the problem by some correspondence he had with Hooke in 1679 and the following year concerning the path of a falling body on the assumption that the Earth offered no resistance to its passage.[1] It was apparently under the stimulus of this discussion that Newton went on to make the vital discovery that a planet revolving round the Sun under the inverse square law of attraction thereto must describe an ellipse (more generally, a conic) with the Sun in one of the foci (and conversely) as stated in Kepler's first Law; also that the second Law (that the radius vector joining Sun and planet sweeps out equal areas in equal times) must hold good for *any* orbit described under a force directed to a fixed origin. This was Newton's crucial and original contribution to celestial mechanics; neither Hooke nor anyone else possessed the mathematical technique necessary for rigorously establishing these results. However, Newton was strangely lacking in the normal impulse to communicate his discoveries even to his friends, who continued to be baffled by the problem he had solved; he 'threw the calculation by being upon other studies' (letter to Halley, 14 July 1686).

It is at this point that Halley comes into the story. Assuming Kepler's third Law and treating the planetary orbits as circles, he, too, had inferred that the Sun's attraction upon the planets must vary inversely as the square of the distance; but he could progress no further towards a mechanical explanation of the elliptic orbits. That was the situation in January 1684 when (as he wrote to Newton on 29 June 1686) Halley

[1] For these and other letters cited below, see Newton, *Correspondence*, ii, passim.

came one Wednesday to town, where I met with Sr. Christ. Wrenn and Mr. Hook, and falling in discourse about it, Mr. Hook affirmed that upon that principle [of the inverse square law] all the Laws of the celestiall motions were to be demonstrated, and that he himself had done it; I declared the ill success of my attempts; and Sir Christopher, to encourage the Inquiry said, that he would give Mr. Hook or me 2 months time to bring him a convincing demonstration thereof, and besides the honour, he of us that did it, should have from him a present of a book of 40s. Mr. Hook then said that he had it, but that he would conceale it for some time, that others triing and failing, might know how to value it when he should make it publick, however I remember Sir Christopher was little satisfied that he could do it, and tho Mr. Hook then promised to show it him, I do not yet find that in that particular he has been as good as his word.

It was then that Halley thought of consulting Newton at Cambridge:

The August following when I did my self the honour to visit you, I then learnt the good news that you had brought this demonstration to perfection, and you were pleased, to promise me a copy thereof, which the November following I received with a great deal of satisfaction from Mr. Paget; and thereupon took another Journey down to Cambridge, on purpose to conferr with you about it, since which time it has been entered upon the Register books of the Society (Newton, *Correspondence*, ii, 442).

Newton had, in fact, mislaid his calculations and could not immediately reproduce them. He continued his investigations and set out the results in what became his professorial lectures, *De motu corporum*, delivered in the Michaelmas term of 1684. Following his November visit to Cambridge, at a meeting of the Royal Society held on 10 November 1684,

Mr. Halley gave an account, that he had lately seen Mr. Newton at Cambridge, who had shewed him a curious treatise, *De motu*, which, upon Mr. Halley's desire, was, he said, promised to be sent to the Society to be entered upon their register. Mr. Halley was desired to put Mr. Newton in mind of his promise for the securing the invention to himself till such time as he could be at leisure to publish it. Mr. Paget was desired to join with Mr. Halley (T. Birch, op. cit., iv, 347).

The 'curious treatise' which Halley saw at Cambridge was probably the manuscript of Newton's lectures, which covered the early propositions of the *Principia*. Halley pressed Newton to continue his researches and to publish his discoveries in celestial mechanics. What may initially have been intended merely as a series of papers destined for inclusion in the *Philosophical Transactions* grew into a great book.

A turning-point in Newton's investigations may well have been his discovery of the exact law of attraction between the Earth, conceived as a sphere of uniform density, and a particle situated somewhere near it. In calculating the Earth's attraction upon the Moon, a quarter of a million miles away, it had seemed a reasonable approximation to treat the Earth as a massive particle concentrated at the centre of the terrestrial globe; but it was not until 1685 that Newton succeeded in proving (an immense gain in rigour) that this substitution is strictly valid even when the object attracted is close to the Earth's surface (like the apple in the orchard), neglecting only the effect of the Earth's compression into a slightly spheroidal figure. The work was completed by 28 April 1686, when the manuscript, dedicated to the Royal Society, was presented by Dr Vincent to the assembled Fellows and by them committed to the care of Halley, their recently appointed Clerk, for a report.

The progress of the *Principia* towards publication can be followed in the Minutes reproduced by Birch and in the correspondence which passed between Newton and Halley in 1686-7. On 22 May 1686 Halley was able to announce that the Royal Society had resolved to publish the book at their own expense and had appointed him to superintend the printing operations. However, it would appear that the Society was not just then in a financial position to incur the expenses of publishing the book; and on 2 June it was ordered 'that Mr. Newton's book be printed, and that Mr. Halley undertake the business of looking after it, and printing it at his own charge; which he engaged to do'. This despite the fact that his own worldly fortunes were at a low ebb following the tragic death of his father in 1684, so that, with a wife and young family dependent upon him, he had been glad to accept the office of salaried Clerk to the Society a few months before. Not many copies of the book were printed, for it could hope to find but few readers; and Halley must have been considerably out of pocket as a result of the venture. Besides

incurring the financial responsibility for the volume, he collected much material for Book III; he also read the manuscript critically and corrected the proofs.

When Halley commenced his editorial labours in May 1686, he first consulted Newton's wishes as to the design of the projected volume. However, the ensuing correspondence was largely occupied with the claims of Robert Hooke, who alleged that it was from him that Newton had learnt of the inverse square law of gravitation. Newton summarized the course of his debate of 1679-80 with Hooke concerning the path of a projectile in the presence of a centre of attraction; and he contended that both he and Sir Christopher Wren had arrived at the inverse square law earlier than Hooke. Newton was so irritated by Hooke's claim that he threatened to suppress the third Book of the *Principia* in which the doctrine of gravitation is applied to account for the mechanics of the solar system: 'Philosophy is such an impertinently litigious Lady, that a man had as good be engaged in lawsuits, as have to do with her'. However, Halley's tactful handling of the situation averted this calamity; and Book III was duly included in the published work. Halley wrote

> I am heartily sorry that in this matter, wherein all mankind ought to acknowledge their obligation to you, you should meet with any thing that should give you disquiet, or that any disgust should make you think of desisting in your pretensions to a Lady, whose favours you have so much reason to boast of. Tis not shee but your Rivalls enviing your happiness that endeavour to disturb your quiet enjoyment, which when you consider, I hope you will see cause to alter your former Resolution of suppressing your third Book, there being nothing which you have compiled therein, which the learned world will not be concerned to have concealed (Newton, *Correspondence*, ii, 441).

The *Principia* received the *imprimatur* of the President of the Royal Society (Samuel Pepys the diarist held the office at that time) on 5 July 1686, and it issued from the press in July 1687, graced by laudatory Latin verses which Halley had penned in honour of the illustrious author.

It would be out of place to include here more than a brief summary of the contents of this great scientific classic. The work, divided into three Books, commences with definitions, the three Laws of Motion familiar to students of mechanics, and the postulation of absolute time and space, the abandonment of which conceptions early in the present century marked the break between 'Newtonian' and modern physics. Book I deals chiefly with the unresisted motion of particles and uniform spherical bodies attracted to fixed centres, or mutually, under forces varying with distance according to specified laws, in particular, according to the law of the inverse square. Book II treats of the motion of bodies in a resisting medium, with the elements of hydromechanics and a refutation of the theory of vortices upon which Descartes had based his system of cosmology. In Book III Newton applied the results thus established to the explanation of the principal phenomena of the solar system: the motions of the planets according to Kepler's Laws, the spheroidal shape of the Earth, the principal inequalities of the Moon's motion, the tides, the precession of the equinoxes, comets.

Following the publication of the *Principia*, Halley urged Newton to continue his investigations:

I hope you will not repent you of the pains you have taken in so laudable a Piece, so much to your own and the Nations credit, but rather, after you shall have a little diverted your self with other studies, that you will resume those contemplations, wherein you have had so good success, and attempt the perfection of the Lunar Theory which will be of prodigious use in Navigation, as well as of profound and subtile speculation (5 July 1687; ibid., 482).

In pursuit of this design, Newton was glad to draw upon observations of the Moon supplied to him by Flamsteed. He also received letters and visits from Halley at Cambridge up to the time of his appointment as Warden of the Mint. Two further editions of the *Principia* were published in Newton's lifetime, that of 1713, edited by Roger Cotes, and the considerably extended edition which Henry Pemberton brought out in 1726.

Halley heralded the appearance of Newton's *Principia* with a generous review in the *Philosophical Transactions* (1687, *16*, 291ff.).

This incomparable Author having at length been prevailed upon to appear in publick, has in this Treatise given a most notable instance of the extent of the powers of the Mind; and has at once shewn what are the Principles of Natural Philosophy, and so far derived from them their consequences, that he seems to have exhausted his argument, and left little to be done by those that shall succeed him. His great skill in the old and new Geometry, helped by his own improvement of the latter, (I mean his method of infinite Series) has enabled him to master those Problems, which for their difficulty would have still lain unresolved, had one less qualified than himself attempted them.

And again:

It may be justly said, that so many and so Valuable Philosophical Truths, as are herein discovered and put past Dispute, were never yet owing to the Capacity and Industry of any one Man.

In 1687 an advance copy of Newton's *Principia* was presented to King James II. It was accompanied by a paper in the form of a letter to the monarch in which Halley gave a brief account of the contents of the book, with especial reference to Newton's theory of the tides, which James, as a former naval commander, was likely to find of particular interest. This paper was printed as a tract (London, 1687); and ten years later it was published in the *Philosophical Transactions* (1697, *19*, 445ff.). In this latter version, the original opening and closing passages of the paper are omitted, and there is an editorial note by Sloane explaining that the paper was re-published for the convenience of readers who felt curiosity as to the cause of the tides yet were unequal to the mathematical demands of Newton's treatise. Halley follows fairly closely the doctrine of the *Principia* (Book III, Prop. 24), referring to the same lettered diagram; and he reproduces Newton's suggested explanation of the anomalous tides at Batsha to which reference has already been made: 'So that the whole appearance of these strange Tides, is without any forcing naturally deduced from these Principles, and is a great Argument of the certainty of the whole Theory'. Should King James encounter any difficulties in his study of the work, Halley offered to clear them up if admitted to the Royal presence (*Correspondence*, 85f.).

In the years immediately following the publication of the *Principia* Halley's interests ranged over a wide field of scientific endeavour, and several chapters must now be devoted to the discussion of the papers he composed during this fruitful period.

Chapter 6

Physics of Earth and Atmosphere—1

1 *Variations of the Compass*

When Halley was still a young man he became interested in a problem which was to tax the ingenuity of his mature years and the pursuit of which eventually involved him in epic adventures. This problem had to do with a property of the magnetic compass; it was of great practical importance in navigation, and it had a history going back several centuries.

Natural magnets, or lodestones, are found in various parts of the world; and the ancients were familiar with their property of attracting or repelling pieces of iron and of converting these into temporary magnets. The directive, or north-and-south-pointing property of a freely-suspended (or floating) magnetic needle seems to have been a Chinese discovery; it passed to the West during the Middle Ages and was soon utilized in the invention of the mariner's compass. It was discovered during the fifteenth century that the magnetic needle does not in general point due north and south but sets itself at a small angle to the astronomical meridian. This angle, called the *variation of the compass* or the magnetic *declination* (we shall use both terms indifferently), varies from place to place; and during the sixteenth century it was measured in many parts of the world by such crude methods as were available to sailors. This was done partly to determine the local corrections to be applied to the indications of the compass in finding the true north wherever the mariner might happen to be. There was also the hope that this magnetic phenomenon might afford a means of determining position at sea. For suppose a curve to be drawn on a map of the world through all places where the variation has a given value; and let this be done for a range of values of the variation. There was a chance that the resulting curves, or *isogonics*, might form a

'family' intersecting the parallels of latitude; and a sailor who wished to fix his position at sea would only have to determine his latitude (found comparatively easily) and the local variation of the compass, and then find where the corresponding parallel and isogonic intersected on the chart. In this way the troublesome problem of determining the longitude would be by-passed. However, the distribution of the magnetic declination over the Earth's surface proved to be too irregular for this plan to succeed. Moreover, it became known by the middle of the seventeenth century, if not earlier, that the amount of the variation is everywhere slowly changing as the years pass.

Other magnetical discoveries of the period related to the phenomenon of *dip*—the inclination to the horizontal assumed by a magnetic needle freely suspended at its centre of gravity. Such studies prompted the suggestion that the magnetic needle behaves as if it were directed towards some 'poynt respective' lying within the Earth, and not, as had been supposed, towards the Pole Star or some legendary northern mountain. Accordingly, in his historic book, *De Magnete* (1600), William Gilbert accounted for terrestrial magnetism by conceiving the Earth as a huge spherical lodestone having two *poles* such as lodestones were known to possess and towards which the ends of the compass needle were directed. He believed these poles to lie at or in line with the geographical poles; and from his experiment of moving a pivoted needle over a globular lodestone he concluded that the variation of the compass was due to local irregularities in the Earth's surface. The needle, he supposed, was deflected towards continents and away from oceans since water was non-magnetic, though local magnetic iron deposits might also play some part in disturbing the needle. Then came the discovery that the variation is everywhere subject to progressive alteration. Great geographical upheavals would have been needed to produce such changes on Gilbert's view, which accordingly gave place to an explanation in line with the fashionable physiography of Descartes. He attributed the deflection of the needle to local accumulations of iron ore, and he supposed that slow changes in the variation arose from the formation, corruption, or transportation of this material.

On an obvious analogy with the above-mentioned isogonics, attempts were made to construct *isoclinic* curves, that is, lines through places where the dips are equal, and to use them for the determination of position just as it had been intended to use the isogonics. This proposal was developed by a London teacher of navigation, Henry Bond, in his *Longitude Found* of 1676, which, though based on mistaken assumptions, contains some interesting anticipations of Halley's ideas. He assigned to the Earth two magnetic poles situated at diametrically opposite points of its surface and distinct from the geographical poles. The dip at any place depended upon the angular distance from the nearer pole, the isoclinics being small circles cutting the parallels of latitude at a constant angle. Bond claimed to be able to represent the secular changes in the magnetic elements (particularly for London) by assuming that the magnetic poles lagged a little behind the Earth in its daily rotation so as to describe circles about the geographical poles. Already in 1674, Robert Hooke had advanced the theory that the magnetic poles revolved in circles of 10° radius about the geographical poles (T. Birch, *History of the Royal Society*, London, 1756, 57, iii, 131).

Knowledge and speculation about terrestrial magnetism had reached this point when Halley first turned his attention to the subject. On his voyage to St Helena he had observed that the magnetic dip vanished when he was 15° north of the equator; and he now began by preparing a synopsis of well-established and dated measurements of the variation at stations of specified longitude and latitude; they cover all the then known parts of the world except the north Pacific area (*Phil. Trans.* (1683), *13*, 208ff.). Among the entries are several of Halley's own determinations:

London (1672), 2° 30′ West;
St Helena (1677), 0° 40′ East;
Paris (1681), 2° 30′ West.

The broad trends revealed by the synopsis supported none of the stock explanations of the magnetic declination. Along the coast of Brazil the needle, contrary to Gilbert's theory, pointed east of north, and therefore away from the land and towards the sea. On the other hand, the deflection of the needle in the same direction over large tracts of the Earth's surface (e.g. throughout the

Indian Ocean) was equally unfavourable to the views of Descartes, though Halley admitted that local accumulations of ore did sometimes affect the needle, for example, at the island of Elba. The attracting material must be very remote to produce these large-scale uniformities; yet a compass was but little affected by the iron guns on the same ship, and some recent experiments had shown how little magnetism there was in most crude iron ore. Then there was the problem of the slow change in the variation with time.

To meet all these difficulties Halley put forward a new hypothesis:

> That the whole Globe of the Earth is one great Magnet, having Four Magnetical poles, or points of attraction, near each pole of the Equator Two; and that, in those parts of the World which lye near adjacent to any one of those Magnetical poles, the Needle is governed thereby, the nearest pole being always predominant over the more remote (215f.).

The pole nearest to us lies in the meridian of Land's End and not more than 7° from the geographical north pole. Arguing from the observed distribution of the magnetic declination Halley suggests approximate locations for these four poles. He conjectures the arrangement of the isogonics in the north Pacific area, and he proposes that, as a test of his theory, his predictions should be compared with the actual measurements which the Spaniards were known to be making there when these should become known. However, before any precise calculations could be made, it would be necessary to secure many more measurements of the magnetic declination *on land* and to ascertain exactly how magnetic force varied with distance from the attracting pole: 'It remains undetermined in what proportion the attractive power decreases, as you remove from the Pole of a Magnet' (220). The secular change in the declination seemed to point to a relative motion of the poles. But this was as far as Halley could go at the moment.

In the years that followed the publication of his paper of 1683, Halley took up again from time to time the problem of the slow changes suffered by the variation of the compass; and he cast about for a reasonable explanation of how the Earth, conceived as a huge lodestone, could possibly have *four* magnetic poles and moving ones at that. The problem seemed insoluble; he became despondent and had given up the quest when one day, 'in accidental discourse, and least expecting it', he stumbled upon a hypothesis which forms the theme of his second notable paper on terrestrial magnetism (*Phil. Trans.* (1692), *17*, 563ff.). It may be noted in passing that Flamsteed, writing to Richard Towneley in 1686, asserted that Halley had borrowed his four-pole hypothesis from Peter Perkins, the Master of the Mathematical School at Christ's Hospital (*The Observatory* (1922), *45*, 280ff.).

Halley argued that the slow, regular changes in the variation exhibited the same trend over too wide an area of the Earth's surface to be explained by the removal of magnetic matter from one place to another. A transference of material so great as would be necessary must have displaced the Earth's centre of gravity and its axis of rotation, altering the limits of land and sea and causing inundations not recorded in history. The motion must be one of *rotation*; and the moving part must have the same centre of gravity as the rest of the globe, and yet be detached from its exterior.

So then the External Parts of the Globe may well be reckoned as the Shell, and the Internal as a Nucleus or inner Globe included within ours, with a fluid medium between. Which having the same common Centre and Axis of diurnal Rotation, may turn about with our Earth each 24 hours; only this outer Sphere having its turbinating Motion some small matter either swifter or slower than the Internal Ball.

Halley, in fact, conceived the Earth as consisting of an outer shell with two magnetic poles, and an inner nucleus, concentric with the shell and possessing two poles of its own. The magnetic axes of shell and nucleus were inclined to each other and to the axis of the Earth's diurnal rotation, about which the two components turned at slightly different rates; this difference gave rise to a slow relative motion of the magnetic poles with a consequent change in the magnetic variation. In the period required for the shell to gain (or lose) one complete rotation on the nucleus, the variation would go through a complete cycle and return everywhere to its initial value. This period might well be a long one, perhaps about 700 years, while determinations of the variation had been in progress for only about a century. The

compass needle was also liable to be upset by local accumulations of magnetic material. Hence any attempt to draw up a numerical theory would be premature. But Halley surmised that the movable poles (those situated on the nucleus) were the one on the meridian of Land's End and the South American one; and he suspected that the nucleus was rotating more slowly than the shell. Future changes in the variation of the compass might be found to follow a more complicated law than could be represented by this hypothesis. In that event it might be necessary to postulate several such concentric shells, each with its own magnetic axis and period of rotation. Halley thought there would be room for spheres comparable in radius to the planets Mercury, Venus, and Mars. Dahl's portrait of Halley in middle life shows him holding in his hand just such a scheme of the Earth's internal constitution. Meanwhile, 'all that we can hope to do is to leave behind us Observations that may be confided in, and to propose Hypotheses which after Ages may examine, amend or confute'. And Halley appealed to ship-masters and travellers to determine the variation of the compass wherever they might happen to be and to communicate it to the Royal Society.

Halley expected opposition to his hypothesis on various grounds. It could be urged that no such physical system was known to occur elsewhere in nature; that the nucleus would bump into the shell; that the sea would leak through into the empty space between shell and nucleus. Or it would be asked, Of what use was the nucleus, 'shut up in eternal Darkness, and therefore unfit for the Production of Animals or Plants'? He replied that Saturn's Ring was an analogous structure; if rotated about one of its diameters it would generate an outer shell which would be kept in its place by the same agency as controls the Ring. Halley supposed that 'should these Globes be adjusted once to the same common Centre, the Gravity of the parts of the Concave [shell] would press equally towards the Centre of the inner Ball'; there would in fact (under the law of the inverse square) be neutral equilibrium and no resultant gravitational attraction between shell and nucleus. As for leakage from the sea bed, doubtless the Creator has provided against that. The nucleus may be of little use to man (Halley continues), but the Earth is one of the planets, and all are habitable, 'though we are not able to define by what sort of Animals'. And as all parts of the creation abound with life, why not suppose our globe capable of other improvements than barely to support life upon its surface?

Why may not we rather suppose that the exceeding small quantity of solid Matter in respect of the fluid Ether, is so disposed by the Almighty Wisdom as to yield as great a Surface for the use of living Creatures as can consist with the conveniency and security of the whole. We our selves in Cities where we are pressed for room, commonly build many Stories one over the other, and thereby accommodate a much greater multitude of inhabitants.

If it be argued that 'without Light there can be no living', there may be many ways of producing light of which we are ignorant. Perhaps there are 'peculiar Luminaries below', such as Aeneas is fabled to have found in the infernal regions.

In support of his hypothetical constitution of the Earth Halley adduced several further ingenious arguments. Perhaps magnetic attraction served as a cement to prevent portions of the inner surface of the shell from falling on to the core. Newton had stated that the Moon was denser than the Earth in the proportion of 9 to 5; this could be explained by supposing four-ninths of the Earth's volume to be empty. Again, it was currently believed that the motion of a celestial body through space was appreciably affected by the resistance of an all-pervading aether. Of two spheres equal in density and in speed, the larger is impeded less than the smaller. (This is because the resistance depends broadly upon the surface presented, or the *square* of the radius, while the mass impeded is proportional to the *cube* of the radius.) Hence the Earth and the Moon would eventually be separated unless the effective density of the Earth were reduced by the introduction of a subterranean cavity.

Halley's hypothesis of four terrestrial magnetic poles received some support from the researches of Christoff Hansteen early in the nineteenth century; but the problem of the Earth's magnetism has proved less tractable than Halley anticipated.

One of the standing problems that exercised the ingenuity of natural philosophers in Halley's day was that of determining the law according to which the force of a magnetic pole varies with distance. The problem was complicated by the invariable

occurrence of magnetic poles *in pairs* exhibiting opposite polarities. There was a strong presumption, based upon analogy with gravitational attraction, that magnetic force would be found subject to the law of the inverse square; and this was eventually confirmed by Coulomb in 1785. Meanwhile, several inconclusive investigations of the problem were carried out, in some of which Halley took part. In 1687 he set up a magnetized needle in the quadrangle of Gresham College and noted the progressive changes in the deflection which it showed as a lodestone was moved up towards it, six inches at a time, from a direction at right angles to the meridian. He thought that better results could have been obtained using a needle balanced between a fixed and a movable lodestone, the attractions being compared on the principle of the triangle of forces (*Correspondence*, 135ff.). Again, it is recorded that, on 20 March 1712, Newton proposed that Halley and Hauksbee should experiment with 'the great lodestone' of the Royal Society to find how its power fell off with increase of distance. Newton believed that the proportion 'would be nearer the cubes than the squares'; and that was the view he expressed in the *Principia* (edition of 1726, Book III, Prop. 6). This is approximately true when the combined effects of both poles of the magnet are taken into account. A week later, Hauksbee alone was ordered to try the experiment, and in due course he reported his results (*Phil. Trans.* (1712), *27*, 506ff.).

2 *The Pneumatics of the Atmosphere*

In 1686 Halley established the mathematical law connecting barometric pressure with height above sea level, assuming the Earth's atmosphere to be of uniform temperature throughout. He went on to estimate the effective extent of this supposedly 'isothermal' atmosphere and to discuss the connection of winds and weather with barometric pressure.

The invention of the barometer may be traced back to the observation, described by Galileo in 1638, that water will not rise in the shaft of a suction pump to a height of more than about 32 feet above the level of its free surface. In 1643 Torricelli conceived the idea of substituting mercury for water, and, with Viviani, he established that the height of the suspended mercury column was shorter than 32 feet in the same proportion as mercury is denser than water. He suspected that the column was counterpoised and supported by the pressure of the air upon the free mercury surface, and he attributed fluctuations in the height of the column to variations in the atmospheric pressure.

Blaise Pascal saw the experiment of the mercury barometer demonstrated in 1646; in the following year he discussed its implications with Descartes, who may have suggested the crucial test of Torricelli's hypothesis carried out, at Pascal's request, by his brother-in-law, Florin Périer, on 19 September 1648. Périer set up a barometer, each time with the same tube and mercury, at various stations on the slopes and on the summit of the Puy-de-Dôme, a high mountain in Auvergne. The mercury showed a progressive fall with increase of altitude in comparison with the indications of a second barometer observed throughout the day at the foot of the mountain, thus confirming the dependence of barometric height upon atmospheric pressure.

Pascal's experiment suggested the construction of a table showing how the barometric pressure varies with the elevation of the place of observation, and intended to serve for determining the heights of mountains and for estimating the extent of the Earth's atmosphere. As the experiment became more widely known, attempts were made in various parts of Europe, notably by George Sinclair in Scotland, to estimate the heights of hills barometrically; but success was impossible without a well-founded theoretical rule connecting barometric pressure with elevation above sea level. The basis for such a rule was provided by Boyle's Law, formulated in 1662, according to which the volume of a given quantity of gas varies inversely as the pressure, the temperature being supposed to remain constant.

Robert Hooke (who had had much to do with establishing Boyle's Law) considered a cylindrical column of the atmosphere extending upward indefinitely; and he supposed it divided into 1,000 layers each containing the same quantity of air by weight (*Micrographia*, London, 1665, 227f.). From the estimated density of the air at ground level he reckoned that, in order to produce the barometric pressure we normally observe, each of these layers must exert the same pressure as a 35-foot layer of air of ground-level density. He then applied Boyle's Law to

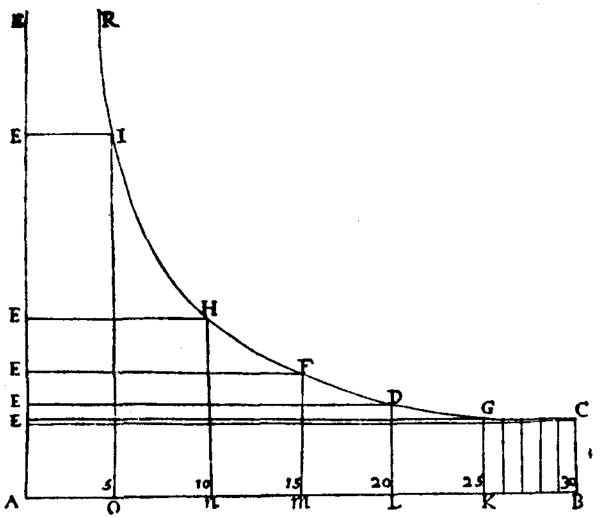
calculate the thickness of each layer from the lowest up to the 999th. Hooke did not attempt to sum the thicknesses, nor did he compute that of the 1,000th layer (restricted only by limits of expansibility which he could not assign), and so his calculation resulted in no definite figure for the extent of the atmosphere. Edmé Mariotte also tackled the problem on somewhat similar lines.

Halley, too, starts out from Boyle's Law and from current estimates of the relative densities of air, water, and mercury (*Phil. Trans.* (1686), *16*, 104ff.). He assumes the density of water to be about 800 times that of air at normal temperature and pressure, and the density of mercury to be about 13½ times that of water. It follows that, if the atmosphere were throughout of the same density as at ground level, a barometric reading of 30 inches (of mercury) would indicate that the atmosphere extended upward for 5·1 miles, each inch of pressure corresponding to an atmospheric layer 900 feet thick. However, the density of the air at any given height depends upon the pressure exerted by the superior layers, and this pressure necessarily diminishes as we ascend, so that the ascents corresponding to successive decreases of pressure of one inch grow steadily greater. Halley considers the pressure of the air in relation not to its density, but to its 'expansion', or degree of rarefaction, which is the reciprocal of density and comparable to what we call the 'specific volume'. Thus the expansion of the air at the normal pressure of 30 inches is taken as 900 feet. If we call the pressure p and the corresponding expansion v, then, by Boyle's Law, pv=constant. But this is analogous to the relation connecting the co-ordinates of a rectangular hyperbola referred to its asymptotes as axes ($xy=a^2$).

Halley applies this analogy to the problem in hand. In his diagram (Fig. 10) the curve CHI is a rectangular hyperbola, and the rectangles ABCE, AKGE, ALDE, . . . are all equal, their lengths being inversely as their breadths. Hence if AB, AK, AL, . . . stand for atmospheric pressures, regularly decreasing, then BC, KG, LD,. . . represent the corresponding expansions on a certain scale,

which expansions being taken infinitely many, and infinitely little, (according to the Method of Indivisibles) their summe will give the spaces of Air between the several heights of the Barometer; that is to say, the summe of all the lines between CB and KG, or the area CBKG, will be proportional to the distance or space intercepted between the Levels of two places in the Air, where the Mercury would stand at the heights represented by the lines AB, AK; so that the spaces of Air answering to equal parts of Mercury in the Barometer are as the areas CBKG, GKLD, DLMF &c.

(The procedure illustrates an early stage in the invention of the calculus, an *area* being conceived to consist of an infinity of consecutive *lines*.)



-Height and Pressure of the Atmosphere

Fig. 10 Height and barometric pressure.

Halley continues:

These Areas again are . . . proportionate to the Logarithms of the numbers expressing the Rationes of AK to AB, of AL to AK, of AM to AL &c. So then by the common Table of Logarithms, the hight of any place in the Atmosphere, having any assigned hight of the Mercury, may most easily be found. For the line CB in the Hyperbola, whereof the Areas design the Tabular Logarithms, being 0·0144765 'twill be, as 0·0144765 to the difference of the Logarithms of 30 and any other lesser Number, so 900 feet or the space answering to an Inch of Mercury, if the Air were equally prest with 30 Inches of Mercury and every where

alike, to the hight of the Barometer in the Air, where it will stand at that lesser Number of Inches,

and conversely. Halley appends two tables showing (1) the atmospheric pressure at any given height above the Earth, and (2) the height at which the pressure should have a given value.

Halley's calculation is equivalent to the modern practice of taking a vertical column of the atmosphere, of unit cross-section (Fig. 11), and considering an element of thickness dh at a level where the pressure is p, the density of the air ρ, and the specific volume v.

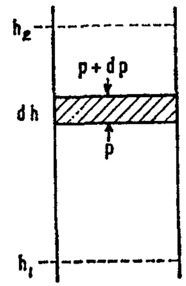


Fig. 11 Calculating atmospheric pressure.

If the increment of pressure over

$$\text{dh is dp, then } dp = -g\rho dh = -\frac{g\,dh}{v}$$

(where g is the acceleration of gravity).

$$\therefore \int_1^2 v\,dp = -\int_1^2 g\,dh = g(h_1 - h_2) \text{ ———————(1)}$$

The integral on the left is the area under the curve of pv = constant, taken between the pressures p_1 and p_2. It is thus shown to be proportional to the difference between the heights at which the pressure has these values. Again, if pv = 1,

$$\int_1^2 v\,dp = \int_1^2 \frac{dp}{p} = \log p_2 - \log p_1 = \log \frac{p_2}{p_1} \text{ ——————(2)}$$

The same areas under the curve thus represent (1) differences between two altitudes, and (2) the natural logarithms of the ratios of the pressures at the different altitudes. Halley derives his rule connecting atmospheric pressure with altitude by establishing a proportion between the dimensions of the curve and the physical quantities they represent. Since pv=1 and AB=30, therefore CB=$\frac{1}{30}$. But CB represents an 'expansion' of 900 feet. Hence, if $h(x)$ represents the height at which the barometric pressure equals x inches,

$$\frac{900}{\frac{1}{30}}=\frac{h(x)}{\log \frac{30}{x}}$$

or, changing over to common logarithms,

$$h(x)=\frac{900\ (\log_{10} 30-\log_{10} x)}{0{\cdot}0144765}$$

the denominator involving the logarithmic modulus.

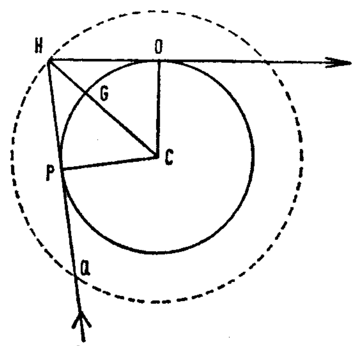


Fig. 12 Finding the height of the atmosphere by Alhazen's method.

It appeared from the tables that, at a height of 41 miles, the air expanded to 3,000 times its volume under normal pressure, and at 53 miles, to 30,000 times that volume. But probably the utmost 'spring' of the air could not exert itself to so great an extension; and Halley estimated the height of the atmosphere as about 45 miles. This estimate could be confirmed by an independent method derived from the Muslim physicist Alhazen (*c.* A.D. 1000) and based upon the observation that twilight begins and ends when the Sun is about 18° below the horizon.

The argument was that twilight continued so long as the Sun illuminated any portion of the atmosphere not concealed from the observer by the curvature of the Earth. Let C be the centre of the Earth and of its atmospheric layer (Fig. 12). Let the

observer be at O and let P be a place where the Sun is setting when twilight ends at O. Then the angle OCP = 18°.

$$\frac{\text{Height of atmosphere}}{\text{Earth's radius}}=\frac{\text{GH}}{\text{CG}}=\frac{\text{CH}-\text{CG}}{\text{CG}}=(\sec 9^\circ-1).$$

However, the rays QPH and HO each suffer maximum refraction (about 32′), and the estimated extent of the atmosphere is thereby reduced, the ratio becoming sec $8\frac{1}{2}°-1$, which gives the height of the atmosphere as about 45 miles. It was thus confirmed that air could be expanded to some 3,000 times its volume under normal atmospheric pressure, and (as experiment showed) compressed to one-sixtieth part of this; and Halley wondered how air was capable of assuming this great range of volumes. It seemed scarcely appropriate to liken its texture to that of wool, or of some other springy material, as Boyle had done.

In drawing up his tables of atmospheric heights and pressures, Halley had ignored inconstancies in the barometric reading at ground level. But he was aware that the pressure could indeed fluctuate to the extent of about one-fifteenth of its normal value; this he attributed to changes in air temperature and in the quantities of 'effluvia and steams' suspended in the atmosphere which increase its gravity like salts dissolved in water. Halley was aware also that Boyle's Law held good only within certain limits; it must break down for air compressed to the density of water (which, as was supposed, 'yeilds not to any force whatsoever') or expanded to a bulk which it cannot exceed even under *no* pressure. He thus looked forward to the more elaborate relations that have supplemented Boyle's Law. Halley also speculated in the crude fashion of his time as to the connection between weather and barometric indications. Two contrary winds blowing away from the place of observation lower the specific gravity of the air; the barometer falls, and the vapours, no longer supported, coalesce and descend as rain. Two contrary winds blowing towards the observer raise the barometric reading and maintain dry weather.

The familiar type of barometer was found unsuitable for use at sea, the motion of the ship causing the column of mercury to oscillate in its upright tube. However, towards the end of his life, Robert Hooke devised a special form of the instrument designed to be of service to navigators. And as Hooke was prevented by illness from demonstrating his invention, the Royal Society invited Halley to give them an account of it (*Phil. Trans.* (1701), *22*, 791ff.). A spirit thermometer and an air thermometer were set up side by side. The former was not affected by variations in the atmospheric pressure; but the air thermometer was sensitive to such changes as well as to fluctuations in the temperature. Hooke graduated the air thermometer to agree with the indications of the spirit thermometer at a certain known atmospheric pressure. At any other pressure the indications on the two scales differed by an amount which showed by how much the existing atmospheric pressure exceeded or fell short of that prevailing when the instrument was graduated. Halley stated that he had taken one of these devices with him on his Atlantic voyage and that 'it never failed to prognostick and give early notice of all the bad weather we had'. The instruments, it was added, were being made to Hooke's specifications by Henry Hunt, Operator to the Royal Society, from whom they could be obtained.

In a later paper on the barometric measurement of heights Halley enumerates the inconveniences of the various types of instrument available for the purpose, and he describes a new device free from these drawbacks (*Phil. Trans.* (1720), *31*, 116ff.). The primitive barometer of Torricelli had been modified in various ways so as to render the variation of the atmospheric pressure more evident. There was Robert Hooke's wheel barometer in which the tube was turned up at the lower end so as to expose a free mercury surface on which rested a float. As the mercury rose and fell, the float operated a pulley and moved a pointer over a dial to indicate the changes of atmospheric pressure. This instrument, however, was not easily portable to the top of a mountain. There were other types of barometer which employed, in conjunction with mercury, other liquids of lower specific gravity, such as alcohol, serving to magnify any changes in the pressure; but these instruments were fragile and their indications were upset by changes of temperature affecting the densities of the liquids. Then there was Hooke's marine barometer, just referred to. But this instrument required for its construction tubes of uniform bore such as could rarely be found, and it operated on the assumption that air and spirit suffered thermal expansion in equal proportions, which lacked confirmation.

The instrument which Halley now had under consideration he attributed to one Patrick, who seems to have made something of a hobby of barometers and who called himself the 'Torricellian Operator'; a rather similar device had, however, already been described by Amontons. It consisted of a glass 'cane', or tube, about five feet long, closed at one end and tapering slightly towards the closed end; this tube was filled with mercury and held with its open end downward when so much mercury would fall away as to leave a thread equal in length to the height of the common barometer. Any increase in the atmospheric pressure forced the mercury towards the narrower end of the tube until the resultant lengthening of the thread restored equilibrium, while a lowering of atmospheric pressure worked the opposite way. The instrument was very sensitive to slight changes of pressure: an ascent of 90 feet, corresponding to a diminution of one-tenth of an inch in atmospheric pressure, would displace the mercury thread through several inches. It could be graduated by taking it up a monument or some such eminence and marking the level of the mercury surface for every ten feet of ascent. A pair of such instruments could serve to measure the difference of level of two places too far apart for ordinary surveying methods to be applicable. Supposing the places to be 20 miles apart, a levelling error of even as little as one minute of arc would affect the estimated height by as much as 30 feet, while Patrick's barometer would give a much closer approximation.

3 *A Survey of Winds*

From his speculations on the inter-connections of wind, weather, and barometric pressure, Halley passed on to survey and chart the broad distribution of prevailing winds over the oceans of the world (*Phil. Trans.* (1686), *16*, 153ff.). His only serious forerunner in this field was the German geographer Varenius in his treatise of 1650, certain errors in which Halley was anxious to correct.

The land masses constitute islands in a great hydrosphere which they divide into three main oceans, the Atlantic, the

Indian, and the Pacific. Halley's account of the Atlantic wind system is the fullest: his own experience had familiarized him with the essential features of the tropical zone, the equatorial calms and the north-easterly and south-easterly trade-winds, which suffer seasonal displacements northward or southward as the Sun passes from one celestial hemisphere to the other. He had less to say about the Indian Ocean, while the Pacific was still largely a preserve of the Spanish navigators whose vague reports afforded the only information as to the winds prevailing there. Halley's paper was illustrated by a world chart of the winds, the earliest meteorological chart to be published (Fig. 13). The direction of the strokes everywhere indicates the course of the wind, the sharp end pointing to that part from which the wind

Fig. 13 Halley's chart of the winds.

comes. Monsoons are indicated by rows of strokes alternately pointing in opposite directions.

Halley's paper of 1686 marked a distinct step towards the understanding of the mechanics of trade-winds and monsoons. Francis Bacon in 1622 had connected the tropical *briza* with the expansion of the air caused by the Sun's heat. Galileo in 1632 suggested that the Earth's eastward rotation produced the impression of a continual east wind in the tropical oceans, where the Earth's motion is swiftest and where there are few surface irregularities to carry the lower layers of the atmosphere along in the diurnal rotation. Varenius (1650) thought the air in the tropical belt was rarefied and thrust along westward by the Sun in its daily course. In a lecture given in 1686, the year of Halley's paper, Robert Hooke connected the trade-winds with the Earth's rotation which, he supposed, rendered the atmosphere high and rarefied about the equator, and low and condensed at the poles,

Plate 5 The exterior of the Royal Observatory in Flamsteed's time (*Science Museum, London*)

Plate 6 The interior of the Royal Observatory in Flamsteed's time (*Science Museum, London*)

thus causing air to flow at ground level from the poles towards the equator and to circulate thence back to the poles in high-flowing currents.

Halley attributes the trade-winds to

> the Action of the Suns Beams upon the Air and Water, as he passes every day over the Oceans, considered together with the Nature of the Soyl and Scituation of the adjoyning Continents: I say therefore, first that according to the Laws of Staticks, the Air, which is less rarified or expanded by heat, and consequently more ponderous, must have a Motion towards those parts thereof, which are more rarified, and less ponderous, to bring it to an Aequilibrium; and secondly, that the presence of the Sun continually shifting to the Westwards, that part towards which the Air tends, by reason of the Rarification made by his greatest Meridian Heat, is with him carried Westward, and consequently the tendency of the whole Body of the lower Air is that way. Thus a general Easterly Wind is formed, which being impressed upon all the Air of a vast Ocean, the parts impel one the other, and so keep moving till the next return of the Sun, whereby so much of the Motion as was lost, is again restored, and thus the Easterly wind is made perpetual.

If (to employ modern terms) we understand by the *subsolar point* the point on the Earth's surface where the Sun is vertically overhead, and by the *heat equator* the parallel of latitude described by the subsolar point in the course of a day, then Halley conceives a trade-wind as the resultant of two motions of the air: a convectional movement of the lower layers of the atmosphere towards the heat equator, and a westward flow of air along the heat equator following the subsolar point. Conceiving the matter in this way, he was able to explain why the trade-winds are generally inclined both to the parallels of latitude and to the meridians. Irregularities in the distribution of the trade-winds could be explained for the most part by the interposition of land masses.

Halley also had something to say about monsoons, which he seems to have regarded as seasonal modifications of the trade-winds rather than as convectional effects brought about by the heating and cooling of the continents. He recognized that every wind forms part of a complete circulation of air:

> The North-East Trade-Wind below, will be attended with a South-

Westerly above, and the South-Easterly with a North-West Wind above. . . . To the Northward of the Indian Ocean there is every where Land within the usual limit [of the trade-wind] of the Latitude of 30°, viz. Arabia, Persia, India, etc., which . . . are subject to unsufferable heats when the Sun is to the North, passing nearly Vertical; but yet are temperate enough when the Sun is removed towards the other Tropick; because of a ridg of Mountains at some distance within the Land, said to be frequently in Winter covered with Snow, over which the Air, as it passes, must needs be much chilled. Hence it comes to pass, that the Air coming according to the general Rule, out of the N.E. in the Indian Seas, is sometimes hotter, sometimes colder, than that by which by this Circulation is returned out of the S.W. and by Consequence, sometimes the under Current or Wind is from the N.E., sometimes from the S.W.

Halley concluded his survey of the wind distribution with one of his many appeals for the co-operation of observers: 'It is not the work of one, nor of few, but of a multitude of Observers, to bring together the Experience requisite to compose a perfect and compleat History of these Winds.'

It was in 1735 and therefore during Halley's lifetime that George Hadley explained the deflection both of the trade-winds and of the westerly winds in temperate latitudes on kinematic principles:

The Air, as it moves from the Tropicks towards the Equator, having a less Velocity than the Parts of the Earth it arrives at, will have a relative Motion contrary to that of the diurnal Motion of the Earth in those Parts, which being combined with the Motion towards the Equator, a north-east Wind will be produced on this Side of the Equator, and a south-east on the other (*Phil. Trans.* (1735), *39*, 58ff.).

Chapter 7

Physics of Earth and Atmosphere—2

1 Studies on Evaporation

Halley carried out a series of investigations on the evaporation of water, particularly as it affects the economy of rivers, lakes, and seas; and although his papers on the subject appeared at intervals extending over nearly thirty years, it may be convenient to consider them as a group.

The earliest paper in the series describes an experiment devised to measure the rate at which water evaporates when the air is at summer heat, the numerical result affording a means of estimating the quantities involved in the aqueous circulation of the Mediterranean system, including the Black Sea (*Phil. Trans.* (1687), *16*, 366ff.). Halley took a pan of salt water with a thermometer inserted, attached it to one end of the beam of a balance, counterpoised it with weights and, by the occasional application of a coal fire, kept the water at a full summer heat. Observing the loss of weight through evaporation, he estimated that in 12 hours a quantity of vapour would be formed sufficient to lower the surface of the water by a tenth of an inch. Assuming the Mediterranean to be maintained at this summer heat for 12 hours a day, and supposing evaporation during the night to be compensated by the dewfall, it appeared that the daily loss in vapour through heating must amount to some 5,280 million tons, over and above what drying winds might carry away. This loss must in part be made good by inflowing rivers, of which Halley specified nine, each supposed capable of supplying ten times as much water as the Thames. Observations of the breadth, depth, and rate of flow of the Thames at Kingston Bridge indicated a daily output of 20,300,000 tons; and from this it followed that the Mediterranean tributaries supplied little more than a third of what the Sun raised in vapour. The balance must be contributed by the constant current flowing in at Gibraltar. What actually became of the vapours (more than sufficient to maintain the rains, dews, and springs of the Mediterranean lands) was reserved for a 'further Entertainment'. Halley's procedure is reminiscent of that adopted a few years earlier by Pierre Perrault and by Edmé Mariotte, who had independently compared the output of the Seine with the rainfall in its basin in support of their contention that rain would be more than sufficient to keep springs and rivers flowing.

Continuing his studies on evaporation, Halley discussed the 'circulation of vapours', the process by which the water raised by the Sun's heat from the sea finds its way back thither (*Phil. Trans.* (1691), *17*, 468ff.). The paper seems to embody material presented to the Society in 1688-90 (*Correspondence*, 140, 212, 217f.). There was first the problem of explaining how, considering that water is some 800 times as dense as air, it could ascend into the atmosphere at all. Halley surmounted this difficulty by conceiving a particle of water as capable of inflation into a sort of bubble under the heat of the Sun: 'If an Atom of Water were expanded into a Shell or Bubble so as to be ten times as big in Diameter as when it was Water, such an Atom would become specifically lighter than Air'. This may not be the only mechanism involved; there may be 'a certain sort of matter whose conatus may be contrary to that of Gravity: as is evident in Vegetation'. However, we know that heat does raise vapour; and

let us for a first supposition put . . . that the whole Body of the Earth were Water, and that the Sun had his diurnal Course about it: I take it, that it would follow that the Air of it self would imbibe a certain quantity of Aqueous Vapours and retain them like Salts dissolved in Water; that the Sun warming the Air and raising a more plentiful Vapour from the Water in the day time, the Air would sustain a greater proportion of Vapour, as warm Water will hold more dissolved Salts, which upon the absence of the Sun in the Nights would be all again discharged in Dews, Analogous to the precipitation of Salts on the cooling of the Liquors.

He continues:

Next, Let us suppose this Ocean interspersed with wide and spacious Tracts of Land with high ridges of Mountains . . . on the tops of which the Air is so cool and rarified as to retain but a small part of those Vapours that shall be brought thither by the Winds. Those Vapours therefore that are raised copiously in the Sea, and by the Winds are carried over the low Land to those Ridges of Mountains, are there compelled by the stream of the Air to mount up with it to the tops of the Mountains where the water presently precipitated, gleeting down by the Crannies of the Stone; and part of the Vapour entering into the Caverns of the Hills, the Water thereof gathers as in an Alembick into the Basons of stone it finds, which being once filled, all the overplus of Water that comes thither runs over by the lowest place, and breaking out by the sides of the Hills, forms single Springs.

These unite their streams so as eventually to form great rivers which bear the water back to the sea. The precipitation of vapours on high hills was not a 'bare Hypothesis': Halley had had an unwelcome experience of it at St Helena, as he proceeds to describe in a passage we have quoted in Chapter 3.

However, there were other ways in which the evaporated sea water returned to the ocean. A portion of the vapour

by the cool of the Night falls in Dews, or else in Rains, again into the Sea before it reaches the Land, which is by much the greater part of the whole Vapour, because of the great extent of the Ocean. . . . A third part falls on the lowe Lands, and is the Pabulum of Plants, where yet it does not rest, but is again exhaled in Vapour by the Action of the Sun. . . . Add to this that the Rain-waters, after the Earth is fully sated with moisture, does by the Valleys or lower Parts of the Earth find its way into the Rivers, and so is compendiously sent back to the Sea.

Edmé Mariotte had indeed contended that rainfall is sufficient to maintain the flow of rivers; there was also a theory that springs are fed by sea water percolating into the earth through pores and losing its saltness in the process. Halley, however, argued that rain is intermittent while many of the springs are perpetual; and many great rivers have their most copious sources farthest from the sea.

This, if we may allow final Causes, seems to be the design of the Hills, that their Ridges being placed through the midst of the Continents, might serve as it were for Alembicks to distil fresh water for the use of Man and Beast, and their heights to give a descent to those Streams to run gently, like so many Veins of the Macrocosm, to be the more beneficial to the Creation.

Halley concludes his paper with some speculations on the difference between rain and dew. When two contrary winds meet, the air is heaped up, the barometer is high, and the vapour is prevented from coagulating into drops; there are no clouds and at night the vapour falls imperceptibly as single atoms of water. But when two winds diverge, the air is rarefied and unable to prevent the vapours from coalescing into raindrops. 'To which 'tis possible and not improbable, that some sort of Saline or Angular Particles of Terrestrial Vapour being immixed with the Aqueous, which I take to be Bubbles, may cut or break their Skins or Coats, and so contribute to their more speedy Condensation into Rain'.

Halley wondered how much evaporation from a water surface was produced solely by the internal warmth of the water, and how important was the part played by Sun and Wind. In 1693 he arranged for Henry Hunt, the 'Operator' of the Royal Society, to measure at Gresham College the daily weight of water evaporated at room temperature from a screened and shaded liquid surface (*Phil. Trans.* (1694), *18*, 183ff.). It was found that the evaporation in one year was equivalent to the loss of a layer of water 8 inches thick. This was insufficient to account for the observed rainfall, which ranged from 19 inches in France to 40 inches in Lancashire. 'Whence it is very obvious, that the Sun and Wind are much more the causes of Evaporation, than any internal heat, or agitation of the Water.' Evaporation seemed to be impeded by a layer of vapour which clung to the water surface; perhaps it was this which produced, by a refraction of light, the curious exaggeration in the apparent heights of objects on shore when these were viewed from out at sea, a phenomenon known as 'looming'. This might also explain why cattle on the Isle of Dogs could be seen from Greenwich at high tide though invisible at low tide.

2 *The Deluge*

From these studies on evaporation and the circulation of water over the Earth's surface, it seems appropriate to pass to Halley's considerations as to what might have been the natural causes of the universal Flood recorded in the seventh chapter of *Genesis*

(*Phil. Trans.* (1724), *33*, 118ff.). This 'Noachian Deluge' had long afforded a stock theme for discussion to the naturalists who were groping their way towards a science of geology and who frequently invoked it to explain the occurrence of sea shells on land at places far above sea level. Halley's paper was read towards the end of 1694; but it remained unpublished for some thirty years as he feared that 'by some unguarded Expression he might incur the Censure of the Sacred Order'. His speculations thus preceded William Whiston's *New Theory of the Earth* (London, 1696), where somewhat similar views are expressed.

Halley was particularly impressed by the precise detail of the Scriptural account of the Deluge. The story, he thought, seemed to have been based upon more careful records than would normally be available for such events, remote even in the time of Moses. On the other hand, they seemed 'much too imperfect to be the Result of a full Revelation from the Author of this dreadful Execution upon Mankind'. He thought the information must have formed part of a fuller account derived from Noah and his sons and transmitted by the Patriarchs to their posterity. The story presented a few difficulties as to 'the Reception and Agreement of the Animals among themselves', and as to the construction of the Ark and its preservation on the face of the waters, lashed by the drying wind.

> This we may, however, be fully assured of, that such a Deluge has been; and by the many Signs of marine Bodies found far from and above the Sea, 'tis evident, that those Parts have been once under Water: or, either that the Sea has risen to them, or they have been raised from the Sea; to explicate either of which is a Matter of no small difficulty, nor does the sacred Scripture afford any Light thereto.

The forty days of rain can have made only a small contribution to the Deluge; for even if the *daily* rainfall over the globe had been as great as the *yearly* rainfall in one of the rainiest counties of England (40 inches), the depth of water (neglecting evaporation) would still have amounted to only 22 fathoms, which would barely suffice to cover the low lands near the sea coasts. However, the biblical narrative suggests, besides rain, 'an extraordinary fall of Waters from the Heavens . . . in one great Body', and an inrush of the ocean, possibly caused, so Halley conjectures, by 'the casual Choc of a Comet'. Robert Hooke had already suggested that the Earth's axis of rotation might have shifted *within the body of the Earth* in the course of ages, with a corresponding alteration in the position of the equator, where the ocean bulges out spheroidally in consequence of the rotation. Areas formerly under the sea might thus become dry land; and this would explain the occurrence of marine shells on the tops of hills. A comparison between the respective meridian altitudes of the solstitial Sun at Nuremberg in 1487 and 1686, and the general consensus of observers through the ages, convinced Halley that no significant gradual change of latitudes was occurring (*Phil. Trans.* (1687), *16*, 403ff.). But an encounter with a comet might explain 'all those strange Appearances of heaping vast Quantities of Earth and high Cliffs upon Beds of Shells, which once were the Bottom of the Sea,' and of producing an alternate flux and reflux of the ocean which would make it 'much more difficult to shew how Noah and the Animals should be preserved, than that all things in which was the Breath of Life, should hereby be destroyed. . . . That some such thing has happened may be guessed, for that the Earth seems as if it were new made out of the Ruins of an old World, wherein appear such Animal Bodies as were before the Deluge'. A change in the situation of the polar axis could explain the extreme cold about Hudson's Bay: perhaps that part of the world was once more northerly than it now is, and its former covering of ice is not yet completely thawed. However, Halley expressed second thoughts on this matter in a supplementary paper (ibid., 123ff.). He had been advised that the convulsion of nature he had envisaged should be referred, not to the Deluge, but to a period before the creation of man, when perhaps a former world was reduced to chaos.

3 *The Age of the Earth*

The last of Halley's studies on evaporation, appearing in 1715, bore especial reference to the economy of lakes possessing no outlets. It is, however, of wider interest as containing his proposal to estimate the age of the Earth by determining the growth, through the centuries, of the salinity of such lakes or of the ocean itself (*Phil. Trans.* (1715), *29*, 296ff.).

Halley could remind his readers that various attempts had already been made to deduce the age of the Earth from natural appearances. The Scriptures, indeed, had been interpreted to imply that man had dwelt on the Earth for six or seven thousand years.

> But whereas we are there told that the Formation of Man was the last Act of the Creator, 'tis no where revealed in Scripture how long the Earth had existed before this last Creation, nor how long those five Days that preceded it may be to be accounted; . . . Nor can it well be conceived how those Days should be to be understood of natural Days, since they are mentioned as Measures of Time before the Creation of the Sun, which was not till the Fourth Day. And 'tis certain Adam found the Earth, at his first Production, fully replenished with all sorts of other Animals.

A new approach to the problem was suggested by the observation that all lakes strictly so called (having no outlet) are salt in some degree. The 'lakes' which Halley believed to answer this description were the Caspian Sea, the Dead Sea, the lake on which Mexico City stands and Lake Titicaca in Peru. But even the ocean 'may also be esteemed a Lake; since by that term I mean such standing Waters as perpetually receive Rivers running into them, and have no Exite, or Evacuation'. Halley clearly grasped the principle that the waters of such a lake must swell until the loss by evaporation from the enlarged surface just balances the inflow supplied by tributary rivers. He was also aware that the rivers' supply dissolved salts while the vapour given off by the lake consists of pure water, and that therefore the salts accumulate, steadily increasing the salinity of the lake:

> Now I conceive that as all these Lakes do receive Rivers and have no Exite or Discharge, so 'twill be necessary that their Waters rise and cover the Land, until such time as their Surfaces are sufficiently extended, so as to exhale in Vapour that Water that is poured in by the Rivers; and consequently that Lakes must be bigger or lesser according to the Quantity of the fresh they receive. But the Vapours thus exhaled are perfectly fresh, so that the Saline Particles that are brought in by the Rivers remain behind, whilst the fresh evaporates; and hence 'tis evident that the Salt in the Lakes will be continually augmented,

and the Water grow salter and salter. . . . Now if this be the true Reason of the Saltness of these Lakes, 'tis not improbable but that the Ocean it self is become salt from the same Cause, and we are thereby furnished with an Argument for estimating the Duration of all things, from an Observation of the increment of Saltness in their Waters.

A procedure is thus suggested: determine the quantity of salt in a certain weight of water taken from, say, the Caspian at a certain place and season; repeat the observation after several centuries, and if the salinity be found to have increased, 'we may by the Rule of Proportion, take an estimate of the whole time wherein the Water would acquire the Degree of Saltness we at present find in it'. The argument would be the more conclusive if a similar increase in salinity were found in the ocean. 'And if upon repeating the Experiment, after another equal Number of Ages, it shall be found that the Saltness is further encreased with the same Increment as before, then what is now proposed as Hypotheticall would appear little less than Demonstrative.' It could have been wished 'that the ancient Greek and Latin Authors had delivered down to us the degree of the Saltness of the Sea, as it was about 2000 Years ago'. However, for the sake of future generations, 'I recommend it . . . to the Society, as opportunity shall offer, to procure the Experiments to be made of the present degree of Saltness of the Ocean, and of as many of these Lakes as can be come at, that they may stand upon Record for the benefit of future Ages'. It was generally believed in Halley's day that the sea owed its salinity to salt rocks in its bed, the rivers serving to *freshen* the water. And Halley would not deny that the lakes might have contained *some* salt when they were first formed, which would upset the calculation. However, he observed 'that such a Supposition would by so much contract the Age of the World, within the Date to be derived from the foregoing argument, which is chiefly intended to refute the ancient Notion, some have of late entertained, of the Eternity of all Things; though perhaps by it the World may be found much older than many have hitherto imagined'.

An investigation of the age of the Earth along the lines suggested by Halley was set on foot some seventy years ago by John Joly, utilizing statistics that had become available. But the obvious weakness of the method is the dubious assumption that the rivers have in fact maintained a uniform rate of transfer of salt to the ocean throughout geological time.

4 *The Distribution of Solar Heat*

Early in 1690 a discussion arose in the Royal Society as to how much of the warmth of the air was due to the direct action of the Sun's beams (*Correspondence*, 218). Halley went into the question, working on the assumption that, if we neglect atmospheric absorption, any area of the Earth's surface exposed to the Sun receives heat at a rate proportional to the sine of the Sun's elevation above the horizon at the time of the observation (*Phil. Trans.* (1693), *17*, 878ff.). At any place other than the poles, the Sun's elevation changes continuously throughout the day; and to find the total heating effect of the solar rays there, it is necessary to take what we should call a time-integral of the sines of the Sun's altitude from rising to setting. Or, as Halley expresses it, 'The time of the continuance of the Sun's shining being taken for a Basis, and the Sines of the Sun's Altitudes erected thereon as Perpendiculars, and a Curve drawn through the Extremities of those Perpendiculars, the Area comprehended shall be proportionate to the Collection of the Heat of all the Beams of the Sun in that space of time'. A rough graph suffices to show that this area is greater for the north pole at the summer solstice (when the Sun maintains a constant elevation of about $23\frac{1}{2}°$ for 24 hours a day and the graph is a straight line) than it is for the equator at an equinox (when the graph is a sine curve and the Sun's elevation exceeds $23\frac{1}{2}°$ for only 8 hours 52 minutes out of the 24 hours). Hence if the Sun's elevation were the only factor involved, it would be hotter at the north pole at the summer solstice than on the equator at the equinox, when the Sun passes vertically overhead.

That the facts are far otherwise Halley attributes partly to the long polar night and partly to the obstruction suffered by the Sun's beams in reaching the polar regions through 'thick Clouds, and perpetual Foggs and Mists, and by that Atmosphere of Cold . . . proceeding from the everlasting Ice'. Halley seems to have overlooked the effect of the sheer thickness of air through which the Sun's rays have to pass to reach the polar regions. He suspected that regional differences of temperature might also depend upon the local configuration of the land and upon the nature of the soil, which varies in its capacity to retain heat.

Halley went on to calculate and to tabulate the proportion of solar heat received in one day at the equinox or at the solstices for every tenth degree of latitude, reducing the problem to that of finding the area of a certain cylindrical surface. 'And if the like could be perform'd for Cold, which is something else than the bare Absence of the Sun, as appears by many instances, we might hope to bring what relates to this part of Meteorology to a perfect Theory'.

5 *Rainbows and Parhelia*

Several of Halley's papers relate to the rainbow or to kindred phenomena of atmospheric optics. On the evening of 6 August 1697, while he was living at Chester, he went for a walk on the city walls and, being caught in a sudden shower, took shelter in a recess. He observed the primary and secondary rainbows, formed in the normal manner with their colours in opposite orders; but he was struck by the appearance of a third rainbow which rose from about the places where the primary bow met the horizon and crossed the interspace between the bows to overlap the middle portion of the secondary. This 'Extraordinary Iris' exhibited the same order of colours as the primary bow; and where it coincided with the secondary the coloration disappeared giving an arc of white light. The phenomenon remained visible for about twenty minutes; and Halley concluded that it was produced by the light of the Sun reflected from the estuary of the river Dee. He quoted a reference to the formation of rainbows by reflected light which occurs in the *Météores* of Descartes; but he believed that this phenomenon had never previously been observed. However, it is now recognized that rainbows are produced from time to time in this manner, the centre of the bow being the point diametrically opposite to the image of the Sun formed by the reflecting surface.[1]

In concluding his account of this spectacle, Halley recalls how, several months before, he had been walking down Abchurch Lane in the City of London during a rainstorm, with the Sun behind him, when he saw a rainbow spanning the street (barely five yards wide) like an arch through which he was to pass but which receded as fast as he approached it. He fancied that Iris, the messenger of the gods, had descended 'to invite me to inquire further into the Nature of her Production'; and he promised to communicate the results of his investigation in the next *Transactions*. His paper was, however, delayed until 1700, owing to his absence at sea.

Considerable progress had already been made in understanding the physical nature of the rainbow. It had become generally recognized by the beginning of the seventeenth century that rainbows are formed by light which has suffered internal reflection in raindrops (as explained in textbooks of optics), the order of the rainbow (primary, secondary, and so on) depending upon the number of reflections which the typical ray undergoes before emerging from the drop. Descartes confirmed this rule by passing a ray of sunlight through a glass globe filled with water, and observing the angular distances from the Sun at which the several prismatic colours were formed (*Les Météores*, Discours VIII). He formulated the sine law of refraction, traced the path of a ray through a raindrop, and tabulated the total angular deviation suffered by the ray against its angle of incidence upon the surface of the drop. He found that, for a certain angle of incidence, the value of the deviation passed through a turning-point (a minimum), and that all rays incident at about that angle emerged as a sensibly parallel beam. The concentration of such beams, coming from innumerable raindrops, appreciably affects the eye of a suitably situated observer, who perceives in the sky a bright circular arc having its centre at the point diametrically opposite to the Sun. Descartes carried out the calculation for both the primary and the secondary rainbows. The angle of incidence being i and the corresponding angle of refraction r, he showed that the primary bow is produced by rays for which $4r-2i$ has its maximum value, which is, indeed, the angular radius of the bow. For a bow of order n, the radius is the maximum of $2(n+1)r-2i$, the centre being taken as the Sun when n is even and as the point diametrically opposite to the Sun when n is odd. This explanation of the rainbow remains one of the

[1] *Phil. Trans.* (1698), 193ff.; J. M. Pernter and F. M. Exner, *Meteorologische Optik*, Wien und Leipzig, 1910, 554ff.

greatest achievements of Descartes, though his mechanical theory of the formation of the colours was soon to be superseded by that of Newton.

In his paper of 1700, Halley set himself the task of determining the critical angles of incidence and the radii of the bows by a more direct procedure; he saw in the problem with which Descartes had earlier wrestled a field for the fruitful application of the Newtonian method of fluxions (*Phil. Trans.* (1700), *22*, 714ff.).

In the modern language of the calculus, the above expression $4r-2i$ is a maximum when its derivative is zero: this gives $di=2dr$. As Halley puts it, the 'augmentum momentaneum' (the moment) of i is exactly double that of r. The further development depends upon the lemma that, if in a triangle PQR (Fig. 14) the vertical angle P acquires a small increment while the sides PQ, PR remain unchanged, then the resulting moment of the base angle Q is to that of R as SR to SQ, where PS is perpendicular to QR (produced).

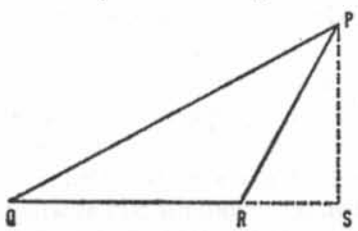


Fig. 14 Lemma on the rainbow.

Now draw a straight line AB of arbitrary length (Fig. 15). Bisect it in C and divide it in D so that (AB:AD) equals the refractive

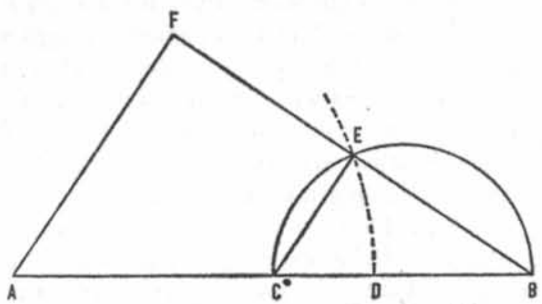


Fig. 15 Finding the angular radius of a rainbow.

index of water. With diameter BC draw a semicircle BEC, and with centre A and radius AD draw the arc DE cutting the semicircle in E. Join AE, BE, CE; and draw AF perpendicular to BE produced. Halley uses the lemma to prove that the angles AEF,

ABE are, respectively, the angles of incidence and refraction involved in the formation of the primary bow. In the similar triangles BAF, BCE, BA=2CA and BF=2EF, therefore, by the lemma, the moment of the angle AEF is twice that of the angle ABE. But by construction,

$$(\sin AEF : \sin ABE) = \frac{AF}{AE} \Big/ \frac{AF}{AB} = \frac{AB}{AE} = \frac{AB}{AD}$$
$$= \text{refractive index of water.}$$

Hence AEF, ABE are the angles sought: their sines and their moments satisfy the required conditions. Subtracting twice the angle of incidence from four times the angle of refraction, we obtain the angular radius of the primary rainbow. To construct the radius of a bow of higher order, C must be situated so that (BC:CA) equals the number of reflections suffered by the incident rays in forming that bow. (In the figure, corresponding to the primary bow, this ratio is unity.) Halley shows how, on the principle of this construction, the required angles may be obtained analytically. He also refers to the explanation of the colours of the rainbow which would be forthcoming should Newton ever publish his book on light and colours (the *Opticks*, destined to appear in 1704): owing to differences of refractive index, each coloured constituent of the sunlight forms its own bow partially overlapping the adjacent ones.

As a convenient experimental method of finding the refractive index of a transparent fluid for light of a selected colour, Halley recommends allowing a beam of light from the setting Sun to pass through a drop of the fluid hanging from the lower end of a glass tube and finding the angle between the direction of the Sun and that of the drop when the eye was placed so as to receive that colour from the drop. When only one internal reflection of the light was involved, the determination was found to give rise to a specified cubic equation; the biquadratic equation arising in the case of two internal reflections was investigated at Halley's request by Abraham de Moivre.

Halley reported from time to time observations he had made of halos and parhelia (mock-suns) such as are occasionally formed by light passing through ice crystals in the atmosphere. On 8 April 1702, about 10 a.m., he was walking in London when he saw the 'waterish' Sun surrounded by luminous appendages. He entered the house of a Mr Morden, near the Royal Exchange, and hastened upstairs to obtain a clearer view of the phenomenon. In Halley's drawing it is possible to distinguish the white parhelic circle parallel to the horizon and passing through the Sun, the 22° halo surrounding the Sun, and two contact arcs touching the halo and one of them forming parhelia at its two points of intersection with the parhelic circle. Halley noted that the clouds seemed nearer the observer than the circles (*Phil. Trans.* (1702), *23*, 1127f.).

Nearly twenty years later, on 26 October 1721, at about 10.30 a.m., Halley was on his way by river from Greenwich to London when he observed the 22° halo round the Sun. Looking through the thick haze and smoke for any associated phenomena, he noticed to the eastward, at the same elevation as the Sun, a parhelion of about 2° diameter with a narrow white streamer joining it to the Sun, a segment, as he supposed, of the white (parhelic) circle which he had once seen girdling the whole sky. 'But how to explain these Appearances, and account for the Magnitude of these Circles, is what seems still wanting' (*Phil. Trans.* (1721), *31*, 211f.).

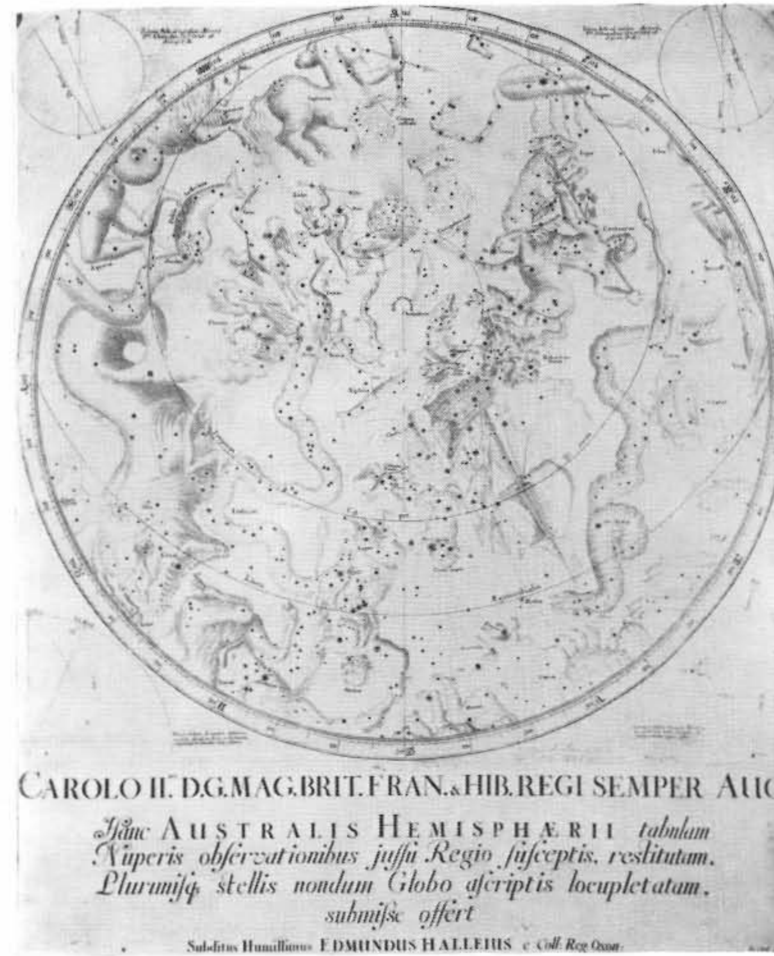


Plate 7 Halley's planisphere of the southern stars (*British Museum*)

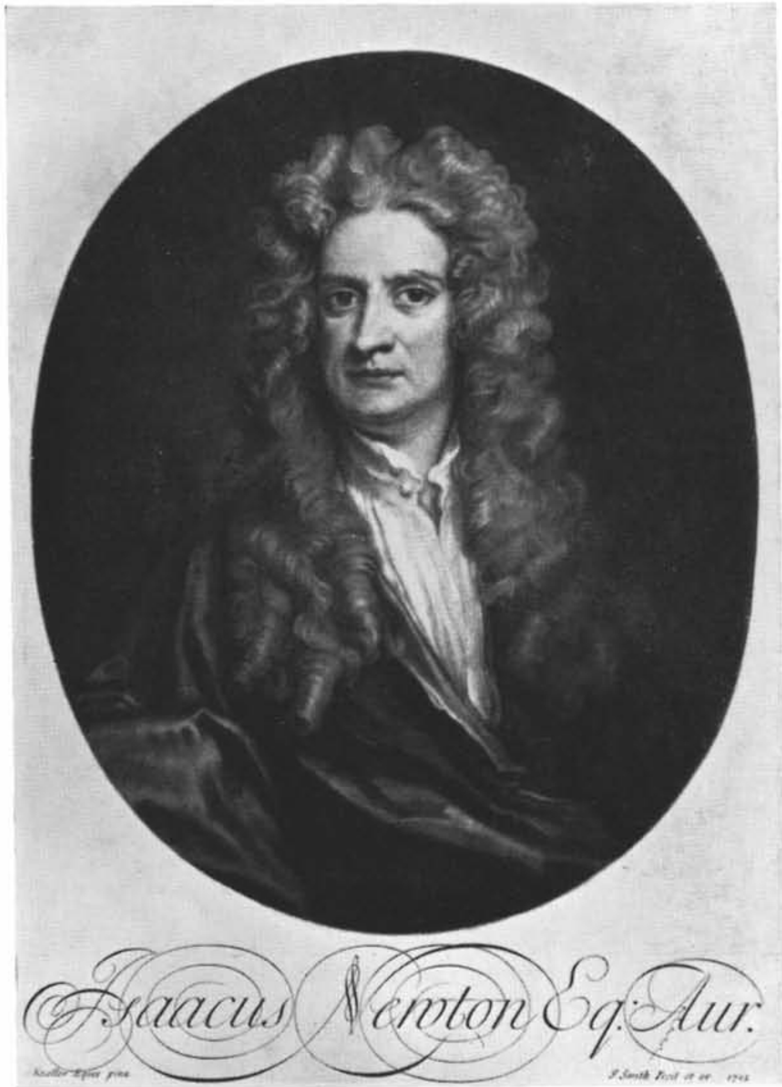


Plate 8 Sir Isaac Newton, after a portrait by Sir Godfrey Kneller (*Crown Copyright reserved, Science Museum, London*)

Chapter 8

Astronomy and General Science

1 Transits of Venus

THE Earth describes about the Sun a disturbed elliptic orbit of small eccentricity; hence its distance from the Sun varies a little in the course of a year. However, a *mean distance* can be defined which is in fact equal to half the major axis of the Earth's orbital ellipse; and this is a quantity of great significance in astronomy. It establishes the scale upon which the solar system is constructed, serving as the unit in terms of which the mean distances of the other planets from the Sun are expressed. It is also the measure of the base-line with reference to which the distances of the nearer stars can be determined by a species of surveying operation. For the past three hundred years astronomers have been striving to estimate this fundamental constant with all attainable accuracy; and it is not the least of Halley's achievements that he opened up a new line of attack upon the problem and bequeathed to his successors a plan of campaign to be put into operation years after he had quitted this mortal scene.

The surveying operation mentioned a moment ago is that by which the distance of an inaccessible object, such as a tree on the far bank of a river, can be ascertained. Besides its application to stellar astronomy, it serves for determining the distances from the Earth of our nearer celestial neighbours, in particular, the Moon and the planet Mars. The operation consists ideally in stationing two observers A and B (Fig. 16) at widely separated places on the Earth, and setting them to fix, at some pre-arranged time, the directions in which they see the body P whose distance is sought. The observers and the object form the three vertices of the plane triangle APB; and it suffices to know the base AB and the base angles in order to 'solve' the triangle and thence eventually to calculate the distance of P from the Earth at the time of the observation. The small angle APB is then the *parallax* resulting from the separation of the two observers. This angle assumes a standard value (*horizontal parallax*) when one observer has the object P vertically overhead while the other sees it upon his horizon, the parallax being then equal to the angle subtended at P by the Earth's radius.

It might be thought that the Sun's distance from us could be found in the same manner; but that distance is so great as to make the horizontal solar parallax less than nine seconds of arc, an amount swamped by the uncertainties arising from the refraction suffered by the Sun's rays in passing through our atmosphere. However, it serves just as well to determine the distance of a

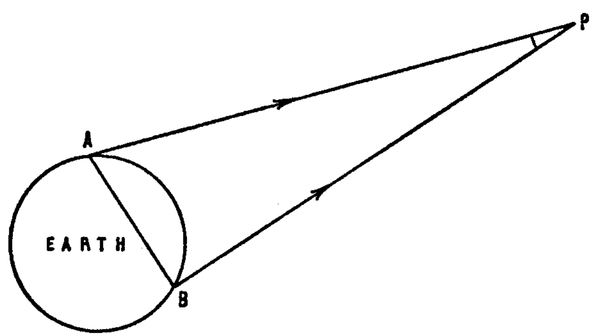


Fig. 16 Finding the distance of a celestial body.

planet from us at some stated time. For there is a simple relation (it is formulated in Kepler's third Law) connecting the mean *distances* at which the planets revolve about the Sun with their *periods* of revolution. These periods are all accurately known from observation; hence the *relative* distances of the planets are known, and a single determination of the distance between the Earth and a planet suffices to establish the scale of the solar system and the Sun's distance from us. And the closer the planet is to the Earth the more accurately can this be done. The earliest reasonably correct estimate of the Sun's distance was, in fact, obtained by this procedure, with Jean-Dominique Cassini and Jean Richer simultaneously observing Mars in opposition from Paris and Cayenne in 1672.

What concerns us here, however, is an ingenious modification

of this method, generally applicable to the planets Mercury and Venus which revolve round the Sun in orbits interior to that of the Earth. On rare occasions one or other of these bodies passes directly between us and the Sun and appears for a few hours in silhouette against the bright solar disc. The observation of such *transits*, as they are called, followed, at any rate in Europe, the invention of the telescope and Kepler's reformation of planetary tables. A transit of Mercury was first observed by Pierre Gassend (and others) in 1631; the earliest glimpse of a transit of Venus was granted in 1639 to Jeremiah Horrox and his friend William Crabtree. In his *Optica Promota* of 1663, James Gregory, mathematician and friend of Newton, briefly touched upon the possibility of determining the solar parallax from observations of a transit of Venus or of Mercury. Fourteen years later Halley, at St Helena, observed the transit of Mercury predicted for 28 October 1677; and he subsequently compared his own observations with those of J. C. Gallet of Avignon to obtain a crude estimate of the Sun's distance. However, better results were to be expected with the planet Venus, which approaches nearer to the Earth than Mercury ever does. And in a Latin paper published in 1691 Halley sought to establish the laws governing the succession of transits for Venus and Mercury, and to predict these phenomena for many years to come (*Phil. Trans.* (1691), *17*, 511ff.). Transits of Venus were shown to occur in pairs separated by eight years with much longer intervals between the successive pairs. In 1716, again in Latin, Halley published a programme for taking advantage of the transits of Venus predicted for 1761 and 1769, 'so that I may point out to our younger astronomers, to whom it may perchance fall (thanks to their youth) to observe these things, a way by which the vast distance of the Sun may be correctly estimated to within one five-hundredth part of its amount' (*Phil. Trans.* (1716), *29*, 454ff.).

Halley's plan was, essentially, to have Venus, V, observed in transit by two observers A and B, stationed in widely different latitudes, which need not be very precisely known (Fig. 17). Both Venus and the Sun show a parallactic shift of direction as between the observers; but the planet, being the nearer, is the more affected, and A and B see it describe different paths, aa_1 and bb_1, respectively, across the disc. The observers record the times taken by the planet in traversing these paths; then, knowing the rate at which Venus is overtaking the Earth in its orbit, and the ratio (VA : VP) (from Kepler's third Law), it is possible to calculate the lengths of the two paths in angular measure. The Sun's angular diameter being known, the two paths can be located on the Sun's disc and the *angular* distance PQ between their mid-points can be calculated. But PQ can be obtained also in *linear* measure from the similar triangles VAB, VPQ, the length of AB and the ratio (VA : VP) being known. Dividing the linear by the angular measure of PQ gives the required distance of the Sun. (In practice, the daily rotation of the Earth and other factors complicate the problem.) All the equipment needed by a transit observer was a telescope and a good clock; but confidence in the results would be increased and the risk from unfavourable weather would be minimized by observing the phenomenon from

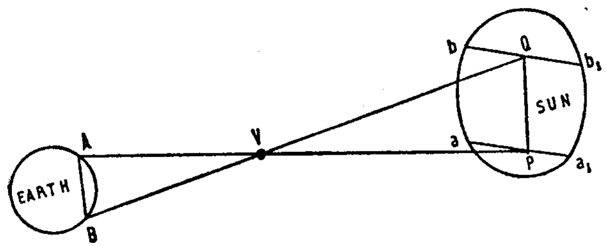


Fig. 17 A transit of Venus.

as many stations as possible. 'Therefore [writes Halley] I strongly urge diligent searchers of the heavens (for whom, when I shall have ended my days, these sights are being kept in store) to bear in mind this injunction of mine and to apply themselves actively and with all their might to making the necessary observations. And I wish them luck and pray above all that they be not robbed of the hoped-for spectacle by the untimely gloom of a cloudy sky; but that at last they may gain undying glory and fame by confining the dimensions of the celestial orbits within narrower limits'.

Halley's programme, supplemented by an alternative and less exacting procedure devised by J. N. Delisle, aroused international interest; and many scientific expeditions were despatched, particularly by the British and French governments, to remote parts of the world where the transits of 1761 and 1769

could be advantageously observed.[1] Working over the results of these transit observations, J. F. Encke narrowed the limits of uncertainty as to the amount of the parallax, arriving at an estimate only about one fifth of a second of arc less than the one now accepted; values based upon the observations of the twin transits of 1874 and 1882 approximated to it even more closely. Nevertheless, Halley's far-sighted project has never entirely fulfilled the high hopes with which he brought it to the notice of astronomers. The exact instants when the planet enters and leaves the Sun's disc have proved difficult to estimate owing to the formation of an illusory ligament, the 'black drop', joining the planet's disc to the Sun's limb about the time of internal contact, an effect probably produced by the planet's atmosphere. Hence there has been a return to the classic procedure of determining the parallax of a planet revolving in an orbit exterior to that of the Earth. The limits of uncertainty have been narrowed by selecting for the purpose (as in 1930–1) the minor planet Eros, which can approach to within 14 million miles of the Earth (little more than a third of the least distance of Mars) and by enrolling scores of observatories in a concerted campaign to secure the asteroid's parallax, and thence that of the Sun, by visual and photographic methods.

2 *The Secular Acceleration of the Moon*

In Halley's day little was known about the revivals of the practice of astronomical observation that had occurred from time to time among the Islamic astronomers of the Middle Ages. It seemed that there had been a great gap between the celestial observations cited by Ptolemy and those recorded by Regiomontanus some thirteen centuries later. However, Halley had come across the principal work of Al-Battani (latinized as Albategnius), one of the greatest of Oriental astronomers; and it seemed to him to have a particular significance as falling almost exactly midway between the age of Ptolemy and the late seventeenth century. Al-Battani flourished about A.D. 900 at Al-Raqqa on the Euphrates; his book was translated into Latin (as *De motu stellarum*) by Plato of Tivoli in the twelfth century. There were

[1] See Harry Woolf, *The Transits of Venus: A Study of Eighteenth-Century Science*. Princeton, 1959.

two printed editions (Nuremberg, 1537, and Bologna, 1645); and it was these, not the Arabic original, that Halley consulted. It seemed that Plato of Tivoli was versed neither in Arabic nor in astronomy; and at the bidding of the Royal Society Halley undertook to purge the text of its numerous errors and to restore the lunar and solar tables of Albategnius (*Phil. Trans.* (1693), *17*, 913ff.).

A comparison of observations between Ptolemy and the Muslim astronomer gave improved values for the length of the year and the eccentricity of the Earth's orbit. However, Halley noticed that, in three out of four eclipses (two solar and two lunar) observed by Al-Battani, the computed time of the eclipse fell earlier than the observed time by as much as an hour. On this very slender evidence, Halley began to suspect that the Moon suffered a slow acceleration in its orbital motion. For if the places of the Moon in its orbit at two widely separated epochs are known, and if the Moon be supposed to have travelled from the one place to the other with a uniform, mean motion, while, in actual fact, it has suffered a steady acceleration, then its computed place at any intermediate date will be in advance of its actual place, and eclipses will appear to fall late.

Halley announced his hypothesis of a secular acceleration of the Moon in 1695 at the end of a paper dealing with the history and antiquities of the ancient Eastern city of Tadmor, or Palmyra (*Phil. Trans.* (1695), *19*, 160ff.). Several papers had recently been contributed by English explorers who had visited the imposing ruins, among them William Halifax, Chaplain to the Factory of Aleppo, whose account of his expedition had reached the Royal Society through the hands of Edward Bernard, sometime Savilian Professor of Astronomy at Oxford. Halley traced the fortunes of the city from its traditional foundation by Solomon to its sack by Aurelian in A.D. 272; and he ventured on the interpretation of certain surviving inscriptions, such as had contributed much to the interest of the other papers. It now appeared that 'Aracta', where Al-Battani had made his observations, was Racca (Al-Raqqa) on the Euphrates. Halley recommended that the latitude of the place, carefully measured by Al-Battani, should now be re-determined, 'thereby to decide the Controversie, whether there hath really been any Change in the

Axis of the Earth, in so long an Interval', as had lately been seriously suggested. However, no greater service could be rendered to astronomy than to determine the longitudes of Baghdad, Aleppo, and Alexandria by concerted observations of lunar eclipses. 'For in and near these Places were made all the Observations whereby the Middle Motions of the Sun and Moon are limited: And I could then pronounce in what Proportion the Moon's Motion does Accelerate; which that it does, I think I can demonstrate, and shall (God Willing) one day make it appear to the Publick' (p. 174f.).

A few years after Halley's death the problem was taken up by Richard Dunthorne, a distinguished mathematical astronomer who had risen from humble beginnings. He established good agreement between the Newtonian lunar theory and recent observations of the Moon, and he then proceeded to test whether the theory was concordant with earlier observations as well or 'whether I should be able to make out that Acceleration of the Moon's Motion which Dr. Halley suspected' (*Phil. Trans.* (1749), *46*, 162ff.). The computed tables were supported by several eclipses observed by Tycho Brahe in the late sixteenth century; but that was too recent to have allowed the suspected acceleration to become evident. Records of eclipses observed by Bernhard Walther and Regiomontanus in the fifteenth century showed the computed place of the Moon to have been ahead of its true place by 5 minutes of arc or more; however the observations were too discordant for an acceleration to be inferred with certainty. In three of the four eclipses of Al-Battani studied by Halley, the computed places of the Moon were considerably too forward; and if Dunthorne could have been sure of the longitude of the Muslim astronomer's place of observation he would have been confirmed in the opinion 'that the Moon's mean Motion must have been swifter in some of the last Centuries than the Tables make it,' though the differences were not sufficiently regular to afford certain proof. However, a group of Ptolemy's eclipses, taken in conjunction with one noted by Theon of Alexandria in A.D. 364 and two observed at Cairo in A.D. 977-8, went to show that

> the mean Motion of [the Moon] has been something greater in the last 700 Years than the Tables suppose it, and therefore must have been

accelerated. . . . If we take this Acceleration to be uniform, as the Observations whereupon it is grounded are not sufficient to prove the Contrary, the Aggregate of it will be as the Square of the Time: And if we suppose it to be 10″ in 100 Years, and the Tables truly represent the Moon's Place about A.D. 700 it will best agree with the before-mentioned Observations.

Dunthorne's conclusions, and his estimate of the amount of the acceleration, were generally confirmed by Tobias Mayer and Lalande. Attempts to account for the phenomenon on dynamical principles were at first unavailing. Euler attributed it to the resistance of a medium encountered by the Moon in its course. In 1787 Laplace claimed to have traced the effect to a slow diminution in the eccentricity of the Earth's orbit, but in 1853 J. C. Adams showed that Laplace's theory could account for only about half of the observed acceleration. Meanwhile, in 1754, Immanuel Kant, the philosopher, had drawn attention to the effect that the friction of the tides must have in retarding the Earth's rotation and thus progressively lengthening the day, so that all celestial processes would appear to be accelerated in a certain proportion. This effect would be especially conspicuous in the Moon, whose rate of travel appears more rapid than that of any other heavenly body. After the partial failure of Laplace's theory, Kant's suggestion was revived by C. Delaunay; and the sufficiency of tidal friction for the role assigned to it has been established by the researches of Sir G. I. Taylor and Sir H. Jeffreys.

3 *How Big is an Atom?*

According to Newtonian mechanics, the weight of a body is proportional to the quantity of matter it contains, of whatever material the body consists. That is why all bodies tend to fall with the same acceleration in an exhausted tube and why the period of a pendulum is not affected by the choice of the material composing the bob. On this view, Halley found it difficult to explain why two substances, of apparently equal solidity and density, could differ so much in specific gravity. A lump of gold weighs about seven times as much as a lump of glass of equal size; and this seemed to imply that the gold contained seven times as much matter as the glass, so that at least six-sevenths of the glass must consist of

empty space. It was natural to approach the problem through the hypothesis of the atomic constitution of matter. This ancient doctrine, long eclipsed by the opposing view of Aristotle, had enjoyed a revival in the seventeenth century, largely through the speculations of Gassend, Boyle, and Newton, with which Halley would be familiar. It was generally agreed by the Atomists that all the atoms were composed of the same primary matter. They were often conceived as being all of the same size, but variously aggregated to form the various natural substances. Or they might differ in size from one substance to another. Halley supposed that the reason for the high density of gold might be that the metal consisted of exceptionally large atoms with correspondingly fewer pores between them; and this brought him on to the problem of determining, or assigning an upper limit to, the size of the gold atom by calculating the thickness of the film of gold on silver-gilt wire (*Phil. Trans.* (1691), *17*, 540ff.).

Halley informed himself among the wire-drawers and learned that it was their practice to commence operations with a cylindric ingot of silver 4 inches in circumference and 28 inches long and weighing 16 pounds troy (12 oz. to the pound), to which they applied 4 ounces of gold. When the wire was drawn, 2 yards of it weighed one grain (1/5,760 lb. troy). It thus appeared that 98 yards weighed 49 grains, and that one grain of gold covered 98 yards of the wire, so that one hundred-thousandth of a grain would be visible to the unaided eye. The cross-sectional area of the wire is calculated and is divided between the silver core and the gold skin directly as the quantities of the two metals present and inversely as their respective densities. The thickness of the gold skin proves to be of the order of 1/134,500 inch, so that, even supposing the skin to be only one atom thick, the cube of one hundredth of an inch would contain 1,345 *cubed*, or more than 2,433,000,000 atoms. But, in fact, though spread so thinly, the gold showed so united a texture as not to suffer the silver to show through it at any point, 'which argues that even in this exceeding thinness, very many of those Atoms may still lie one over the other'.

In 1688 Halley had proposed experiments on solution and the glaciation of fluids designed to 'discover the hidden secret of the figures and motions of the constituent parts of the most simple substances, by which we must begin if we shall ever hope to attain a true and adequate notion of materiall substances'. He examined with a microscope the formation of salt crystals by precipitation from water, noting that 'when they first begin to appear out of the liquor, the figures are exactly square, and all of a magnitude; that the edges of the square are first coagulated, whilst the insides continue fluid'. Repeating the observations with nitre, he noticed that the crystals 'shott as by a kind of vegetation out of and above the water . . . and after it proceeded to emitt branches, resembling those of trees' (*Correspondence*, 138, 215).

4 *Studies on Thermometry*

The rapid development of science since the beginning of the seventeenth century has owed much to the invention and perfection of appropriate instruments designed for the detection and measurement of natural phenomena. In his labours as an astronomer, Edmond Halley was chiefly dependent upon the telescope and its adjuncts; but through his wider interests in physics and meteorology he became involved in the use of other instruments and concerned for their improvement. We shall look now at his account of some experiments that he undertook with a view to selecting an ideal thermometric fluid and standardizing the thermometric scale (*Phil. Trans.* (1693), *17*, 650ff.).

The thermometer appears to have been invented by Galileo and developed by members of his circle late in the sixteenth century. Air was the thermometric medium first employed; and the instrument consisted of a glass bulb terminating in a long tube the open end of which dipped down into water. Heating or cooling the bulb caused the contained air to expand or contract, and a column of water half filling the tube fell or rose in consequence. It was a natural step to attach to the tube an arbitrarily graduated scale. The readings of such an air thermometer, however, are affected by changes in the atmospheric pressure as well as in the temperature. This source of error, clearly recognized by Robert Boyle in 1665, was often ingeniously eliminated; but it favoured the introduction of liquid-in-glass thermometers, which are not thus affected.

The excellent alcohol thermometers of the Florentine Academicians were known all over Europe in Halley's youth; but, like all these early thermometers, they suffered from the fundamental defect that, for want of a universal standard scale of temperature, the indications of one such instrument were not comparable with those of another, particularly when they had been fashioned by different craftsmen. To remedy this defect, the graduation of thermometers began to be related to some *fixed point* indicating where the fluid stood in the tube when at the temperature of some easily reproducible physical change, such as the freezing of water. The successive degrees on the scale were then made to correspond to expansions of the fluid in equal proportions of its volume at freezing-point. That was broadly the situation when Halley turned his attention to the problem, although the use of *two* fixed points had already been suggested. About the time when Halley was writing, Carlo Renaldini recommended the melting-point of ice *and* the boiling-point of water for this purpose, the interval to be divided into twelve equal degrees.

'Qualities [Halley begins] such as Heat and Cold, Moisture and Driness, and the like, are not otherwise to be estimated, but by their Effect on the Quantity of some body they act on . . . or else by the Motions they produce'. Halley was aware that the same degree of heat did not expand all fluids (he is thinking particularly of air and alcohol) in the same proportion; and his paper of 1693 is chiefly concerned with the results of an investigation he had made in 1688 (*Correspondence*, 211) to test the comparative thermal expansions of water, mercury, and alcohol (spirit of wine), with a view to discovering which fluid would serve best as a thermometric medium.

Halley took a large flask and filled it with cold water up to a point in its long, narrow neck. He then plunged the flask into a 'skillet' or pan of warm water and waited until this had communicated its 'Temper' to the water in the flask, which showed but little expansion. He put the pan on the fire to boil; the water in the flask rose to a certain fixed height which was noted; but when the flask was taken out and allowed to cool, the water sank to its original level. The total expansion was about one twenty-sixth part of the initial bulk of the water. 'It was obvious that Water, encreasing so very little with all the degrees of Heat the Air receives from the Sun, was a very improper Fluid to make a Thermometer withal; and besides, any freezing Liquor is useless for this purpose in these Northern Climates'. Next, Halley filled a smaller flask with mercury and plunged it into the pan of water, which was then boiled. The mercury responded quickly and had almost reached its maximum height in the neck of the flask before ebullition began. The expansion amounted to about one seventy-fourth of the initial bulk, to which the mercury returned upon cooling. 'This Fluid being so sensible of a gentle Warmth, and withal not subject to evaporate without a good degree of Fire, might most properly be applied to the Construction of Thermometers were its Expansion more considerable.' However, small as it is, the expansion due to the change from the cold of winter to the heat of summer is sufficient to produce a difference of about a fifth of an inch in the height of a barometric column. Lastly, the spirit of wine, when subjected to the same treatment, expanded slowly at first, then more rapidly, increasing its volume by about one-twelfth; but at a certain degree of heat, far below that of boiling water, it began to boil violently, 'which great Dilatation makes it a Liquid sufficiently adapted to our purpose, were it not for the Evaporation thereof'.

None of the three liquids studied had as great a thermal expansion as air. Experiments carried out by Boyle and Halley suggested that, between winter cold and summer heat, air might expand by one-twelfth of its normal volume. 'For which reason, and for its being so very sensible of Warmth or Cold, and continuing to exert the same Elastick Power after never so long being included, in my Opinion it is much the most proper Fluid for the purpose of Thermometers'. Halley thought the boiling-point of alcohol might serve as a fixed point for the graduation of thermometers: 'This degree of Heat which made Spirit of Wine begin to boil, being determined so nicely as I have said, made me conclude, that this might very properly be taken for the Limit of the Scale of Heat in a Thermometer; and the effect thereof in the expansion of any other Fluid being accurately noted, might be easily transferred to any sort of Thermometer whatever'. There was, however, the difficulty that the boiling-point varied according to the purity of the spirit employed; indeed it might

serve as a test of the purity. Halley's suggestion for choosing a lower fixed thermometric point was not so progressive: instead of the freezing-point of a liquid, one should adopt the temperature (Halley employs this word also) of places deep underground, where summer and winter make no difference, as Mariotte found in the cellars of the Paris Observatory.

5 Light and Sound

The telescope and the microscope appear to have been invented by chance, with no precise understanding as yet of the optical effect of passing a ray of light through a lens. However, the interest aroused by the discoveries made with these instruments, and the need to understand their working and to improve their construction, alike encouraged the study of the geometrical optics of lenses. The pioneer in this field was Johannes Kepler who, in his *Dioptrice* of 1611, gave an approximate theory of the telescope though still in ignorance of the now accepted sine law of refraction. This law, discovered by Willebrord Snell, was formulated by Descartes in 1637; it was employed by Huygens, by Cavalieri, and (in 1674) by Isaac Barrow to determine the focal length of a lens by the detailed consideration of many special cases. It was reserved for Halley to derive a formula for the focal length of a 'thick' lens (that is, of a lens whose thickness is taken into account) as 'an Instance of the Excellence of the Modern Algebra', the various cases being covered by changing the signs of the terms in an algebraic expression (*Phil. Trans.* (1693), *17*, 960ff.)

Halley proceeds by tracing the path of a nearly axial ray proceeding from a point object situated on the axis at a specified distance from the lens, and finding where it cuts the axis again after refraction through the lens, whose refractive index, thickness, and radii of curvature are given. He thus obtains the distance of the point image from the lens; and this becomes the required focal length when the distance of the point object is made infinite. The expression obtained does not differ essentially from that given in a modern textbook.

Halley passed his formative years in the midst of keen controversy as to the nature of light. It was generally accepted that vision was produced by something travelling to the eye from the object seen; but opinion was divided as to the physical mechanism involved. Kepler had drawn on medieval ideas in conceiving light as an immaterial 'species' propagated at an infinite speed. In the system of Descartes light was a mechanical pressure exerted by a luminous source and communicated to the eye through the intervening matter. In the latter part of the seventeenth century two conflicting theories arose. Light was conceived either on the analogy of a stream of particles shot out by the light-giving body, or as originating in vibrations of the luminous source which caused waves to travel outward through the aether filling all space. The former hypothesis became chiefly associated with Newton, who held a corpuscular theory of light (in which, however, aether vibrations played some part) and whose views became known to Halley considerably in advance of their publication in the *Opticks* of 1704. The rival wave theory received its classic formulation from Christiaan Huygens, who showed that it was capable of accounting for most of the optical phenomena known at that period.

The publication of Huygens's *Traité de la Lumière* in 1690 revived discussion as to the nature of light; and it prompted Halley to ventilate some difficulties he had encountered in his optical studies. Like Newton, he framed them in the form of Queries (*Phil. Trans.* (1693), *17*, 998f.). Why, he asks, are such media as glass and water transparent? Is it sufficient to assume that they are traversed by rectilinear pores through which light can pass directly? Why are bodies such as wood shavings or paper, which are more porous than glass or water, nevertheless opaque? Is light propagated more easily (and rapidly) through glass or water than through air or the aether of space, as Descartes and Hooke maintained, or is it retarded in these denser media as required for Huygens's geometrical explanation of refraction? On this latter view, why does light, passing through a glass plate, acquire again upon its emergence the same speed as it had before it entered the glass? Why is mercury almost the only pure, simple, homogeneous fluid which is not transparent? If, as Descartes and Hooke supposed, light travels with greater ease through a transparent solid medium, why does this medium, however pellucid, reflect *some* of the incident light and cast a shadow? Can any texture of atoms of the same primary matter explain the pellucidity of heavy bodies and the opacity of light ones? If light be an undulation of the aether, 'as it is most likely', as sound is an undulation of the air, and if the aether freely interpenetrates all bodies, should not all or most bodies be transparent? Are not the ultimate particles of matter of several different kinds, the differences among bodies not arising solely from varieties in the *arrangement* of their constituent atoms, as maintained by the ancient Atomists and generally by those of Halley's day?

William Derham records how Flamsteed and Halley tried to determine the speed of sound by trials carried out on Shooter's Hill, near the Royal Observatory, over a measured distance of about three miles (*Phil. Trans.* (1708), *26*, 3f.). They used a clock to estimate the time-interval between the flash of a distant explosion and the arrival of the sound; and they gave the speed as 1,142 feet a second (modern value at standard temperature and pressure: 1,090 feet a second). Halley sometimes doubted whether sound was transmitted by a vibration of the air. In 1691 he cited an experiment in which the striking of a watch was heard through combined layers of glass and water from the substance of which, he argued, air was completely excluded (*Correspondence*, 224).

6 *Natural History*

Halley's interests lay predominantly in the fields of physics and astronomy; but he lived before the days of narrow specialization in science; and he could not but be challenged by the record of the rocks as he surveyed the English scene, and fascinated by the unfamiliar forms of life that met his eye as he voyaged through the uncharted seas. It may be convenient to gather together at this point some of Halley's observations on natural history.

On 6 June 1683 'Mr. Halley described a sailing fish about St. Helena, called a carvel, being like a worm in a bladder' (T. Birch, op. cit., iv, 208). He had found the blood of a newly killed sea tortoise 'as cold as water' (ibid., 257). On 22 June 1687 Halley showed the Society a plant called the 'Star of the Earth', a principal ingredient in a supposed remedy for the bite of a mad dog and stated to grow abundantly near Thetford in Norfolk (ibid., 543). Later the same year he described 'water galls', apparently some species of jelly-fish; and he offered a mechanical explanation of the intermittent emptying and refilling of Lake Zirknitz in Austria (ibid., 549, 558). Recalling again his experiences in St Helena, Halley described a sort of *adianthum*, 'which bore perfect plants with a root on the Extremities of its leaves', and a polypus native to the Island

> which emits an Inky Juice, but of a dark russet colour. This fish, called there a Cuttfish, will walk on the dry land, on its points, as it were with Leggs, raising itselfe like a great long legged spider, and being pursued he makes to the water, where immediately he reciprocates by a motion not unlike respiration, the water through a glandulous substance in his body, whereby the water becomes troubled and opake, but if so be this will not secure him, he will stick so fast to the rocks by the means of several acetabula on his points, or leggs, that he will be torn in pieces before he will let go his hold (ibid., 211, 214f.).

In 1688 Halley produced before the Society some shells from Harwich, which he had taken from a sloping bed of them two feet thick and 90 feet above sea level, but which must once have formed the sea bottom (ibid., 213).

In the same year, 1688, Halley related an observation made by the keeper of the Chelsea Physic Garden, who had noticed that a plant covered with a bell-glass flourished, but that if the glass were covered with brown paper or some other opaque substance the plant became white, withered, and died. Halley 'conceived it was necessary to the maintenance of vegetable life that light should be admitted to the Plant'. He proposed as a test the experiment of covering the plant all day with an opaque bell and all night with a transparent one in order to see whether the factor essential to the plant's life was really light or some other aethereal substance that only passed through the pores of transparent bodies (ibid., 211). The essential role of light in the economy and growth of plants was established about ninety years later by Jan Ingenhousz and by Joseph Priestley.

In 1722 the Chelsea Garden was placed under the care of Philip Miller, the author of a celebrated *Gardener's Dictionary* and from whom, in years to come, the youthful Joseph Banks was to learn the nurseryman's craft. Among a bundle of letters used

by Francis Baily in compiling an article on Halley there was found one (dated 21 December 1739) from Miller to the astronomer. The bearer of the letter had been engaged on Miller's recommendation to serve the East India Company as their gardener at St Helena; and Halley was asked to give him a letter to the Governor of the Island (who was known to the astronomer) requesting permission for the man to collect a few plants and seeds to send to England. The letter concludes, 'We heard yesterday you were well and drank to your good health at the Queen's Arms' (Miss Lindsay-MacDougall's file at the National Maritime Museum).

Chapter 9

Diver and Mint-Master

1 The Art of Living under Water

ALREADY in 1688 Halley had become interested in the problem of supplying air to divers; and about 1691 he appears to have been engaged in actual under-water operations. Some contributions of his own to the technique of diving were communicated about this time; and eventually he published a paper on 'The Art of Living under Water' (*Correspondence*, 144f., 150ff.; *Phil. Trans.* (1716), *29*, 492ff.).

It appeared that an inexperienced person could not remain under water for more than half a minute without distress, but that a practised diver could stay down for as much as two minutes, 'as I once saw a Florida-Indian at Bermuda'. These times were in general agreement with the results of an experiment which showed that a gallon of air, confined in a bladder and alternately breathed in and out through a pipe, became unfit for respiration in about a minute. A diver could slightly prolong his stay below the surface by taking down with him a sponge dipped in oil and drawing upon the air imprisoned therein. Or sometimes the diver was encased in a species of armour through the interior of which air was circulated by means of a bellows and a pair of flexible tubes communicating with the surface. This had the advantage of relieving the pressure of the water on the diver, which is an added complication, but at depths of more than about three fathoms there was a danger that the water would force a way in through any chink in the integument and drown the diver before he could be hauled to the surface.

These various inconveniences were remedied by the introduction of the diving-bell, which dates, in essentials, from the middle of the sixteenth century. Shaped like a truncated cone open at the base, it was loaded and suspended so as to sink full of

air, the diver seated within. If large enough to hold one tun of water (252 gallons) the bell (so Halley reckoned) would contain enough air to supply one man for at least an hour at 5 or 6 fathoms. However, the air in a diving-bell contracts under the pressure of the surrounding water so as to occupy only about one half of its initial volume when the bell has sunk to a depth of 33 feet. The condensation and subsequent rarefaction of the air which occur as the bell is lowered and then raised should have no ill effects on the divers if the motion is slow, though the descent is attended by pain in the ears which, Halley found, was relieved by introducing oil of sweet almonds into the ears. But the contraction is inconvenient to the occupants of the bell, who are thereby immersed in water and cannot long endure the cold; also the air becomes heated and unfit to breathe, and the bell has to be hauled up and recharged with air at frequent intervals.

Halley's ingenuity supplied a remedy for these drawbacks by suggesting a contrivance for conveying air down to the bell and expelling the intruding water at any depth (Pl. 12b). He used a wooden bell of 60 cubic feet capacity, weighted with lead and furnished above with a plate-glass window and a cock for letting out the exhausted air. Within the bell was a bench, and below it a stage was hung; and the whole was slung from a projecting spar secured to the mast of the parent ship. To supply air to the bell, Halley provided two barrels cased in lead; each had a bung-hole in the bottom to let water in and out and another hole at the top to which was fixed a leather hose with its open end weighted so as to hang down below the level of the bung-hole. The barrels were contrived to rise and fall alternately like the buckets in a well. Each was received in turn by a man on the stage who lifted the hose, whereupon the pressure of the water blew the air with great force into the bell. In this manner the divers were amply supplied with air: 'I my self have been One of Five who have been together at the Bottom, in nine or ten Fathoms Water, for above an Hour and a half at a time, without any sort of ill consequence.' When the stage was removed and the bell rested on the sea bed, the divers might not be over shoes in water, and the light from the window sufficed for reading and writing when the sea was clear; when it was turbid a candle could be used. Halley used the barrels to send up orders written with an iron pen on leaden plates. It was noticed that when the bell was hauled up, 'the Air coming to be rarified would immediately fill with a white fogg'. Other observations seemed to suggest to Halley that no sound in the upper air, not even the discharge of a cannon, could reach the occupants of a submerged diving-bell.

Halley explained how a diver, wearing a 'cap of Maintenance', could go some distance from the bell and be supplied by air conveyed to him by flexible tubes which served also as a clue to direct him back to the bell. The fabrication of these tubes was the subject of a later paper (*Phil. Trans.* (1721), *31*, 177ff.). In somewhat the same way a lamp or candle could be kept burning under water for the diver's convenience. To avoid being carried about by the turbulence of the sea, Halley provided himself with a 'girdle of leaden shott quilted as is used for making of Horsmans weight'. Wearing this, the diver descended to the bell, which remained permanently on the sea bottom. To return to the surface, he slipped off the girdle and was borne up by the air in his cap. Halley proposed to raise sunken ships by attaching empty bells to them and then filling these from air barrels. He suggested forms of pressure gauge for enabling the diver to ascertain his depth below the surface; also a device for blowing up a sunken ship or a burning house with a small quantity of gunpowder.

In discussing the technique of diving, Halley glanced briefly at the problem of what happens to air in respiration. The Royal Society had done much to establish the view that the air contains some constituent necessary alike for respiration and combustion and consumed in these processes. The air, he writes,

> though its Elasticity be but little altered, yet in passing the Lungs, it loses its vivifying Spirit, and is rendered effete, not unlike the Medium found in Damps which is present Death to those that breath it; and which in an instant extinguishes the brightest Flame. . . . I shall not go about to show what it is the Air loses by being taken into the Lungs, or what it communicates to the Blood by the extream ramifications of the Aspera Arteria, so intimately interwoven with the Capillary Blood-Vessels; much less to explain how 'tis performed, since no discovery has yet been made, to prove that the ultimate Branches of the Veins and Arteries there, have any anastomoses with those of the Trachea; as by the Microscope they are found to have with one another.

This problem Halley was content to leave to 'Curious Anatomists'.

Constantyn Huygens, writing to his more famous brother Christiaan (26 January 1962, New Style), described a conversation with Halley in which the astronomer had had much to say about his work with diving-bells, and how he had remained an hour at a depth of 60 feet below the surface without suffering the slightest inconvenience and had 'recognized all the kinds of fishes in whose company he found himself' (C. Huygens, *Œuvres Complètes*, x, 237). It is clear, in fact, that Halley acquired considerable personal experience of working with diving-bells. His interest in their improvement was not merely theoretical. It seems that he formed a company for salvaging wrecks; the price of its shares (or 'actions' as they were called) was regularly quoted, and we find it printed week by week (with those of other companies) from 1692 to 1696, under the heading 'Diving, Halley' in the original issues of John Houghton's *Collection for Improvement of Husbandry and Trade* (London, 1692-1703). Offering a few 'tips' to investors, Houghton has this to say about Halley's diving venture: 'Some good Effect has been of Diving; but if Mr. Halley's should succeed, of which (were the Wars at an end, and the Seas secure) he seems very sure; and, to my knowledge, has given such reasons for, as abundance of Learned and ingenious Men cannot gainsay, it would be very considerable' (*Collection*, No. 103, 20 July, 1694).

There is evidence that Halley's salvage operations centred for some time, about 1691, upon Pagham in Sussex. On 13 May 1691, 'Halley shewed the Method he intended to use in raising the Ship' (*Correspondence*, 224), after which there are no Royal Society Minutes until 12 August of that year. He was obviously in the thick of some such enterprise when, in June 1691, he wrote from Pagham to Abraham Hill, Comptroller to the Archbishop of Canterbury, asking that the election to the vacant Savilian Chair in Astronomy be postponed until Halley could return to London and clear himself of the charge of asserting the existence of the world from all eternity (*Correspondence*, 88). In August he reports having received the Royal Society's commands to give them an account 'of the success of my attempt on the Guiney frigat' (ibid., 150). Three years later he wrote from Chichester, in Sussex, to ask Sloane to take the Minutes of the Royal Society's meetings during his absence that summer—evidently again on the south coast (ibid., 89f.).

It seems to have been the practice in Halley's day to send the fleet to sea for only about four months of the year. He found that the great ships were laid up during the squally season partly because the weight of their guns strained their sides and tended to force them apart. He suggested in 1692 that the breeching tackle by wnich a gun was held in its place should be secured to the opposite side of the ship; this would actually help to bind the timbers together (*Correspondence*, 164f.). Hooke's comment (14 December 1692) was 'Hally's way to lash guns absurd' (R. T. Gunther, op. cit., x, 197).

2 *Oxford Candidature of 1691; Religious Beliefs*

In 1673 Edward Bernard succeeded Sir Christopher Wren as Savilian Professor of Astronomy at Oxford. As the years drew on, however, Bernard found his duties increasingly distasteful and he would gladly have resigned his Professorship in favour of Flamsteed or Halley if some satisfactory arrangement could have been made (*Vita Bernardi* in T. Smith, *Roberti Huntingtoni Epistolæ*, etc., Londini, 1704, 45). Bernard was, in fact, succeeded in 1691 by David Gregory. Halley's claims to the Chair were considered, but he was passed over on account of his religious unorthodoxy. William Whiston, the mathematical divine, wrote:

> I will add another Thing which I also had from Dr. Bentley himself. Mr. Halley was then thought of for Successor, to be in a Mathematick Professorship at Oxford; and Bishop Stillingfleet was desired to recommend him at Court; but hearing that he was a Sceptick, and a Banterer of Religion, he scrupled to be concerned; till his Chaplain Mr. Bentley should talk with him about it; which he did. But Mr. Halley was so sincere in his Infidelity, that he would not so much as pretend to believe the Christian Religion, tho' he thereby was likely to lose a Professorship; which he did accordingly; and it was then given to Dr. Gregory: Yet was Mr. Halley afterwards chosen into the like Professorship [of Geometry] there, without any Pretence to the Belief of Christianity (*Memoirs of the Life and Writings of Mr. William Whiston*, London, 1749-50, i, 123).

Halley's writings, indeed, convey his strong belief in an all-wise and all-powerful Designer and Creator of the realm of nature, and his acceptance of the Mosaic story of creation as set forth in the Book of Genesis. However, several anecdotes about him tend to support Whiston's assertion that he was not an orthodox Christian; he seems to have held some form of Unitarian doctrine, perhaps Arianism (which denies the coeternity and the consubstantiality of the Son with the Father). On the other hand, Halley's religious orthodoxy was maintained by S. P. Rigaud, who, towards the end of his life, collected materials for a biography of the astronomer, and by his son, S. J. Rigaud, whose intention to complete the book remained unfulfilled. They trace the charge of unorthodoxy to Flamsteed; and they point out that Whiston published his account of the Savilian election more than fifty years after the event, when he was nearly 80 and Halley and Bentley were both dead. They quote the letter written by Halley from Pagham in 1691 to Abraham Hill asking him to intercede for the election to be deferred until Halley could return to clear himself from a charge of asserting the existence of the world from all eternity, a doctrine against which several of his published papers were directed. This letter contains no such suggestion of defiant unbelief as is suggested by Whiston's anecdote.

Whiston himself suffered for his Arian views, being deprived of the Lucasian Chair in which he had succeeded Newton. Yet he held to many traditional Christian beliefs and practices including that of fasting until 3 o'clock in the afternoon on Wednesdays and Fridays:

> On my Refusal from [Halley] of a Glass of Wine on a Wednesday, or Friday, . . . he said He was afraid I had a Pope in my Belly, which I denied, and added somewhat bluntly, that had it not been for the Rise now and then of a Luther, and a Whiston, he would himself have gone down on his Knees to St. Winifrid and St. Bridget: Which he knew not how to contradict (ibid., 243).

One of the leading astronomers of his time, Whiston was refused the Fellowship of the Royal Society as 'an Heretick'. At Child's Coffee-House one day with Whiston and Sloane, Halley said that he would second Whiston's election to the Society if

Sloane would propose it. Newton, who at least maintained a show of orthodox belief, was greatly concerned and threatened to resign from the Presidency of the Society if the election went forward; and Whiston did not press the matter (ibid., 292f.).

It is worth noting that the Royal Society supported Halley's claim to the vacant Oxford Chair in 1691, their testimonial being drawn up by Dr Gale, the astronomer's old schoolmaster (*Correspondence*, 226).

3 Classical Studies

Halley gave proof of a remarkable capacity for combining classical scholarship with scientific deduction by his ingenious investigation of the time and place of Julius Caesar's first invasion of Britain (*Phil. Trans.* (1691), *17*, 495ff.). For the history of the episode he was dependent upon the account given by Caesar in the fourth book of his Commentaries, supplemented only by a few points from Dion Cassius, a Greek historian who wrote nearly three centuries after the event.

Caesar's first descent upon our shores took place during the consulate of Pompey and Crassus, which fixes the *year* as 55 B.C. 'Only a small part of the summer was left' when he crossed the Channel with two legions, the Seventh and the Tenth, carried by a fleet of about 80 ships; 18 other vessels, which were to transport the cavalry, remained wind-bound at their port of embarkation. Arriving off the coast of Britain one morning at the fourth hour (between 9 and 10 a.m.), the Romans found the natives lined up in hostile array on cliffs from which they were able to cast their darts down on to the beach. Caesar waited at anchor until the ninth hour, then 'catching at one and the same moment a favourable wind and tide, he gave the signal, and weighed anchor, and moving on about seven miles from that spot, he grounded his ships where the shore was even and open' (Caesar, *The Gallic War*, translated by H. J. Edwards, Loeb Classical Library, 1917, 211). There the Romans made an opposed landing. Four days later an abortive attempt was made to bring the cavalry across the Channel; and 'that same night, as it chanced, the Moon was full'. Also we are told, 'the equinox was close at hand'.

From the indications of time thus furnished by Caesar, Halley identifies the full moon referred to as that of the night of 30 to 31 August, which would make 26 August in 55 B.C. the *date* of the landing. As for the *place*, Halley conjectures that the fleet first touched the coast of Britain at Dover. In Book V, Caesar gives the most convenient passage to Britain from Portus Itius (probably Boulogne) as involving a transit of about 30 miles, which seemed in good agreement with Halley's hypothesis. There remained the problem of deciding in which direction the Romans sailed along the coast after leaving Dover. From the age of the Moon and the time of day, Halley deduces the direction in which the tide must have been flowing on 26 August at the ninth hour, when the fleet sailed *with* the tide along the coast; the result goes to establish that the Romans must have travelled *northward*, eventually landing in the Downs (probably near Deal or Walmer). On the way, according to Dion Cassius, they rounded a promontory: this would probably be the South Foreland. Halley found confirmation of this conclusion in the fact that, when Caesar landed at almost the same place in the following year, he sailed thither 'under a gentle south-west wind'. Had he not been making for a destination more northerly than the South Foreland, he could not have thought such a wind proper for his passage, since in those times a ship could hardly sail nearer to the wind than eight points.[1]

It may be convenient to summarize at this point some of Halley's later essays in classical scholarship.

In 1685 J. Hardouin produced his Paris edition of Pliny's *Historia Naturalis*, which marked an advance on those previously available. Halley, however, suspected a few of the readings, and he published his suggested emendations (*Phil. Trans.* (1691), *17*, 535ff.). Of particular interest is his correction of a passage relating to the Moon's motion. Reading in Pliny that eclipses recur after 222 months, he correctly altered the period to 223 months after which, in fact, the Moon returns to roughly the same position with regard both to the Sun and to the lunar node

[1] For an exhaustive discussion of the problem, see T. Rice Holmes, *Ancient Britain and the Invasions of Julius Caesar*, Oxford, 1907, 595ff. The conclusions are that 26 August is the probable and 25 or 27 August in 55 B.C. are possible dates of Caesar's first landing, which occurred in the locality indicated by Halley, the scene of the second invasion being near to but not necessarily identical with that of the first.

on the ecliptic. However, he connected the passage with a statement by the encyclopaedist Suidas (*c.* A.D. 1000) that a 'Saros' (described as a Chaldaean measure) contains 222 months. Suidas was not referring to eclipses, but Halley (supposing him to have been misled by the reading in Pliny) mistakenly attached the word 'Saros' to this important lunar period after which the sequence of eclipses had been found (supposedly by the Babylonian astronomers) to repeat itself predictably in roughly the same order; and this confusion has persisted despite the protests of scholars (see O. Neugebauer, *The Exact Sciences in Antiquity*, second edition, Providence, 1957, 141f.).

At the request of his former school friend and fellow-traveller Robert Nelson, Halley briefly summarized the elaborate account of the Greek and Roman systems of chronology drawn up by Henry Dodwell, the seventeenth-century scholar and theologian, in his book *De Cyclis*.[1] Dodwell contributed a series of learned dissertations to a collection of Greek geographical texts edited and translated into Latin by John Hudson, classical scholar and Bodley's Librarian (*Geographiæ Veteris Scriptores Graeci Minores*, 4 vols., Oxoniae, 1698-1712). To the third volume of this collection Halley contributed a restored text of Ptolemy's star catalogue : Hudson was confident that the inclusion of this somewhat extraneous document would be excused by anyone 'who knows how many blots the learned Halley has expunged from these stars and how much light he has shed upon them, the same, indeed, as that by which they shone when Ptolemy contemplated them' (*Praefatio*).

In this same field of ancient lore, Halley sought to render a last service to Newton's memory by coming to the defence of his new and original system of chronology which, after circulating for some years in unauthorized drafts, was published posthumously in 1728 under the title *The Chronology of Ancient Kingdoms Amended.* At the request of Caroline of Anspach, the studious Princess of Wales, Newton had drawn up an abstract of his system; later, and again at her wish, he allowed the Abbé Conti to make a copy of this document for his own private use. Through

[1] *An account of Mr. Dodwell's Book De Cyclis in a Letter to Robert Nelson Esq.*, London, 1715; printed in F. Brokesby, *The Life of Mr. Henry Dodwell* etc., 2 vols., London, 1715, ii, 611ff.

some breach of confidence the manuscript was translated into French and printed, with an adverse commentary. Newton published a protest and defence. However, in 1726 a further attack was made upon Newton's chronology in five virulent dissertations composed by the Jesuit Father Étienne Souciet. Meanwhile Newton was drawing up his formal treatise on the system; but he died before this could be published. Two of Souciet's dissertations dealt with astronomical questions; and Halley felt it laid upon him as Astronomer Royal, and Newton's old friend, to take up the challenge, dealing with the technicalities of the controversy in two papers (*Phil. Trans.* (1727), 34, 205ff.: 35, 296ff.). Only to the student of Newton or of eighteenth-century thought can this long out-dated chronology be of any interest today.

4 *A Table of Mortality*

The seventeenth century saw the earliest serious application of statistical methods to the investigation of social phenomena. One manifestation of this interest in what came to be called 'political arithmetic' was the construction of mortality tables designed to show how the deaths occurring in a community are distributed among the various age-groups of the population. These tables were mainly based upon the 'bills of mortality', periodical returns of the numbers of deaths or burials in a city. The bills for London and for Dublin were closely studied by John Graunt and his friend and collaborator William Petty, the latter a foundation member of the Royal Society. In 1662 Graunt published a mortality table based upon the partly fanciful assumptions that, of children born, 36 per cent died before completing their sixth year, that perhaps one per cent reached the age of 76, and that in each of the intervening seven decades the population was reduced in the same proportion. Edmond Halley was another of the pioneers in the statistical investigation of the relation between mortality and age in a normal population. He showed how a table of mortality could serve as a scientific basis for life insurance, which previously had been conducted as a game of chance in which little account was taken of the ages of the participants. His reflections on the problem were presented to the Royal Society in March 1693, and they were subsequently published in the *Philosophical Transactions* (1693, *17*, 596ff.).

'The Contemplation of the Mortality of Mankind [we read] has besides the Moral, its Physical and Political Uses'. Halley found the bills of mortality for London and Dublin inadequate for his purpose. The ages of those dying were not specified; the total populations were not known; and both cities attracted a 'great and casual Accession of Strangers', as was shown by the large excess of deaths over births. However, it chanced that Henri Justel, a Protestant refugee from Paris, had recently communicated to the Royal Society certain bills of mortality for

7 . 8 9 . . 14 . 18 . 21 . 27 . 28 . . 35 .
11 . 11 . 6 . $5\frac{1}{2}$. 2 . $3\frac{1}{2}$ 5 6 $4\frac{1}{2}$ $6\frac{1}{2}$ 9 . 8 . 7 . 7 .

36 . 42 . 45 49 54 . 55 . 56 . 63
8 . $9\frac{1}{2}$ 8 . 9 . 7 . 7 . 10 11 . 9 . 9 . 10 . 12

70 71 . 72 77 81 84 . 90 91 .
$9\frac{1}{2}$. 14 9 . 11 $9\frac{1}{2}$ 6 . 7 . 3 . 4 . 2 . 1 . 1 . 1 .

98 . 99 . 100.
0 . $\frac{1}{5}$. $\frac{3}{5}$

Fig. 18 Incidence of mortality in Breslau.

the German city of Breslau covering the five years 1687-91. The information seems to have been supplied to Justel by a clergyman of Breslau, Caspar Neumann by name, from whom Halley later sought to elicit further information about the vital statistics of Breslau (*Correspondence*, 88f.; *Notes and Records of the Royal Society* (1938), *1*, 70ff.). These bills gave monthly returns of deaths, stating age and sex, together with the number of births, which only slightly exceeded the deaths. The total population was not given. Over the five years, Halley found an average annual birth-rate of 1,238, of which 348 died in their first year and 198 between the ages of one and six, leaving 692; the average annual death-rate was 1,174. The average numbers dying at each (current) age according to the bills of mortality were then given

by the table in Fig. 18; where a dot occurs, the number below signifies the number of deaths for each year between the ages in the preceding and the following columns.

The figures were irregular because of the shortness of the period covered; but they served to show how mortality in middle life at first increased with age, 'they growing more crasie', and then decreased as ever fewer persons remained to die. Adopting these data as a basis and smoothing out irregularities, Halley drew up a second table 'whose Uses are manifold, and give a more

Age. Curt.	Per-ſons.	Age. Curt.	Per-ſons	Age. Curt.	Per-ſons	Age. Curt.	Per-ſons	Age. Curt.	Per-ſous	Age. Curt.	Per-ſons
1	1000	8	680	15	628	22	586	29	539	36	481
2	855	9	670	16	622	23	579	30	531	37	472
3	798	10	661	17	616	24	573	31	523	38	463
4	760	11	653	18	610	25	567	32	515	39	454
5	732	12	646	19	604	26	560	33	507	40	445
6	710	13	640	20	598	27	553	34	499	41	436
7	692	14	634	21	592	28	546	35	490	42	427
Age. Curt	Per-ſons.	Age. Curt.	Per-ſons	Age. Curt.	Per-ſons	Age Curt	Per-ſons	Age. Curt.	Per-ſons	Age. Curt.	Per-ſons
43	417	50	346	57	272	64	202	71	131	78	58
44	407	51	335	58	262	65	192	72	120	79	49
45	397	52	324	59	252	66	182	73	109	80	41
46	387	53	313	60	242	67	172	74	98	81	34
47	377	54	302	61	232	68	162	75	88	82	28
48	367	55	292	62	222	69	152	76	78	83	23
49	357	56	282	63	212	70	142	77	68	84	20

Age.	Perſons.
7	5547
14	4584
21	4270
28	3964
35	3604
42	3178
49	2709
56	2194
63	1694
70	1204
77	692
84	253
100	107
	34000
	Sum Total.

Fig. 19 Halley's mortality table.

just Idea of the State and Condition of Mankind, than any thing yet extant that I know of'. It claims to show the number of people of each age living in Breslau: the numbers add up to nearly 34,000, representing the estimated total population of the city (Fig. 19).

Halley goes on to explain the various uses to which a mortality table can be put. He finds the number of men of military age in Breslau by adding the age-groups (of both sexes) between 18 and 56 inclusive, which amount to 18,053, and halving this figure to obtain the number of 'Fencible Men'; it represents about a quarter of the total population. Secondly, the table serves to show the degrees of mortality (or of vitality) at all ages, 'for if the number of Persons of any Age remaining after one year, be divided by the difference between that and the number of the Age proposed, it shews the odds that there is, that a Person of that Age does not die in a Year'. It would appear from the table that, for a person of 25, the odds are 560 to 7, or 80 to 1, that he does *not* die within a year.

Again, the table shows the period within which a person of a given age has an even chance of dying and of not dying; his 'expectation of life', as we should say. It is the period in which the number of living persons of the age proposed is halved; thus a person of 30 has an expectation of 27 or 28 years. In the purchase of annuities 'the Purchaser ought to pay for only such a part of the value of the Annuity, as he has Chances that he is living; and this ought to be computed yearly, and the Sum of all

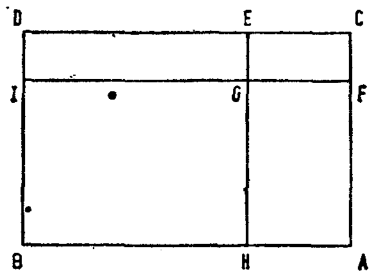


Fig. 20 Determination of chances involving two lives.

those yearly Values being added together, will amount to the value of the annuity for the Life of the Person proposed.' Halley drew up a table showing the value of an annuity in years' purchase for every fifth year of age from one to seventy. For a joint annuity in which two lives are involved 'the number of Chances of each single Life, found in the Table, being multiplied together, become the Chances of the two Lives.' Halley recommended the use of geometrical diagrams in the solution of such problems. In Fig. 20 'AB or CD represents the number of persons of the younger Age, and DE, BH those remaining alive after a certain term of years; whence CE will answer the number of those dead in that time: So AC, BD may represent the number of the Elder Age; AF, BI the Survivors after the same term; and CF, DI, those of that Age that are dead at that time. Then shall the whole Parallelogram ABCD be . . . the Product

of the two Numbers of persons . . . and . . . after the Term proposed the Rectangle HD shall be as the number of Persons of the younger Age that survive, and the Rectangle AE as the number of those that die. So likewise the Rectangles AI, FD shall be as the Numbers, living and dead, of the other Age' etc. In extending this technique to actuarial problems involving three lives, Halley had recourse to a three-dimensional figure, a parallelepiped.

Halley laid firm and lasting foundations for the theory of annuities and life insurance. His mortality table, too, long continued in use, even after better ones had become available, such as that of A. Deparcieux (1746). Its serious imperfections arose partly from the assumption that the population in question was a stationary one, whereas this was not strictly so; and the successive age-groups considered were composed of persons born at periods increasingly remote from that covered by the bills of mortality. Halley was aware that differences in the salubrity of places might be urged against adopting the Breslau tables as a universal standard; but 'at least 'tis desired that in imitation hereof the Curious in other Cities would attempt something of the same nature, than which nothing perhaps can be more useful.'

Halley's paper on the Breslau Bills of Mortality was reprinted in his *Miscellanea Curiosa* of 1705-7, an account of which will be given in Chapter 11. To the original text he added some further considerations bearing upon current social problems and reflecting his attitude towards these.

'Besides the Uses mentioned [he writes], it may perhaps not be an unacceptable thing to infer from the same Tables, how unjustly we repine at the Shortness of our Lives, and think our selves wronged if we attain not old Age; whereas it appears hereby, that the one half of those that are born are dead in Seventeen Years Time, 1238 being in that time reduced to 616. So that instead of murmuring at what we call an untimely Death, we ought with Patience and Unconcern to submit to that Dissolution which is the necessary Condition of our perishable Materials, and of our nice and frail Structure and Composition: And to account it as a Blessing that we have survived, perhaps by many Years, that Period of Life, whereat the one half of the whole Race of Mankind does not arrive.

'A second Observation I make upon the said Table, is that the Growth and Increase of Mankind is not so much stinted by any thing in the Nature of the Species, as it is from the cautious difficulty most People make to adventure on the State of Marriage, from the Prospect of the Trouble and Charge of providing for a Family. Nor are the poorer sort of People herein to be blamed, since their difficulty of subsisting is occasion'd by the unequal Distribution of Possessions, all being necessarily fed from the Earth, of which yet so few are Masters. So that besides themselves and Families, they are yet to work for those who own the Ground that feeds them: And of such does by very much the greater part of Mankind consist; otherwise it is plain, that there might well be four times as many births as we now find, For by Computation from the Table, I find that there are nearly 15,000 Persons above 16 and under 45, of which at least 7,000 are Women capable to bear Children. Of these notwithstanding there are but 1,238 born yearly, which is but little more than a sixth part: So that about one in six of these Women do breed yearly; whereas were they all married, it would not appear strange or unlikely, that four of six should bring a Child every year. The Political Consequences hereof I shall not insist on; only the Strength and Glory of a King being in the Multitude of his Subjects, I shall only hint, that above all things, Celibacy ought to be discouraged, as, by extraordinary Taxing and Military Service: And those who have numerous Families of Children to be countenanced and encouraged by such Laws as the *Jus trium Liberorum* among the Romans. But especially, by an effectual Care to provide for the Subsistence of the Poor, by finding them Employments, whereby they may earn their Bread, without being chargeable to the Publick' (*Miscellanea Curiosa*, i, 301ff.).

In 1692 John Houghton, a writer on agriculture and trade, started a periodical appearing as a weekly folio sheet at the price of one penny. It ran for nineteen years and covered, at a popular level, a great range of topics selected from contemporary science, natural history, technology, agriculture, economics, and much else besides (*A Collection for the Improvement of Husbandry and*

Plate 9a
Johannes Hevelius
(*Crown Copyright reserved, Science Museum, London*)

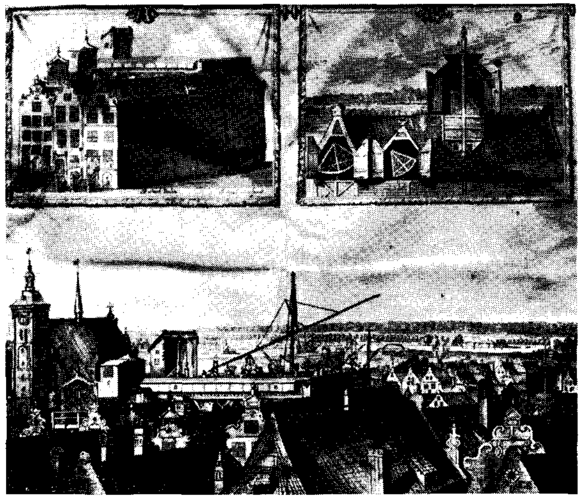


Plate 9b
Hevelius's observatory at Danzig
(*Science Museum, London*)

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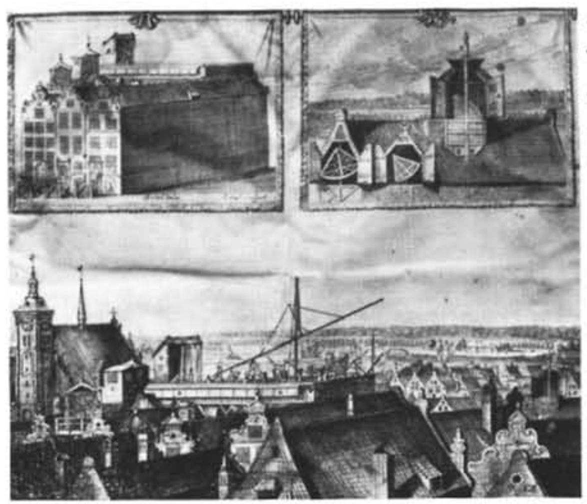

Plate 9b
Hevelius's observatory at Danzig
(*Science Museum, London*)

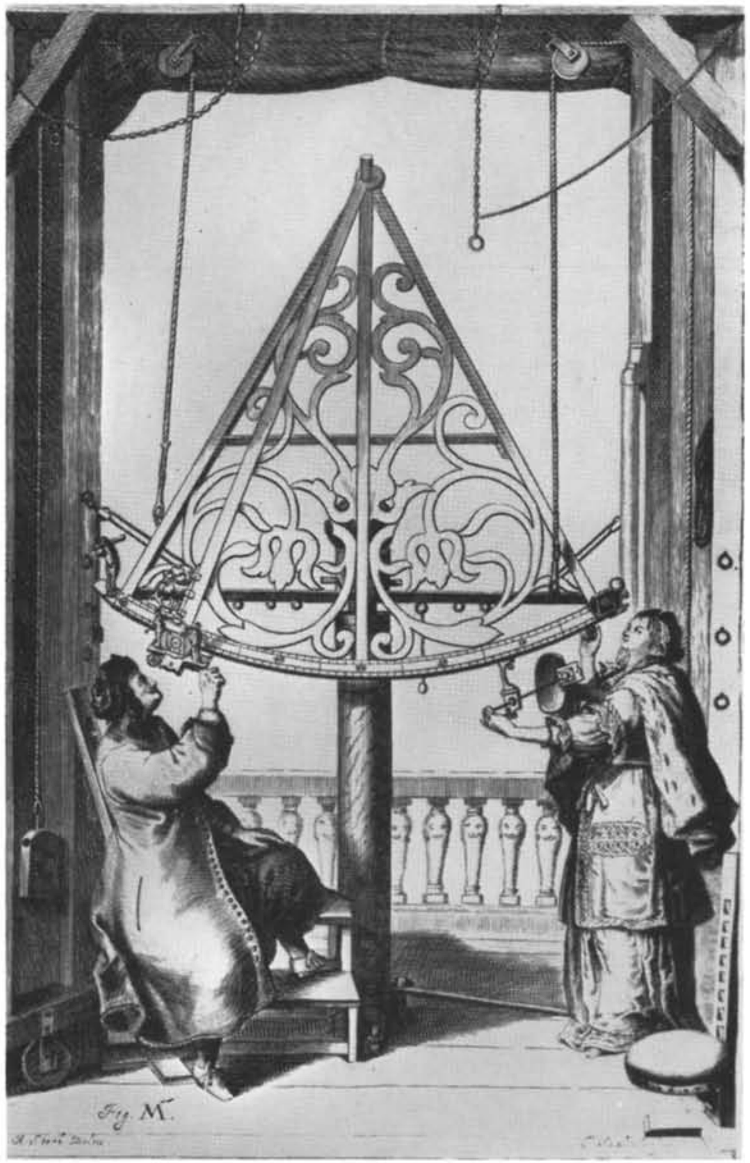

Plate 10 Johannes and Elizabetha Hevelius observing with the great bronze sextant (*Science Museum, London*)

Trade, ed. R. Bradley, 4 vols., London, 1727-8). Houghton, a Fellow of the Royal Society, drew much of his information from other Fellows; and he relied not a little upon the counsel of his 'very ingenious friend' Edmond Halley. In No. 24 of the *Collection* (20 January 1693) he refers to his recently published sixpenny sheet giving 'An Account of the Acres and Houses, with the Proportional Tax &C. of each County in England and Wales (1693)'. For this information he was largely indebted to Halley, 'whom I find always very ready to advise or assist in any thing that may do his country service'.

About ten years earlier Houghton had asked Halley to estimate the acreage of England and Wales; and he goes on to quote the astronomer's reply (*Collection*, i, 67ff.). From Adams's sheet map of England, Halley cut out the portion which represented the land area (including the islands of Wight, Anglesey, and Man) and weighed it 'in nice scales'. He then cut out from the middle of the map a circle having a diameter of 2° of the meridian (or 138⅗ miles, according to the best available estimate) which lay wholly within the land area. He found that the land map weighed just four times as much as the circle, which would give England and Wales an area of about 38,600,000 acres, assuming that 'a million or two of acres will break no squares' (a modern estimate: 37,300,000 acres). The next issue of the *Collection* gives the acreage of each county, which Halley obtained in a similar manner, using Saxton's map for the purpose. For greater accuracy he dried the paper cuttings before weighing them, but he found that they quickly re-imbibed their original moisture.

In a paper on sheep-rot (No. 34), Houghton writes:

I am informed by my good friend Mr. Edmond Halley, that at St. Helena (where he lived a year) sheep are very fertile, and commonly have each two or three lambs yearly, but they are there very short liv'd, dying of this rot; which he takes to be the effect of the over great moisture on their hills, which are half a mile high, and so moist, that paper in the night could not be kept dry enough to write on (ibid., i, 98).

In No. 39 there is a table (for which Houghton was again indebted to the astronomer) showing how the number of ounces in a penny loaf should be related to the price of corn, allowing a stated balance to the baker (ibid., i, 111).

5 Halley at Chester

In the closing years of the seventeenth century a halt was called to the scandalous debasement of the English silver coinage, the cause of great hardship and unrest among the traders and wage-earners of the time. In 1696 the Chancellor of the Exchequer, Charles Montague, persuaded Parliament to enact the compulsory re-coinage of all hand-struck silver coins according to the former standards of weight and fineness. The new coins were to be milled and the whole cost was to be borne by the Treasury, provided the old pieces were handed in by a stated date. Montague, the patron of Newton, appointed him to be Warden of the Mint, rightly estimating his friend's administrative ability, which contributed not a little to the success of this hazardous financial operation.

It fell to Newton to appoint officials at each of the five temporary mints set up in provincial centres; and Halley was made Deputy Comptroller of the Mint at Chester, where he suffered much vexation. It seems that Newton wrote to him (21 June 1697)

> about tightening up the examination of coins before issue. . . . The Deputy Master, Clarke, evaded these directions; he also issued coin out of their turn to persons who had brought silver to the Mint. The other two Deputies, Weddell and Halley, stymied him by locking all coin in the safe. Clarke turned on his colleagues. After an exchange of Billingsgate in the public office, Weddell spat in his face; Clarke challenged Weddell to a duel, but did not await his arrival on the challenge field; then he hastened to London to pull strings, while Halley called in Newton's influence to have the 'proud insolent fellow' removed. . . . The clerks at Chester were bidden to cease affronting their superiors by wearing hats and swords in the Mint Office, as in London only upper staff were allowed to appear on the premises in full kit (Sir John Craig, *Newton at the Mint*, Cambridge, 1946, 14f.).

Meanwhile, Newton, at the Royal Mint, which then formed part of the Tower of London, was having to cope with intrusions from the undisciplined soldiers of the garrison. It must have been a

relief to Halley when, in 1698, the Chester Mint closed and he was released to resume his normal life. However, at Chester he had found matter for several papers illustrating the wide variety of his interests.

Halley's interest in the antique world is reflected by the account which he wrote while at Chester of a Roman altar, or votive tablet, which had been excavated there several years earlier (*Phil. Trans.* (1696), *19*, 316ff.). This object (measuring 32 by 16 by 9 inches) bore an inscription (which Halley partly transcribed and dated as late Empire) as well as carvings depicting a genius with cornucopia, a flower pot, foliage and the face of a man wearing a cap. The tablet was of local stone, a soft grit intermixed with shining particles. This material stood up badly to the weather; walls built of it were soon eroded and had to be repaired by officials called 'murengers'. Halley had diligently examined specimens of the rock for shells and other animal remains, but so far without success.

The spring of 1697 appears to have been unusually tempestuous; and No. 229 of the *Transactions* contains several accounts of destructive hailstorms. Halley describes such a storm which approached Chester on 29 April (*Phil. Trans.* (1697), *19*, 570ff.). Coming up from Carnarvonshire, it passed near Snowdon 'with a horrid black Cloud'; thereafter it crossed Denbighshire and Flintshire, skirted the Wirral Peninsula, and so to Ormskirk and Blackburn in Lancashire. The hailstones, which weighed up to five ounces, mostly consisted of a snowy kernel surrounded by hard, transparent ice. In the track of the storm, which was about two miles in width, the hail broke windows, ploughed up the soil and cut down the growing corn; it killed poultry, lambs, and 'a stout Dog'. Halley attributed the storm to a 'Vapour that disposed the Aqueous Parts thus to congeal'; but he wondered how this vapour could continue undispersed for a distance of more than 60 miles.

In 1697, at Whitsuntide, Halley made an excursion into North Wales (*Phil. Trans.* (1697), *19*, 582ff.). On 26 May he read the barometer on the summit of Snowdon, the highest mountain in south Britain; the mercury stood at 26·1 inches. At Llanberis, near the foot of the mountain, the reading that same evening was 29·4 inches; and at Carnarvon the following day, at sea level, it was 29·9 inches. Observations at Llanerch, near St Asaph, showed the pressure to have been nearly constant during four days covering Halley's determinations. Hence the difference of pressure between summit and sea level could be taken as about 3·8 inches. The height of the mountain had been determined trigonometrically by John Caswell, an Oxford mathematician and geodesist: he had estimated it at 3,720 feet, 'which abating the height of the Mercury 3 Inches, 8 Tenths, may serve for a Standard 'till a better be obtained on a higher place'. Halley's formula connecting height with barometric pressure would have given the height of Snowdon as 3,773 feet; the modern estimate is 3,560 feet.

Halley described Snowdonia as a 'horrid spot of Hills, the like of which I never yet saw'. From the summit he could descry Ireland and the mountains of Cumberland and Westmorland, as well as many lakes abounding with trout, which he sampled and found to his taste. He embarked on a 'floating Island', a large clump of turf which had broken off from the bank of one of the lakes.

Halley observed an eclipse of the Moon at Chester on 19 October 1697, under ideal conditions; and he timed the various phases of the phenomenon in the hope that the observation might serve for determining the longitude of the city (*Phil. Trans.* (1697), *19*, 784).

6 *Visit of Peter the Great*

In 1697 the Tsar of Muscovy, to be known to future generations as Peter the Great, set out on a tour of Western Europe. It was chiefly his intention to study shipbuilding and seamanship, for he had determined to create a navy and to challenge the Turkish command of the Black Sea. Peter spent several months in Holland, toiling as a journeyman in the shipyards; but he found that the Dutch worked by rule of thumb, and he preferred the look of English-built ships. Hence he welcomed an invitation from King William III to visit England; and he reached London early in 1698, being then in his twenty-sixth year. He soon took up his abode at Sayes Court, the manor house of Deptford and country home of John Evelyn the diarist; it adjoined the Royal Docks,

where the Tsar worked with his own hands. He spent his evenings smoking and drinking beer at a tavern in Great Tower Street or partaking of hot pepper and brandy with his mentor, the Marquis of Carmarthen.[1] Tsar Peter visited the Royal Observatory and the Royal Society; and he made a point of meeting Halley:

> When Peter the Great, Emperor of Russia, came into England, he sent for Mr. Halley, and found him equal to the great character he had heard of him. He asked him many questions concerning the fleet which he intended to build, the sciences and arts which he wished to introduce into his dominions, and a thousand other subjects which his unbounded curiosity suggested; he was so well satisfied with Mr. Halley's answers, and so pleased with his conversation, that he admitted him familiarly to his table, and ranked him among the number of his friends (*Biographia Britannica*, loc. cit., 2517).

The Tsar seems to have persisted in a boyish pastime of wheeling members of his entourage, or being wheeled by them, through hedges in a wheelbarrow. He ruined Evelyn's magnificent garden and much-prized holly hedge by these diversions; indeed, the damage to Sayes Court, which included 300 window panes broken and many fine pictures destroyed, was officially assessed at over £300. The legend that Tsar Peter trundled Halley through a hedge, though in character, seems to lack any historical foundation.

[1] See Ian Grey, 'Peter the Great in England', *History Today* (1956), *6*, 225ff.

Chapter 10

Halley at Sea

1 The Atlantic Voyages

WE come now to the most dramatic episode in Halley's eventful life, his career as a naval officer commanding a man-of-war on a grand tour of the Atlantic Ocean. Only rarely in our naval history can such a commission have been held by a landsman. Not that Halley was unused to the sea and its ways. As a boy he may well have conversed with the mariners who came to buy ship's stores from his father. His expedition to St Helena had involved spending months on board ship; doubtless he had shared in the daily routine of navigation. He had experimented with diving-bells and had used them in salvage operations. He had written papers on tidal phenomena and had (as we shall see) charted the Thames Estuary and the Sussex coast and advised Samuel Pepys on nautical matters. He had improved the Davis's Quadrant and the azimuth compass. He had also invented a new type of log for measuring the way of a ship (its speed through the water), his device consisting of a ball of brass let down into the sea on a line from whose inclination to the vertical he claimed to be able to calculate theoretically the speed of the ship (*Correspondence*, 227).

Halley's voyages were foreshadowed by negotiations and preparations extending over several years. Curiously enough, the prime mover in the enterprise appears not to have been Halley himself but one Benjamin Middleton, of whom nothing is known with certainty except that he was elected F.R.S. in 1687. On 12 April 1693 Middleton asked for the assistance of the Royal Society in procuring for him a vessel of about 60 tons to be fitted out by the Government but manned and victualled at his own expense for a voyage to study magnetic variation and to circumnavigate the globe (*Correspondence*, 186). Hooke's Diary (of

1688-93) records, already on 11 January 1693, how 'Halley [spoke] of going in Middleton's ship to discover'—we are not told what; and then, on 12 April following (the day of the Royal Society meeting at which the matter was raised), 'Halley and Middleton made proposalls of going into the South Sea and round the World' (R. T. Gunther, *Early Science in Oxford*, Oxford, 1923 etc., x, 205, 230).

The Admiralty ordered the Navy Board to build and fit out a suitable craft and to deliver it to Middleton for his temporary use. The ship, built at Deptford in 1694 by Fisher Harding, the Master Shipwright, was of the kind known as a *pink*, three-masted, flat-bottomed and with bulging sides and a narrow stern. She was 64 feet long, 18 feet in beam, 7 ft 7 ins. in draught, and her displacement was 89 tons; she was named the *Paramour* and is often loosely referred to as the *Paramour Pink*. There followed two years of unexplained delay. Preparations for the voyage were resumed in June 1696; but the initiative had now passed to Halley, who was commissioned as the master and commander of the *Paramour*, while Middleton's name fades out of the record. 'I had waited on you on Saturday, but I was obliged to go on board my frigatt,' wrote Halley to Newton, probably in 1696 (*Correspondence*, 96). The re-coinage, resulting in Halley's appointment to the Mint at Chester, caused a further two years' delay; but by 1698 he was free once more. It had originally been intended that the expedition should be the responsibility of the Royal Society. The Admiralty (who regarded the ship as 'going on a private affair') were to enlist the crew in the usual manner (this would contribute to the maintenance of discipline); but Halley was to be responsible for paying the officers, and he had to give a security for a year's wages (£340 17s. 4d.) for the whole ship's company. He named Sir John Hoskyns, the Vice-President of the Royal Society, who gave a bond for £600. However, by the time the *Paramour* was ready to sail, the enterprise had passed under the patronage of King William III; guns had been mounted on the vessel, and the Royal Society were no longer responsible for the expenses. The ship was victualled for twelve months and she carried a crew of 20 men.

Halley's lieutenant on his first voyage was one Edward Harrison, an efficient naval officer of eight years' service but an unfortunate choice in the circumstances. He resented serving under a captain with no experience of command at sea. He also blamed Halley for the cool reception accorded to a paper on the problem of the longitude which he had submitted to the Royal Society in 1694 and which he had since (but again to little purpose) expanded into a book, *Idea Longitudinis* (London, 1696). Halley did not realize the identity of his lieutenant, and the cause of Harrison's animosity towards him, until it was too late, and in consequence his first venture into the Atlantic had to be prematurely terminated, as we shall see.

On 15 October 1698 Halley received from the Admiralty a set of instructions (in the framing of which he had had a considerable say) bidding him proceed south of the equator to observe the variation of the compass in the South Atlantic, to determine the longitude and latitude of all ports and islands visited, to attempt the discovery of whatever land lay to the south of the Atlantic, and, on the return voyage, to visit the English West Indian plantations. Halley published no formal account of his expeditions, but his Journals have been preserved, and they were first printed by Alexander Dalrymple, a distinguished eighteenth-century hydrographer, in his *Collection of Voyages chiefly in the Southern Atlantick Ocean published from original MSS.* (London, 1775). Supplementary to Halley's Journal are the letters he wrote, in the course of his voyages, to Josiah Burchett, Secretary to the Admiralty (*Correspondence*, 103ff. and quoted here by permission of the Public Record Office).

We read that Halley sailed from Deptford on 20 October 1698. He had barely left Margate when he ran into severe and prolonged gales, and he soon 'found the weakness of my Crew, and that our Vessel was very leewardly'. On the 30th they passed the Isle of Wight, 'but it being covered with Snow, some that ought to have known it better took it for Portland'. By now the ship had become leaky, and the pumps brought up quantities of the sand ballast. From Portland, Halley wrote to inform the Admiralty of his plight, asking for orders to be sent to Portsmouth and to Plymouth (he was uncertain which port he would make) for the ship to be examined and for the sand ballast to be changed for shingle. Anchored off Spithead on 4 November, they fired five guns and hung out all colours for the King's birthday;

and on the morrow, Guy Fawkes' day, 'we fired five guns for Powder Treason'. In Portsmouth dock from 10 to 15 November, the ship received all needed attention; and on the 22nd they sailed out to join Admiral Benbow's squadron, 'saluting him with five pieces, and he returned me as many'. They sailed in company with the squadron as far as Madeira; there they parted from the Admiral and 'I went ashore in order to get my Wine,' though that operation was delayed for four days 'on account of the Holidays and the great Suff of the Sea'. Heading for the Cape Verde Islands, they ran into a colony of jellyfish: 'We passed through a streak of Water in appearance turbid, but when in it we took up some of the Water and it was full of small transparent globules somewhat less than white peas, their substance appeared like that of our Squids or Urtica Marina . . . there were two or three sorts of them'. At Santiago, in the Cape Verde Islands, Halley's ship was fired on by two English merchantmen who mistook her for a pirate.

Early in the new year Halley reports the first sign of trouble with his crew. As they bore down on the equatorial island of Fernando Noronha, 'this morning [18 January] between two and three looking out I found that my Boatswain, who had the Watch, steered away NW instead of W. . . . I conclude with a design to miss the Island, and frustrate my Voyage, though they pretended the candle was out in the Bittacle and they could not light it'. They made the uninhabited island and Halley landed, finding there only green turtles, land crabs, and turtle doves; then, being short of water, they called at Pernambuco. The southern winter was now approaching; and 'my Officers showing themselves uneasy and refractory, I this day [17 March] chose to bear away for Barbadoes in order to exchange them if I could find a flag there'. They sighted Barbadoes on 1 April.

> My Lieutenant then having the Watch clapt upon a wind, pretending that we ought to go to windward of the Island, and about the North End of it, whereas the road is at the most Southerly part almost; He persisted in this Course which was contrary to my orders given overnight, and to all sense and reason, 'till I came upon deck; when he was so far from excusing it, that he pretended to justify it, not without reflecting language.

On 9 May Halley started his homeward voyage, 'finding it absolutely necessary to change some of my Officers, which I found I could not do, without returning into England'.

Back in England, Halley informed the Admiralty that the reason for his speedy return was

> the unreasonable carriage of my Mate and Lieutenant, who, because perhaps I have not the whole Sea Dictionary so perfect as he, has for a long time made it his business to represent me, to the whole Shipps company, as a person wholly unqualified for the command their Lordships have given me. . . . He was pleased so grossly to affront me, as to tell me before my Officers and Seamen on Deck, and afterwards owned it under his hand, that I was not only uncapable to take charge of the Pink, but even of a Longboat; upon which I desired him to keep his Cabbin for that night, and for the future I would take the charge of the Shipp my self, to shew him his mistake: and accordingly I have watcht in his steed ever since, and brought the Shipp well home from near the banks of Newfound Land, without the least assistance from him (*Correspondence*, 107f.).

Lieutenant Harrison appeared before a court-martial which reprimanded him but which showed but little regard for Halley; it 'very tenderly styled the abuses I suffered . . . to have been only some grumblings such as usually happen on board small Shipps' (ibid., 109). Harrison left the Royal Navy and entered the merchant service. On his second voyage Halley was careful not to take a lieutenant; on that account, and because he had been allotted a one-armed boatswain, he asked to be allowed to include three or four more seamen in his crew.

It was on 16 September 1699 that Halley set sail again from Deptford. There were reports of a 'Sally man' (Moroccan pirate) in the Channel; and the *Paramour* sailed with a consort, the *Falconbird*, as far as Madeira. Here they had hoped to take on supplies; but the weather was stormy, and 'our people chose rather to go without their wine than to lie beating it, so much in danger of the Sally Rovers'. As they headed for the Canary Islands, 'my poor boy Manley White had the misfortune to be drowned, falling overboard'. They crossed the equator on 16 November; and on 9 December 'we fell into such a smooth, notwithstanding it blew a stout gale at NNW, that we concluded we were gotten under the shelter of the land, and about 7 in the morning we all smelt a very fragrant smell of flowers, which the wind brought off the land, and several Butterflies flew on board us'. The following day they sighted Cape St Thomas on the Brazilian coast, and on the 13th the 'Sugar Loaf' at the entrance to Rio de Janeiro. As they pursued their course towards the far south, unfamiliar forms of life met their eyes.

> Last night [20 January 1700] the Sea appeared very white and abundance of small Sea Fowl about the Ship, and several beds of weeds drove by the Ship, of which we took up some for a sample being what none of our people had seen elsewhere. . . . The Alcatrosses which we first met with in about 27° South have now left us, we see no more of them.

On 25 January 'it was so cold as to be scarce tolerable to us used to the warm Climates'; and on 27 January, in latitude 50° 45′ S.,

> All this Morning we have had a great Fog, so have gone away with my fore topsail only, lowered down on the Cap, and sounded every two hours, apprehending myself near land; and the rather because yesterday and to-day several fowls, which I take to be Penguins, have passed by the Ship side, being of two sorts; the one black head and back, with white neck and breast; the other larger, and of the colour and size of a young Cygnet, having a bill very remarkably hooking downwards, and crying like a Bittern as they passed us. The Bill of the other was very like that of a Crow. Both swam very deep, and always dived on our approach, either not having wings, or else not commonly using them.

On 28 January

> We have had several of the Diving Birds with Necks like Swans pass by us, and this Morning a couple of Animals, which some supposed to be Seals, but are not so; they bent their Tails into a sort of a Bow . . . and being disturbed shewed very large fins as big as those of a large Shark. The head not much unlike a Turtle's.

The *Paramour's* course passed just south of the fifty-second parallel, and there the explorers sighted three large icebergs; one was 'of an incredible height', and the sailors christened it 'Beachy Head'. On 2 February

> we were in imminent danger of losing our Ship among the Ice, for the Fog was all the Morning long so thick, that we could not see a Furlong about us. When on a sudden a Mountain of Ice began to appear out of the Fog about 3 points on our Leebow; this we made a shift to weather,

when another appeared more on head, with several pieces of loose Ice round about it; this obliged us to tack, and had we mist stays, we had most certainly been ashore on it, and we had not been half a quarter of an hour under way, when another Mountain of Ice began to appear on our Leebow; which obliged us to tack again, with the like danger of being on shore; but the Sea being smooth and the gale fresh, we got clear; God be praised: this danger made my men reflect on the hazards we run, in being alone without a Consort, and of the inevitable loss of us all, in case we staved our Ship, which might so easily happen amongst these Mountains of Ice in the Fogs which are so thick and frequent here.

Steering northward, Halley sighted Tristan da Cunha on 17 February; thence he ran to St Helena for water. On the 26th, 'about 6 this morning a great Sea broke in upon our Starboard quarter, and withal threw us to, that we had like to have overset; the Deck being full of water, which had a clear passage over the Gunnel; but it pleased God she righted again. So we handed our Foresail and scudded a-hull'. Crossing now to the western side of the Atlantic they passed the islets called after Martin Vaz, their Portuguese discoverer, and reached the uninhabited volcanic island of Trinidad (to be distinguished from its West Indian namesake). Halley filled his casks from its abundant springs, throwing away the brackish St Helena water, and he 'put some Goats and Hogs on the Island for Breed, as also a pair of Guiney Hens I carried from St Helena. And I took possession of the Island in his Majesty's name, as knowing it to be granted by the King's Letters Patents, leaving the Union Flag flying'. Towards the end of the last century the island became a Brazilian possession; but a visitor to Trinidad reported in 1927 that 'wild goats and wild hogs liberated on the island in 1700 by the astronomer Halley, roamed the ridges' (*Notes and Queries* (1928), *154*, 152f.). On 21 April Halley sighted the coast of Brazil, and upon presenting himself to the Governor of Pernambuco he 'was informed that all was at peace in Europe, which I was most desirous to be assured of'. However, before he could leave the port he was arrested by the *soi-disant* English Consul on suspicion of being a pirate. His ship was searched and his sailors interrogated; but the episode ended amicably. At sea again, they noted many passing birds 'both Noddies and Men of War'. The next call was at Barbadoes, where, however, the Governor 'advised me to make no more stay here, than was absolutely necessary, by reason the Island had not been known so sickly as at present'. They departed in haste; but 'I found myself seized with the Barbadoes Disease, which in a little time made me so weak I was forced to take my Cabin. I ordered my Mate to shape his course for St. Christopher's [St Kitts]', where they lay up while the commander slowly recovered. So, by way of Anguilla and Sombrero, they came to Bermuda, where Halley took the opportunity to careen, scrub, and refit his vessel in readiness for the homeward voyage. The mate was given his discharge to take a better post. At the end of August they reached Newfoundland, narrowly escaping shipwreck in the fog. They were taken for pirates by the New England fishermen, 'one Humphrey Bryant, a Biddiford [Maine] man fired 4 or 5 Shot through our rigging, but without hurting us'. The homeward run was uneventful; on August 27 they anchored in Plymouth Sound; in the Downs Halley received the Lords Commissioners' orders 'to make the best of my way to Long-reach there to put out my Guns and so to Deptford to be laid up'. On 7 September 'I sent up my Gunner to give notice at the Tower of our Arrival'.

In the course of his voyages, Halley had determined the geographical locations of many places; he had visited unfrequented islands and observed unfamiliar phenomena, and he had collected the data he needed for the completion of his chart of the variation of the compass. The successive editions of the *English Atlas*, published in the early part of the eighteenth century by John Senex and his associates, contained a map of South America prepared with the advice of Halley and showing the route followed by the *Paramour* while in South Atlantic waters (R. P. Stearns, 'The Course of Capt. Edmond Halley in the Year 1700,' *Annals of Science* (1936), *1*, 294ff.).

In his Journal Halley recorded the estimated positions of his ship and the distances run from day to day. He determined latitudes and, where possible, longitudes (taking advantage of eclipses of Jupiter's satellites or of appulses of the Moon to stars). He noted temperatures, pressures, and the force and direction of the wind. And he took one or more magnetic observations nearly every day. In determining the variation of the compass, he would measure the Sun's *magnetic amplitude* (that is, the Sun's angular distance from the magnetic north measured round the horizon), say, at sunset, and again at sunrise the following morning. Half the difference of these two amplitudes represented the required variation, which was applied to the geographical position of the ship at midnight. When reliance had to be placed upon a single measurement of magnetic amplitude, compared with the Sun's computed azimuth from the *geographical* north, care was taken to allow for refraction and for the 'dip' of the apparent horizon when viewed from above the sea level, or to observe the Sun when on the true horizon; this was particularly important in high latitudes where the Sun's diurnal arc makes a small angle with the horizon. The Sun's magnetic amplitude was measured with an instrument known as an azimuth compass; this served for comparing the direction of the magnetic needle with that of the Sun, which cast the shadow of a thread upon the slit of an upright sight. Halley was credited with having improved the design of these instruments; and he had had two of them supplied to the *Paramour*. Halley's magnetic observations were analysed and re-computed by J. P. Ault and W. F. Wallis (*Terrestrial Magnetism* (1913), *18*, 126ff.). His determinations of the variation are estimated to have been reliable to one degree, or even to half a degree; and they compare well with those made on the iron-clad vessels of a later age.

2 *The Isogonic Charts*

For years Halley had been assiduously collecting recorded determinations of the variation of the compass from many parts of the world; he now had at his disposal a mass of additional data which he had secured in the course of his Atlantic voyages. He embodied all this magnetical material in two charts, constructed on a principle now familiar in physical geography but of which these are believed to have been the earliest printed examples. Curves were drawn through sets of places all having the same variation of the compass, so that the charts were inscribed with what, following Hansteen, we call 'isogonics'; Halley referred to them as 'Curve Lines' and his successors often called them 'Halleyan Lines'. These charts, reduced to the epoch 1700, were

published as separate sheets. The first to appear was soon forgotten, and it remained unknown to bibliographers until re-discovered by L. A. Bauer in 1895; it covers the Atlantic area (Pl. 11). The second, known since Halley's time, embraces the oceans of the world. The charts, which are on Mercator's projection, differ in the spellings of some place names, but the isogonics are identical. The Atlantic Chart also gives the course of the *Paramour*. Inferences as to the dates of publication have been drawn from the dedications. The Atlantic Chart is dedicated to King William III, who died on 8 March 1702. The World Chart was long assigned to 1701, but all known copies are dedicated to Prince George, Consort of Queen Anne, as Lord High Admiral and Generalissimo, a title he did not bear before 11 April 1702.

In the explanatory text attached to the World Chart we read:

> What is here properly New, is the Curve Lines drawn over the several Seas, to shew the degrees of the variation of the Magnetical Needle or Sea-Compass: Which are design'd according to what I my self found, in the Western and Southern Oceans, in a Voyage I purposely made at the Publick Charge, in the Year of our Lord 1700; or have collected from the Comparison of several Journals of Voyages lately made in the Indian Seas, adapted to the same year.

Halley added that continual slow changes in the variation would necessitate corrections; and, in fact, revised editions of the World Chart were published from time to time in the early eighteenth century. The Journal of the Royal Society for the years following Halley's return refers to several occasions when he showed the assembled Fellows a sea chart of magnetic variations, finally presenting it to the Society.[1]

In constructing the isogonics of the north Pacific Ocean, Halley had to rely upon a modicum of foreign observations; however, his conjectures as to the distribution of the curves were dramatically confirmed some years later when Anson captured a Spanish treasure ship on its way from Manila to Mexico, together with charts and the recorded magnetic observations of many years

[1] See G. Hellmann, *Neudrucke von Schriften und Karten* etc., Berlin, 1893 etc., No. 4; L. A. Bauer in *Terrestrial Magnetism* (1896), *1*, 28ff.: (1913), *18*, 113ff.; S. Chapman in *Occasional Notes of the Royal Astronomical Society* (1940), *1*, 122ff.

(J. B. Hewson, *A History of the Practice of Navigation*, Glasgow, 1951, 148).

3 *Charting the Seas*

On both his voyages Halley had been put to great inconvenience by the contrary winds that he encountered in the English Channel. He realized that this cause of delay could be circumvented in some measure by taking advantage of tidal currents where these were known; and he soon persuaded the Admiralty to place him once again in command of the *Paramour* with instructions (of his own framing) to survey the tides in the Channel.

The collection of sea-lore about the Channel, with its shoals and tides, must have begun with the earliest visits of ships from Gaul and Phoenicia to the southern coasts of England. Books of sailing directions, or 'Rutters' (Routiers), for the Channel were being printed at Rouen by the beginning of the sixteenth century. Since then the mapping of British coastal waters has been actively pursued in the interests of navigation and defence (A. H. W. Robinson, *Marine Cartography in Britain*, Leicester University Press, 1962). Already, on 3 July 1689, Halley, reporting to the Royal Society, had 'produced his Sea-draught of the Mouth of the River of Thames, wherein he saith, that he hath corrected several very great, and considerable faults in all our Sea-Carts hitherto published' (*Correspondence*, 215). Two entries in Hooke's Diary may refer to this enterprise: 22 March 1689, 'Halley a sayling'; 3 April 1689, 'Halley returned' (R. T. Gunther, op. cit., x, 108, 111). Again, it is recorded on 15 November 1693, that 'Halley produced his draught of the West coast of Sussex between Selsey and Arundell with the line form and situation of the dangerous sholes called the Owers' (*Correspondence*, 233). About this period Halley also produced (in 1691) a map of ancient Britain based upon the *Itinerary* of Antoninus and Ptolemy's *Geography*, and (in 1695) the draught of a survey of the New River contrived to bring water from Ware to London (ibid., 221, 236). Thus Halley was not inexperienced in land and sea cartography when he found himself again in command of the *Paramour*.

He was instructed to observe the course and the height of the

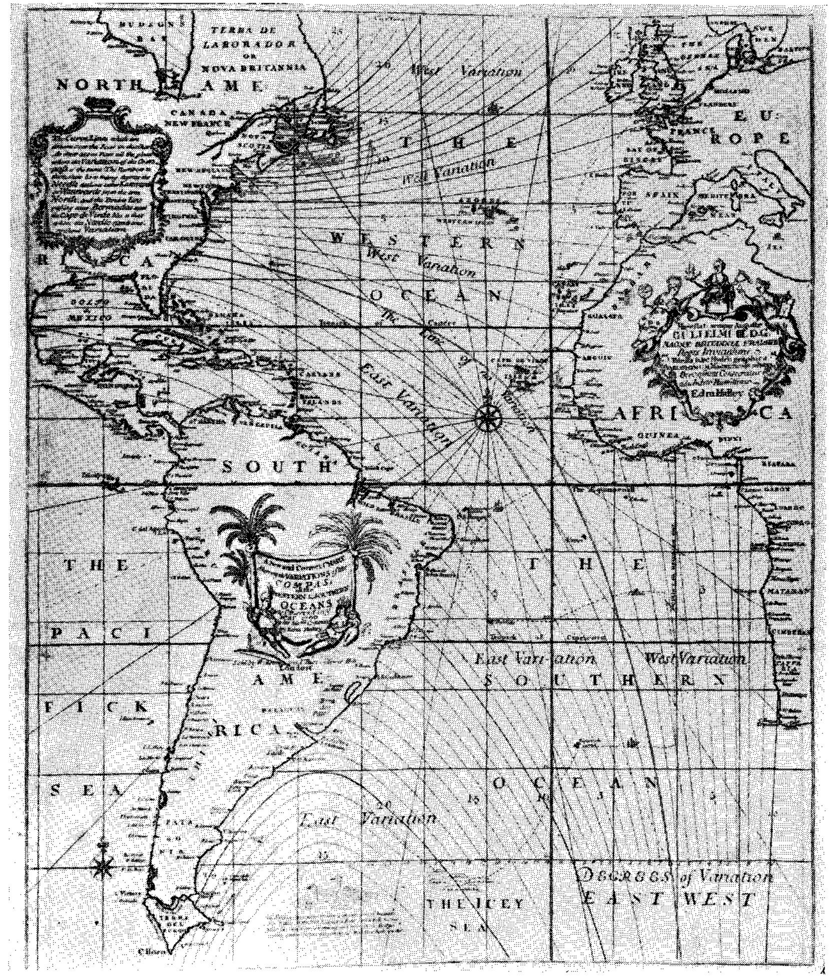

Plate 11 Halley's isogonic chart of the Atlantic Ocean (*British Museum*)

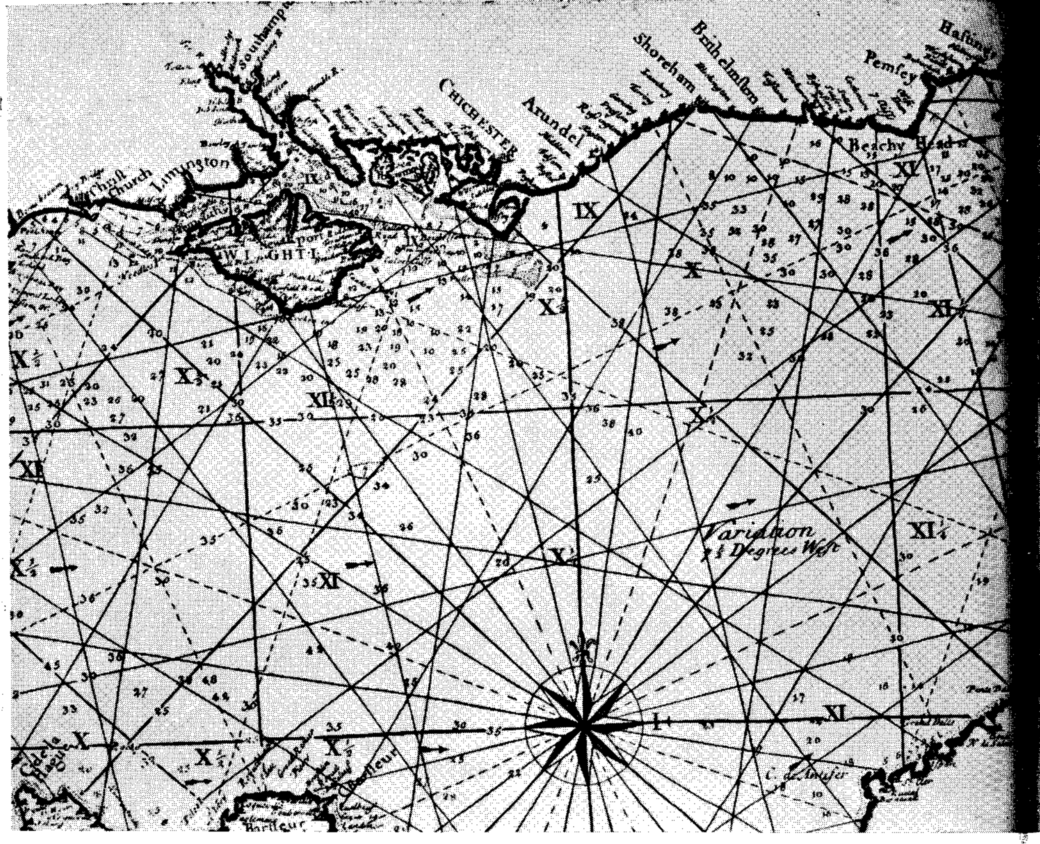

Plate 12a Part of Halley's chart of the English Channel (*British Museum*)

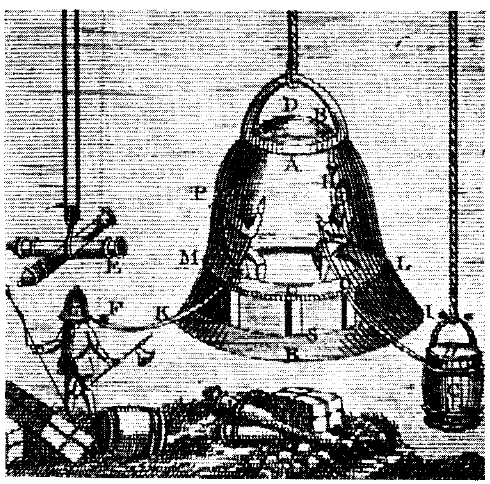

Plate 12b
Halley's diving-bell.
From W. Hooper
Rational Recreations,
London, 1782.
(*Ronan Picture Library*)

tides in the Channel, in mid-sea as well as along the shore, with special reference to times of high and low water and to irregular tidal phenomena; also to take the bearings of the successive headlands along the English coast one from another, and, where convenient, to produce the meridians from the English to the French coast so as correctly to lay down the two coasts in relation to each other (ibid., 118). The necessity for frequent anchoring, often in deep water, was likely to impose an extra strain upon the crew; and the ship's complement was increased to 25 men. Halley experienced considerable difficulty in obtaining them against the competing attractions of the merchant service. Between 19 June and 1 October 1701 he carried out a very thorough survey of the Channel tides from the Forelands to the Lizard and along the French coast to Ushant. He rode out frequent gales and was on four occasions forced into harbour. He relied upon the skill and experience of a Jersey pilot, Peter St Croix, who received £11 10s. for 58 days' service. Halley's survey seemed likely to be 'interrupted by the breaking out of Warr, which I find is suddainly expected here'. The progress of the operation was described by Halley in letters to Burchett, the Secretary to the Admiralty (*Correspondence*, 116ff.). While Halley was engaged upon his Channel survey he received an award of £200 for the great services he had rendered to navigation through his Atlantic voyages. The *Paramour* was paid off in October 1701. In 1706, the Navy Board not seeing any further use to which the little craft could be put, Prince George (Consort of Queen Anne) ordered 'the said pink to be sold by inch of candle to her Majesty's best advantage'. She fetched £122.

The results of Halley's tidal survey were set out in a 'New and Correct Chart of the Channel between England and France' (Pl. 12a), which was published in various quarters. The plate eventually came into the possession of Samuel Thornton who, in 1714, issued the chart from his shop in the Minories. It shows depths, hours of high tide, directions of the tidal currents, and variations of the compass, with conspicuous coastal features displayed in elevation.

One of the interests of the early Royal Society was the introduction of improved techniques for marine surveying. In 1674 John Collins solved the problem of determining the position of an observer at sea by taking the bearings of three fixed points on land. Early in 1702 Halley wrote to Sir Robert Southwell explaining how he had solved this three-point, or 'resection' problem (*Correspondence*, 120ff.). His procedure was somewhat as follows:

Select three prominent landmarks A, B, C lying along the coast (Fig. 21). Take the bearing of B from A and of C from B, reckoning angles from the rising or setting Sun. From a ship S anchored off shore, take the bearings of A, B, and C. These data suffice to plot the relative positions of A, B, and C on a chart whose scale can subsequently be determined from the time taken by sound to travel between any two points marked upon it. If the

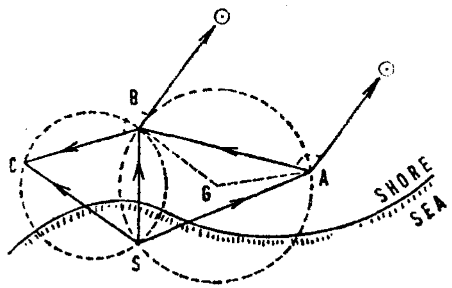

Fig. 21 Fixing the position of a rock at sea.

coast is occupied by enemy forces or is otherwise inaccessible, observations must be made from *two* separate stations off shore. To locate, now, a shoal or a submerged rock on this chart, suppose the ship S to be anchored near it, and measure the angles ASB, BSC. In the triangle ASB make $\angle ABG = \angle BAG = 90° - \angle ASB$. With centre G and radius GA (= GB) draw a circle: it will pass through S. So will a circle similarly related to the triangle BSC. The intersection of the two circles fixes the position of the shoal or rock.

When the marine cartographer passed beyond the charting of narrow seas or the construction of harbour plans and sketch-maps of limited coastal stretches, and tackled the problem of laying down appreciable areas of the Earth's surface, he ran into difficulties resulting from the spherical form of the Earth's

surface (J. B. Hewson, *A History of the Practice of Navigation*, Glasgow, 1951). Up to the middle of the sixteenth century all charts were 'plane charts', constructed with no allowance for this complication, a ship's constant course being represented by a straight line in a plane. However, in the extensive charts required for ocean navigation, particularly in high latitudes, it was necessary to allow for the convergence of the meridians towards the poles. This could be done, while still conveniently representing the meridians as parallel straight lines, by proportionately exaggerating the scale in latitude as the poles were approached. A constant course was now correctly represented on the chart by a straight line cutting the meridians at an angle equal to that which the ship's course made with the terrestrial meridians.

Gerard Mercator put this artifice to practical use in 1569; but the theoretical principles on which his map was constructed were not then clearly explained, and his world chart at first attracted little notice. 'Mercator's projection' was put upon a sound theoretical basis by the English mathematician Edward Wright in his book, *Certaine Errors in Navigation* (London, 1599), the fruit of experience gained on a voyage to the Azores ten years earlier. Wright also calculated a table of 'meridional parts' serving to show precisely how the latitude increases with distance measured up the meridians in a Mercator chart, in which the scale is at all points proportional to the secant of the latitude. There is evidence, however, that Thomas Harriot calculated such a table independently of, if not earlier than Edward Wright.[1] Halley, too, as we have seen, contributed a paper on the subject in 1696. He grasped the analogy which the meridian in a Mercator chart bears to a scale of logarithmic tangents: the construction of such a chart involves what we should call the integration of the secant of the latitude.

It was in this same year, on 17 February 1696, that Halley wrote to Samuel Pepys, better known as a diarist, but distinguished, too, in the annals of naval administration, who had invited him to report upon deficiencies in the existing practice of navigation. The episode preceded Halley's voyages, but it can most appropriately be considered here in the context of his

[1] See E. G. R. Taylor and D. H. Sadler in *The Journal of the Institute of Navigation* (1953), *6*, 131ff.

contributions to marine charting, for the astronomer's letter is largely concerned with the improvement of that art.

It was still a common practice (Halley noted) for a shipmaster to proceed to his port of destination by bringing his vessel into the latitude of the port and then sailing due eastward or westward until he reached it. But clouds often made it impossible for days on end to determine the latitude from observations of the Sun at noon; and sailors were not instructed as they should be in the use of the stars for this purpose, so that they were forced to rely upon soundings. However, Halley chiefly condemns the obstinate conservatism of many shipmasters in clinging to the use of plane charts 'as if the Earth were a Flat'. He stresses the confusion thereby introduced into the reckoning when a voyage involved a considerable change of latitude, as, for example, the crossing to Barbadoes. It was the practice for outward-bound ships to run southward so as to pick up the north-east trades and then to cross the Atlantic in a low latitude where a degree of longitude represented about 20 leagues. But, returning home, they stood to the northward until they passed Bermuda and found a westerly wind, crossing where there are not more than 15 leagues to a degree of longitude: 'Whence they commonly find Barbadoes above 100 Leagues more to the Westward of the Lizard than the Lizard is to the Eastward of Barbadoes.' The sailors blamed the discrepancy upon a current setting eastwards! Halley cites also the predicament of a 'plane sailing' shipmaster, accustomed to crossing to Barbadoes but now required to call at Newfoundland on the way: what course should he set thence for Barbadoes? It was to avoid such complications that mariners had adopted the aforesaid practice of approaching a port along its parallel of latitude. But in time of war ships adopting such a course ran a great risk of being intercepted by an enemy patrolling this favourite sea-lane.

Even in the early years of the eighteenth century the uncorrected plane chart had not been entirely superseded, for we find Halley writing in 1728: 'It is well known that many of our sailors, and some that would be accounted artists, are at this day obstinate in the use of the Plain Chart . . . and many of our masters that teach navigation teach what they call plain sailing (and many times that only) to such as are designed to take

charge of ships . . . rejecting the truly Nautical Chart, commonly called Mercator' etc. (Preface to *Atlas Maritimus et Commercialis*, London, 1728, quoted by J. B. Hewson, op. cit., 35).

To conclude this account of Halley's naval career. In 1729, following the accession of King George II, Halley asked Sloane to present him to the Queen, Caroline of Anspach, 'her Majesty not having yet honoured the Observatory with her Royal presence' (*Correspondence*, 132). Later in that year the Queen did pay a visit to the Royal Observatory, where she was graciously received, and, upon learning that Halley had held a Captain's Commission in the Navy, she obtained for him a warrant for his half pay which he drew for the rest of his life.

4 *Halley and the Nautical Sextant*

In the early days of ocean navigation, latitude was usually determined at sea by measuring the elevation of the Sun above the horizon about noon when it was crossing the meridian and had reached its highest point in the sky. Special instruments, suited for use on board ship, were designed for this purpose. In Halley's earlier years, the contrivance in general use was the Davis's Quadrant, already described.

By the middle of the seventeenth century, however, inventors were beginning to develop the idea of an instrument which should reflect the light of the Sun, or other celestial object whose elevation was required, so that, when the correct setting had been made, the object and the horizon would appear to coincide in the observer's field of view. These developments culminated in the invention, by John Hadley in 1731, of an instrument working on the principle of the familiar nautical sextant, which fulfils perfectly the need it is designed to supply. Meanwhile, in 1666, Robert Hooke had submitted to the Royal Society an instrument involving a single reflection of the celestial object to be observed; but it was unsuited to practical use. Newton, at some unknown date, conceived the idea of adding a second reflecting mirror, the essential feature of the modern sextant; but he did not publish the suggestion. Hadley's invention, however, awakened in Halley's mind a recollection of some such proposal by Newton. The records were searched and a suggestion by Newton for improving the sea quadrant was found: it was seen to have no relevance to Hadley's invention. But Halley was proved right; for after his death in 1742 there was found, mislaid among his papers, a manuscript in Newton's handwriting, describing the instrument he had designed (*Phil. Trans.* (1742), 42, 155f.).

Meanwhile, in 1692, Halley himself had described 'a Sea Quadrant Wherein both the Horizon and Object shall be seen distinctly and enlarged at one view in the common focus of a Telescope' (*Correspondence*, 161ff.). It consisted essentially of a brass telescope AB (Fig. 22), with eyepiece at A and object glass at B, to which was attached a jointed, equal-sided framework CDEF. A convex lens was affixed at G; and to a brass ruler CF was attached at right angles a glass or metal plate F covering

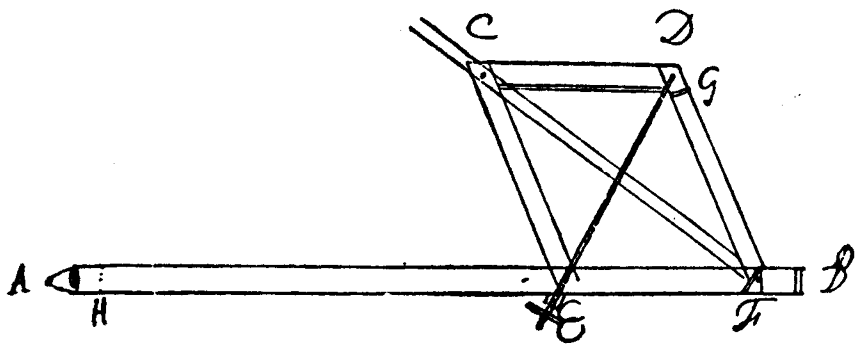

Fig.22 Halley's reflecting instrument.

half the object glass B. The observer viewed the horizon through the telescope while pointing the limb FD to the Sun, whose rays, refracted through the lens at G and reflected towards A by the plate at F, formed an image coinciding with the horizon. The Sun's altitude was given by the angle AFD, which was regulated and measured by means of a screw forming the diagonal ED.

Successful trials with the instrument suggested to Halley the possibility of observing a celestial object for hours through a fixed telescope into which the light was fed by a reflecting plate 'which must be made to move either by the hand or clockwork, so as to answer the motion of the heavens,' the reflector turning at half the rate of rotation of the celestial sphere. Halley was soon made aware that Hooke had already suggested some such anticipation of the modern coelostat (ibid., 163).

Chapter 11

The Savilian Professor

1 *Vienna and Oxford*

During part of 1702 and the year following, Halley was engaged on two diplomatic missions to Vienna. As part of the plan already prepared by William III for forming a Grand Alliance against France, the German Emperor Leopold I intended to fortify two ports on the Dalmatian coast, apparently with a view to providing safe harbourage for British men-of-war in the Adriatic. Captain Halley, as he had come to be called, was sent to advise on the project and to superintend its execution. Travelling with a safe-conduct from Queen Anne, he crossed to Holland and passed thence through Germany to Vienna, where he met the English Ambassador, George Stepney. He proceeded to Istria to carry out Leopold's designs; but some opposition from the Dutch temporarily interrupted his activities. Returning to Vienna, he met the Emperor, who presented him with a costly diamond ring from his own finger and gave him a letter of high commendation to take back to Queen Anne. Soon after his return to England he was despatched again upon the same business; and travelling this time by way of Hanover he supped with the future King George II. After another meeting with the Emperor he repaired again to Istria with the Imperial Engineer. They improved the fortifications of Trieste and pronounced Boccari fit to receive all kinds of shipping. Having carried out this mission, Halley returned home late in 1703.

The autumn of 1703, which saw Halley's return from his second mission to Vienna, saw also the death of John Wallis, Savilian Professor of Geometry at Oxford, one of the greatest mathematicians of the age of Newton and a pioneer in the use of the techniques that were developing into the calculus. Flamsteed wrote to Sharp (18 December 1703): 'Dr. Wallis is dead:

Mr. Halley expects his place, who now talks, swears, and drinks brandy like a sea-captain: so that I much fear his own ill behaviour will deprive him of the advantage of this vacancy' (F. Baily, op. cit., 215). However, early in the following year Halley was appointed to succeed Wallis; he occupied the Chair for the rest of his life, reading the lectures appointed by the founder. Halley took up his abode in New College Lane in a house that Wallis had occupied since 1672, and which Wallis's son had now divided into two tenements, designing them to serve as residences for the two Savilian Professors and transferring the lease to the University. David Gregory occupied one of the houses and Halley the other. A little observatory was built for Halley on the roof of his house; it still survives as a room hung with portraits and charts, and the writer was kindly permitted to examine it by the authorities of New College (the College sanatorium occupies the lower floors). Flamsteed gives a different turn to the story (letter to Sharp, 21 October 1704): 'Mr. Halley . . . has been in London all this vacation, but designs not to reside at Oxford. Dr. Wallis's son offers to give his father's house to the professors of mathematics, if they will constantly reside in it and the University; to make it into two tenements for them: but, by what I hear, it seems they have no mind to comply with the condition; so the University will not have the honour of their company, who are angling for better preferment at court', and so forth (F. Baily, op. cit., 218). In any case, Halley lived in London after 1713 when he became Secretary of the Royal Society. He sublet the Oxford house to one Sedgley, but he retained one room for his own use 'as long as He came to Oxon to read Lecture in ye Schools (so Sedgley wrote) at which Time He was wont to Table with me' (H. E. Bell, 'The Savilian Professors' Houses and Halley's Observatory at Oxford,' *Notes and Records of the Royal Society of London* (1961), *16*, 179ff.).

On 16 October 1710 Halley received from the University of Oxford the degree of Doctor of Civil Law (J. Foster, *Alumni Oxonienses* (1891), etc., ii, 635).

Soon after his appointment to the Savilian Chair, Halley published a *réchauffé* of some of the most interesting papers so far contributed to the Royal Society, hoping that they would thereby reach a wider circle of readers and remain no longer 'so obscurely hid, that but very few inquisitive Gentlemen ever so much as heard of them'. This was the origin of *Miscellanea Curiosa, being a Collection of some of the Principal Phenomena in Nature*, etc. (3 vols., London, 1705-7). Halley's Preface (his name does not appear) suggests a limitation to papers of mathematical and physical interest which had already passed 'the Censure of the Learned World,' and the collection includes, among other items, the cream of his own contributions, with a world chart showing both magnetic variation and wind distribution. However, the selection embraces memoirs of biological and geographical content as well.

2 *Contributions to Mathematics*

In his earliest printed paper, dealing with planetary theory, Halley gave proof of his competence in mathematics; and his other published contributions to physics or astronomy often reveal a mathematical expertise not unworthy of the age of Newton. But he also composed several papers dealing specifically with purely mathematical subjects; and though they were not of the first importance and do not lend themselves to detailed analysis in a book such as this, some brief account of them may be given here.

Seventeenth-century mathematics was still predominantly geometrical, following in this respect the tradition of the Greek masters whose classic treatises, restored and printed in their ancient tongue, still passed as standard textbooks along with others of more recent date. However, algebra had made great strides in the sixteenth century, and particularly the theory of equations, to which Halley devoted several papers; though the algebraic solutions of the various kinds of equations continued to be closely associated with the corresponding geometrical constructions. Equations of the first and second degrees (simple and quadratic equations) presented little difficulty; their solution corresponded to the ruler-and-compass constructions warranted by Euclid. Even so, it was with some hesitation that negative and imaginary roots had been accorded recognition in Renaissance times. Cubic and biquadratic equations (involving respectively the third and the fourth powers of the unknown) constituted a more formidable hurdle which, however, was surmounted in the sixteenth century by a brilliant school of Italian mathematicians.

Such equations correspond to geometrical constructions of a higher class; and Descartes, in his *Géométrie* of 1637, exhibited the discovery of the real roots of a cubic or biquadratic equation as equivalent to the determination of the points of intersection of a certain circle with a parabola. The radius of the circle and the situation of its centre in relation to the parabola were chosen according to the coefficients of the equation to be solved. Descartes began by removing the term (if present) containing the second highest power of the unknown. Similar procedures, not involving this reduction, were devised by Franz van Schooten, and by Thomas Baker in his *Geometrical Key: or the Gate of Equations Unlock'd* (London, 1684). However, their demonstrations were very laborious; and in 1687 Halley devoted two papers to simplifying the treatment of the problem. His method was primarily applicable to biquadratic equations; a cubic was first converted into a biquadratic by multiplying throughout by the unknown (*Phil. Trans.* (1687), *16*, 335ff., 387ff.). Halley later returned to the subject with a paper prompted by the several contributions of Newton, Joseph Raphson, and T. F. de Lagny to the technique of numerical approximation to the roots of equations (ibid. (1694), *18*, 136ff.).

In 1695 Halley contributed a paper dealing with 'the Doctrine of Logarithms' (ibid., (1695), *19*, 58ff.). Students are now usually introduced to this branch of mathematics through the theory of indices; but John Napier, who published his invention of logarithms in 1614, defined and computed them in a very different manner. Nicholas Mercator and David Gregory, utilizing properties of the rectangular hyperbola, had expressed logarithms as proportional to the sums of certain series. Halley welcomed this development; but he strove to eliminate geometrical considerations and to establish the series analytically, relying only upon Newton's binomial theorem. He regards a number N as formed by repeatedly multiplying unity by

$$\left(1+\frac{1}{n}\right)$$

where n is a very large number, thus,

$$1\times\left(1+\frac{1}{n}\right)^m = N.$$

The successive ratios by which 1 becomes N are called *ratiunculae*, and the number m of these is shown to be proportional to the natural logarithm of N.

Halley wrote also on the areas of the cycloid and epicycloid generated by a point on a circle which rolls upon a straight line or upon another circle (ibid., 125). Another of Halley's mathematical papers treats of the calculation of the 'meridional parts' which define the relation between the latitude and the distance measured up the meridians in a chart constructed on Mercator's Projection (ibid. (1696), *19*, 202ff.).

Halley addressed himself to some of the problems presented by various species of infinite quantities encountered in mathematics (*Phil. Trans.* (1692), *17*, 556ff.). From simple geometrical considerations he disproved the common assumption that all infinite quantities (of the same kind) could be regarded as equal. Thus a line beginning at a point and extending infinitely in one direction has only half of the length of a line through the point extending infinitely in *both* directions. If, in an infinite plane, each of the two arms of an angle of $\theta°$ be infinitely extended, the infinite area they enclose is to the total area of the plane as $\theta°$ to 360°. The infinite area intercepted between two infinite parallel lines is infinitely less than that intercepted between two infinite lines inclined to each other at however small an angle. Three different kinds of solids of infinite volume can arise according as they have one, two, or all three dimensions infinite. Proportions by volume can exist within each species; but a solid of one species can have no assignable proportion to one of another. Other kinds of infinite quantities arise from the relations of curves and their asymptotes; but these do not lend themselves to such simple treatment.

Halley also rendered service to mathematical studies by producing scholarly editions of several of the classics of ancient Greek geometry. The preparation of such editions had been the main contribution of the humanists of the scientific Renaissance; but something still remained to be done in this field. It was normally an editor's task to establish the original Greek text by the collation of the best available manuscripts and to publish this text together with a translation into classical Latin. However, the Greek literary heritage had been transmitted partly through Islamic channels; and it was not unusual for a Greek classic to survive in Arabic translation only, so that the editor had to be something of an Arabic scholar. Or perhaps the classic had been wholly or partly lost in the collapse of the ancient civilization; and it was in the fashion of Renaissance times for a scholar to attempt to 'restore' such a missing treatise with the aid of any hints that could be gathered as to its probable contents, somewhat as Dickensians have endeavoured to supply the missing conclusion of *Edwin Drood*. Halley managed to combine all these editorial roles, despite the fact that he was not primarily a classical scholar or an orientalist.

Much of Halley's scholarship was exercised upon the works of Apollonius of Perga, one of the greatest mathematicians of antiquity, and indeed of all time, who flourished in the latter part of the third century B.C. One of his minor works, *Sectio Rationis* (Cutting-off of a ratio), an exercise in geometrical algebra, was thought to be lost until an Arabic translation of it was found among the Selden manuscripts in the Bodleian and identified by Edward Bernard, the Savilian Professor of Astronomy. Bernard set about translating it into Latin; but the manuscript was very defective and he soon laid the task aside. His successor, David Gregory, made a fair copy of the original for the use of Henry Aldrich, Dean of Christ Church, at whose invitation Halley, upon succeeding Wallis in the Savilian Chair of Geometry, undertook to complete the translation. He had never previously studied Arabic; but, using as a key the few passages translated by Bernard, he eventually made out the meaning of the text. He proceeded to restore the lost companion tract, *Sectio Spatii*, following hints from Pappus as a guide to its contents. He gave his reasons for regarding the works as genuine; and he included in his edition the earliest printed Greek text of Pappus's preface to the seventh Book of his *Synagoge* (Collection). The whole was published as *Apollonii Pergæi de Sectione Rationis libri duo* etc. (Oxonii, 1706).

Apollonius, however, is chiefly remembered for his great textbook on conic sections which carried what used to be called 'geometrical conics' almost to the point where it stands today, though without employing the focus-directrix property which we treat as fundamental. This treatise was divided into eight Books of which I-IV survive in the Greek and V-VII in Arabic translation only, while Book VIII is lost. Following the publication, in 1703, of David Gregory's fine edition of Euclid, Halley was urged to edit one of the other ancient geometrical classics, and he chose the *Conics* of Apollonius. It was originally intended that Gregory should prepare a Greek text and a Latin translation of Books I-IV, with the commentary of Eutocius, while Halley translated Books V-VII from Arabic into Latin and restored Book VIII, the contents of which, closely related to those of Book VII, were sufficiently indicated by the lemmas composed by Pappus for the missing Book. However, Gregory died while the work was proceeding, and Halley found himself responsible for the whole enterprise.

The magnificent folio volume contained also the first printed Greek text of Serenus' tracts on the Section of a Cylinder and the Section of a Cone, which embodied properties employed by Halley in his investigation of the distribution of the Sun's heat among the various terrestrial latitudes, elsewhere described (*Apollonii Pergæi Conicorum libri octo* etc., Oxoniae, 1710). Halley also prepared a Latin translation of the *Sphaerica* of Menelaus of Alexandria (late first century A.D.), an early manual of spherical trigonometry. The Greek text having disappeared, he worked from surviving Arabic manuscripts and from a Hebrew version. Halley delayed publication with the intention of including some other mathematical classic in the volume; but Menelaus eventually appeared alone sixteen years after the astronomer's death.[1]

3 Halley and the Comets

Halley's greatest achievement may well have been his contribution to cometary astronomy; certainly the popular mind associates him almost solely with the comet that bears his name.

[1] *Menelai Sphæricorum libri III Quos olim, collatis MSS Hebræis & Arabicis, Typis exprimendos curavit Vir Cl. E. Halleus* etc., Oxonii, 1758.

Newton had classed comets with planets in the synthesis of universal gravitation, and he had inferred that they must travel in regular orbits describing conics of one species or another with the Sun in one focus. Observations suggested that these orbits could, without sensible error, be assumed to be parabolas. Such an orbit is an ideal trajectory never likely to be realized in nature, or not for long; but it is mathematically simpler than the ellipse or the hyperbola (between which it constitutes a transitional case), and it usually suffices to represent a comet's motion over the observable arc of its course. Newton solved the problem of determining the parabolic orbit of a comet from three given observations (*Principia*, Book III, Prop. 41), while Halley set himself 'to bring the same Method to an arithmetical Calculation, and that not without success'. He applied his technique to establish the 'elements' which serve to define the size and the position in space of such an orbit and the comet's position therein at some specified time. Modern cometary astronomy may, in fact, be said to date from Halley's *Astronomiæ Cometicæ Synopsis* (*Phil. Trans.* (1705), *24*, 1882ff.), an English translation of which is included in his posthumous *Astronomical Tables* (London, 1752).

The *Synopsis* begins with an historical introduction to cometary astronomy which we shall supplement considerably from other sources. The view that comets are celestial bodies visible to us only when they are describing the lower parts of their elongated orbits was put forward by certain Greek writers, as reported by Seneca (Halley was acquainted with the passage). However, up to the sixteenth century the Aristotelian view generally prevailed that a comet was an accumulation of terrestrial exhalations slowly burning high up in the Earth's atmosphere and therefore nearer to us than the Moon. It is not surprising that the ancients saw no point in compiling and transmitting to us any precise records of the wanderings through the heavens of comets so conceived; the earliest serviceable record that Halley could find dated from 1337. However, in 1577 there appeared a bright comet which many European astronomers examined for 'diurnal parallax', an apparent shift in the position of the object due to the observer's motion as he is carried round by the Earth's daily rotation. The discovery that

the comet showed no such parallax proved that it must be considerably more remote from us than the Moon; it must therefore be an astronomical phenomenon occurring in a region of space where it had been thought impossible for any new luminaries to appear.

The next problem was to grasp the geometry of a comet's motion which often carries it through several constellations during the brief period of its visibility. Astronomers were favoured during the sixteenth and seventeenth centuries by the passage of many notable comets through the northern heavens. Tycho Brahe assigned to the comet of 1577 a nearly circular orbit round the Sun. Kepler sought to represent the courses of comets by straight lines traversed at regularly varying speeds. Thomas Harriot's correspondent, Sir William Lower, investigating 'the unknown walkes of comets,' suggested in 1610 a greatly elongated ellipse. Seth Ward, the Savilian Professor of Astronomy, in a lecture prompted by the appearance of a comet in 1652, declared that comets are probably 'carryed round in Circles or Ellipses (either including or excluding the Globe of the Earth) so great, that the Comets are never visible to us, but when they come to the Perigees of those Circles or Ellipses, and ever after invisible till they have absolved their periods in those vast Orbs' (Robert Plot, *The Natural History of Oxfordshire*, Oxford, [1677], 225).

In a passage first published in 1678 but claiming to be part of his lecture-notes for 1665, Robert Hooke (Ward's disciple in astronomy) asked, concerning the comet of 1664, 'What kind of motion it was carried with? Whether in a straight or bended line? And if bended, whether in a circular or other curve, as elliptical or other compounded line, whether the convex or concave side of the curve were turned towards the Earth? Whether in any of those lines it moved equal or unequal spaces in equal times? . . . Whether it ever appears again, being moved in a circle; or be carried clear away, and never appear again, being moved in a straight or parabolœidical line?' (R. Hooke, *Cometa*, London, 1678, 7f.). It appeared natural to Hooke that a comet should pursue a closed orbit under the Sun's attraction; and he surmised that the comet of 1664 might be a return of that of 1618.

Writing of the same comet, Hevelius inclined to the view that a cometary orbit was a parabola or, exceptionally, a hyperbola or a straight line; but his arguments were pseudo-physical and there is nothing to suggest that he located the Sun at the focus. However, the Saxon astronomer Georg Dörffel, investigating the comet of 1680, which Halley observed on his tour arrived by a graphical construction at a parabolic orbit having the Sun in the accessible focus. Dörffel's achievement escaped the notice of the astronomers of his day, and it was deprived of all but historical interest by the publication, six years later, of Newton's *Principia*. Newton, who seems not to have heard of the Saxon astronomer, established a cometary orbit as some species of conic by inference from his propositions on the motions of mutually gravitating bodies, among which he tacitly classed the comets along with the planets; and he chose the same comet of 1680 to

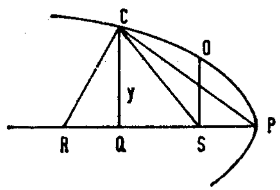

Fig. 23 Computing a comet's motion.

establish this hypothesis. In the remainder of his *Synopsis*, Halley claims to have followed in Newton's footsteps.

He first collects and analyses all available cometary observations recorded up to the end of the seventeenth century and sufficiently precise for his purpose. This information enabled him to compute and tabulate the elements of the hypothetical parabolic orbits of twenty-four comets, dating from 1337 to 1698.

For this purpose he had to construct a general table showing how a comet's angular distance from the vertex of its orbit increases with lapse of time. The same table served for all the comets, since all parabolas are similar and all are similarly described under gravitation; only the scale of distances from the Sun was peculiar to each. He constructed his table somewhat as follows. Suppose a comet C (Fig. 23) describes a parabola POC with focus S (the Sun), vertex P (the perihelion), O the point

90° from perihelion, and C any other point on the orbit. Let CR be the normal at C and let the ordinate CQ equal y. Then by Kepler's second Law, the area of the sector CSP increases proportionately to the time elapsed since the comet passed through P. Assign the value 2 to the latus rectum; then (by some process of integration) the area of the segment COP is $\frac{1}{12}y^3$; also the triangle CSP is $\frac{1}{4}y$; hence the sector CSP is $\frac{1}{12}y^3+\frac{1}{4}y=\text{A}$, say, or, $y^3+3y=12\text{A}$. Assuming A known (as proportional to the time from P), the real root of this equation gives y, which is the tangent of the angle CRQ (since RQ=SO=1), and this is the tangent of half the angle CSP (since SC=SR in the parabola). Hence the polar angle CSP can be tabulated against the time; CS is also given in terms of the latus rectum.

It was noticeable that the comets, unlike the planets, did not all revolve in the same direction round the Sun nor restrict their orbital planes to a single zodiacal belt; this seemed to contradict the vortex theory of Descartes. The type of conic described by a body under gravitation is indicated by its speed at any point on its orbit. No comet had been found to possess a velocity capable of carrying it on a hyperbolic arc into the depths of space never to return to the Sun. However, it was impossible in practice to distinguish between a parabolic and an elongated elliptic orbit; and many comets might really be describing ellipses, thus returning to our skies at regular intervals, so that they might not be so numerous as they appeared to be. The space between the Sun and the stars was sufficient to accommodate such elongated orbits.

Halley's table of cometary elements served to show up any periodic comets: 'If ever a new comet appears, we may, by comparing the elements, distinguish whether or not it could be one of those formerly observed, and accordingly determine its period and axis and predict its return.' He pointed out that the orbital elements of three historic comets were so similar as to suggest that the apparitions represented successive returns of the same body, describing a closed orbit in a period of about 75 years.

Many considerations incline me to believe the comet of 1531 observed by Apianus to have been the same as that described by Kepler and Longomontanus in 1607 and which I again observed when it returned in 1682. All the elements agree; only the inequality of the periods appears to oppose this conclusion, but this is not so considerable that it cannot be attributed to physical causes. For the motion of the planet Saturn is so disturbed by the others, particularly by Jupiter, that the planet's period of revolution is uncertain by several whole days. How much more subject to such errors will a comet be, which travels into space about four times further than Saturn, and whose velocity, if increased ever so little, could change an orbit from elliptic to parabolic. Also the identity of the two comets is supported by the fact that, in the summer of 1456, a comet was seen travelling retrograde between the Sun and the Earth in the same manner as the others; and though it was observed astronomically by no one, yet I conjecture from its date and course that it was no other than the before-mentioned comet. Whence I would venture confidently to predict its return, namely in the year 1758. And if this occurs there will be no further cause for doubt that other comets ought to return also.

In the later (Latin and English) edition of the *Synopsis* included in his Tables of 1752, Halley confidently identified his comet with those that had appeared in 1305, 1380, and 1456, 'wherefore if according to what we have already said it should return again about the year 1758, candid posterity will not refuse to acknowledge that this was first discovered by an Englishman'.

The comet was indeed sighted on Christmas Day 1758 and it returned to perihelion early in the following year. It subsequently reappeared in 1835 and 1910, as predicted. Halley's hypothesis gained further support from the subsequent revelation of earlier historic appearances of this awe-inspiring visitant, going back to the year 240 B.C.[1] It was Halley's comet whose appearance in A.D. 1066 was depicted in the Bayeux Tapestry (Pl. 14a); and Josephus describes how it appeared to hang like a sword over Jerusalem at the commencement of the war that ended in the destruction of the Holy City. The present writer can recall the apparition of the comet in 1910 and the superstitious association of the event with the death of King Edward VII (Pls. 14b and 15).

It appeared from Halley's table that some comets crossed the plane of the ecliptic near the Earth's orbit; and if the Earth happened to be passing at the time, the comet, viewed from widely separated terrestrial stations, would exhibit an observable parallax and one, moreover, having a calculable ratio to the Sun's parallax. This suggested a method of accurately determining the Sun's distance from the Earth. The comet of 1680 might have served for this purpose as its diurnal parallax could have equalled that of the Moon. As for what would happen should a comet collide with the Earth, 'May the great good God avert a shock or contact of such great Bodies moving with such forces (which however is manifestly by no means impossible), lest this most beautiful order of things be intirely destroyed and reduced into its antient chaos'.

[1] See papers by P. H. Cowell and A. C. D. Crommelin in *Monthly Notices of the Royal Astronomical Society* (1907-8), Vol. 68.

On the evening of 10 June 1717, as Halley was preparing to examine the spots on Mars through his 24-foot telescope, he noticed, in the field of the instrument and not far from the planet, a small whitish object like a nebula, barely visible in the moonlight. It appeared to emit 'a very short kind of Radiation . . . nearly towards the Point opposite to the Sun'. Halley marked the position of the object by reference to two small stars, and he continued to observe it until midnight in the company of two friends, Moses Williams and Alban Thomas. As it appeared to have no proper motion, he pronounced it a nebula and not a comet. But when all three observers looked for it on the following evening, and again on the night of the 15th when the sky was clear and the Moon absent, they found the two stars but no trace of the visitant; and Halley concluded that it must after all have been a comet far outside the Earth's orbit. He could not recall that a comet had been seen in England for 35 years, though several small, tail-less objects had been detected by the French observers; and others had been reported from the southern hemisphere. But he thought there might be many more, completely invisible to the naked eye and destined to escape discovery unless a skilled observer should chance to point his telescope to their exact location on the sphere. And it was partly to prove that this was no mere conjecture that he reported his discovery of the telescopic comet of 1717 (*Phil. Trans.* (1717), 30, 721ff.).

4 *The* Historia Cœlestis *of 1712*

As the century drew to its close, Halley had become involved in an unhappy and long-drawn quarrel with his former friend John

Flamsteed, the Astronomer Royal. The root cause of their alienation was probably some mutual incompatibility of the two men, finding its most concrete expression in their conflicting religious convictions; for while Flamsteed was a devout Christian, Halley seems to have inclined towards the Unitarian position. Again, as something of a sick man, boxed up at Greenwich, Flamsteed may have envied the vigour and the versatility of the younger astronomer and may have seen in his rapid rise to fame a threat to his own tenure of office. However, the dispute, in which Newton became equally involved, came to turn upon the accusation that Flamsteed was excessively tardy in publishing his observations; and it was greatly embittered by the publication, without the Greenwich astronomer's knowledge or consent, of an imperfect edition of those observations.

This particular issue arose largely out of the anomalous conditions of Flamsteed's appointment. The Government had provided him neither with instruments nor (apart from his small salary, sometimes in arrears) with funds to meet the cost of making or repairing them, nor with the services of a computer. He spent in all some £2,000 of his own fortune on the equipment of the Royal Observatory, and he was forced to take pupils in order to supplement his income. These conditions prevailed throughout the forty-five years of his service at Greenwich. Under these circumstances he felt entitled to decide for himself when his observations had reached the degree of completion and refinement which would justify him in publishing them, as he had always intended to do. On the other hand, Halley and Newton took the view that Flamsteed was a public official whose observations belonged to the State and should be published expeditiously for the common good. Newton, particularly, was anxious to refine his lunar theory for publication in a revised edition of his *Principia*; and Flamsteed was the only man in the world who could give him the accurate positions of the Moon that he required for that purpose. It was unfortunate that Flamsteed's interests ran chiefly to observation; he had but little comprehension of what Newton was aiming at, and what he had already achieved in the realm of gravitational astronomy.

By 1704 Flamsteed had collected materials for a catalogue embracing between two and three thousand star places as well as

corrections to the current lunar and planetary tables. Prince George of Denmark, the Consort of Queen Anne, generously undertook to pay the expenses of publication. Flamsteed was persuaded into handing over to a committee, dominated by Newton, his original observations and an incomplete set of star places merely as a guarantee that the complete catalogue would be forthcoming (he had already given Newton, in confidence, upwards of 300 lunar places). In 1708, before the printing was finished, Prince George died, and the scheme should have lapsed; but the committee now entrusted Halley with the task of publishing so much of the Greenwich observations as was in their hands, and of making good deficiences with observations of his own. The imperfect catalogue and the observations, curtailed and re-arranged, thus appeared in a premature and partly spurious edition (*Historiæ cœlestis libri duo*, Londini, 1712). However, the death of Newton's patron, the Earl of Halifax, in 1715, and the resulting changes in the political scene, afforded Flamsteed the opportunity to gain possession of the unsold copies of the 1712 edition. He detached the signatures giving correctly his earlier observations and committed the rest to the flames ('I made a *Sacrifice of them to Heavenly Truth*'). He then set about preparing the record of his life's work for publication in his own way and at his own expense. Flamsteed's task was still unfinished when he died at Greenwich on 31 December 1719; and it was left to the (unrewarded) devotion of two of his friends and sometime assistants, Joseph Crosthwait and Abraham Sharp, to publish the three volumes of the *Historia cœlestis Britannica* (Londini, 1725), an historic monument (despite inevitable imperfections) to the progress of fundamental astronomy.[1]

[1] See F. Baily, *An Account of the Revd. John Flamsteed*, London, 1835; W. Whewell, *Newton and Flamsteed*, Cambridge, 1836; Olin J. Eggen, 'Flamsteed and Halley,' *Occasional Notes of the Royal Astronomical Society* (1958), 3, 211ff.

Chapter 12

Signs in the Heavens

1 *Thoughts on Meteors*

METEORS, or 'falling stars', are familiar to every observer of the night sky. In Halley's time the ancient view still generally prevailed that they were explosions of inflammable vapours in the Earth's atmosphere. By comparing observations of such a phenomenon made from various places where it was visible, it had sometimes been possible to estimate the approximate height above the Earth's surface at which the supposed explosion had occurred. The altitudes thus calculated for several recent meteors were of the order of 40 miles, which seemed to approach the effective limit of the Earth's atmosphere as established by Halley in his paper of 1686. The problem of how the vapours involved could rise so high formed the starting-point of a paper by Halley on 'Extraordinary Meteors or Lights in the Sky' (*Phil. Trans.* (1714), *29*, 159ff.).

> It may deserve the Honourable Society's Thoughts, how so great a Quantity of Vapour should be raised to the very Top of the Atmosphere, and there collected, so as upon its Accension or otherwise Illumination, to give a Light to a Circle of above 100 Miles Diameter, not much inferior to the Light of the Moon; so as one might see to take a Pin from the Ground in the otherwise dark Night. 'Tis hard to conceive what sort of Exhalations should rise from the Earth, either by the Action of the Sun or subterraneous Heat, so as to surmount the extream Cold and Rareness of the Air in those upper Regions: But the Fact is indisputable and therefore requires a Solution.

Equally difficult to account for were the size and the speed of travel of these meteors, such as the one which G. Montanari of Bologna observed in 1676 as it passed over Italy with a hissing and rattling noise, looking bigger than the Moon. What substance could be so impelled and ignited, 'there being no

Vulcano or other Spiraculum of subterraneous Fire in the N.E. parts of the World, that we ever yet heard of, from whence it might be projected?' Halley was accordingly led on to the hypothesis of an extra-terrestrial origin of such meteors:

> I have much considered this Appearance, and . . . am induced to think that it must be some Collection of Matter form'd in the Aether, as it were by some fortuitous Concourse of Atoms, and that the Earth met with it as it passed along in its Orb, then but newly formed, and before it had conceived any great Impetus of Descent towards the Sun.

The noise heard might well be that of the meteor being quenched in the Tyrrhenian Sea after 'losing its Motion from the Opposition of the Medium'.

After completing his paper, Halley learned of other recorded instances of these phenomena:

> It is plain . . . that this sort of luminous Vapour is not exceedingly seldom thus collected, and when the like shall happen again, the Curious are entreated to take more Notice of them than has been hitherto done, that we may be enabled thereby better to account for the surprizing Appearances of this sort of Meteor.

Early in the nineteenth century it was established that meteors are initially diminutive members of the solar system, revolving round the Sun. When one of them, in its course, enters the Earth's atmosphere, it is heated to incandescent vapour by the friction of the air, or, very rarely, penetrates to the Earth's surface as a meteorite.

A 'wonderful luminous Meteor', seen on the evening of 19 March 1719, directed Halley's attention afresh to these phenomena. (Since publishing his former paper on the subject, he had happened upon another recorded instance of a spectacular meteor, one seen all over Germany in November 1623 and described by Kepler.) He missed seeing this latest prodigy, but he was able to build up an account of it from reports sent in to the Royal Society (*Phil. Trans.* (1719), *30*, 978ff.). The Society's Vice-President, Sir Hans Sloane, was in Bloomsbury Square, near what is now the British Museum, when, about 8.15 p.m., he saw a sudden light, scarcely less refulgent than the full light of the Sun. A ball of fire, 'whitish, with an eye of Blue', seemed to pass across the sky from the Pleiades to Orion's Belt, leaving a reddish-yellow trail which persisted for more than a minute; but no sound was heard. Further information was supplied by the astronomer James Pound, by John Whiteside, the Keeper of the Ashmolean, who observed the phenomenon from Oxford, and by Nicolas Fatio, at Worcester.

An analysis of their estimates of the elevation of the explosion above the horizon suggested that it had occurred about 74 miles above the Earth's surface, high enough to be visible over much of western Europe; and Halley's immediate reaction was that it could have served as a celestial signal for the determination of longitudes. No telescopes would have been required, only clocks correctly set. Other reports enabled the course of the meteor to be tracked. There seemed reason to suppose that the initial explosion occurred over Tiverton in Devon, and that the meteor then moved away towards Brittany at a speed of about 300 miles a minute, 'which is a Swiftness wholly incredible, and such that if a heavy Body were projected horizontally with the same, it would not descend by its Gravity to the Earth, but would rather fly off, and move round its Centre in a perpetual Orb, resembling that of the Moon',—in other words, the meteor moved with a spaceman's speed. For observers in Devon and the adjoining counties the flash was attended by a report like a broadside, followed by a rattle like small-arms fire; the sound was heard as far east as Lewes, in Sussex. At the same time the air tremor shook doors and windows. From the supposed height and apparent size of the meteor, its diameter was estimated to be not less than one and a half miles.

The phenomenon was, in fact, a 'detonating meteor' of a not unfamiliar type (the writer observed one on 2 October 1926); and the chief interest attaches to Halley's explanation of it. It seemed difficult to account for the great height at which the combustion occurred, the vast quantity of matter apparently involved and the extravagant speed of the meteor, and to explain how the noise and tremor of an explosion occurring in an extremely rare medium could yet be propagated to such great distances. Halley supposed that the heat of the previous summer might have raised much vapour from the Earth; most of this would return as watery precipitation, but

the inflammable sulphureous Vapours, by an instant Levity, have a sort of *Vis centrifuga*, and not only have no need of the Air to support them, but being agitated by Heat, will ascend in Vacuo Boileano [the vacuum of Boyle's air pump], and sublime to the top of the Receiver . . . the Experiment whereof was first shewn me by the Reverend Mr. Whiteside at Oxford, and was very lately made before the Royal Society.

The matter composing the meteor might have been collected from a large area of the Earth's surface and raised in the manner stated far above the reputed limits of the atmosphere, there to coalesce and to 'lie like a Train of Gunpowder in the Ether, till catching fire by some internal Ferment as we find the Damps in Mines frequently do, the Flame would be communicated to its continued parts, and so run on like a Train fir'd'. This would account for the enormous speed of the meteor: 'it was not a Globe of Fire that ran along, but a successive kindling of new Matter'. How sound could be propagated through an almost vacuous space Halley could not imagine; he knew sound was supposed to travel through air and was greatly diminished in the artificial vacuum of the air pump. But perhaps the immensity of the explosion compensated for the tenuity of the medium.

2 *An Historic Eclipse*

In studying the constitution of the Sun, astronomers have been greatly favoured by the fortunate accident that the Earth possesses a satellite of just the apparent size required to cover the solar disc from time to time. Certain appendages of the Sun, ordinarily lost in the glare of his rays, are rendered momentarily visible by the interposition of the Moon. Only within the last hundred years has full advantage been taken of the opportunities thus afforded by total solar eclipses; but the close observation and interpretation of eclipse phenomena may be said to have begun with Halley and his contemporaries at the beginning of the eighteenth century.

As a prelude to Halley's more elaborate observations, reference may be made to a report by one Captain Stanyan, a kinsman of Abraham Stanyan, British Envoy to the Swiss Cantons (*Phil. Trans.* (1706), *25*, 2240f.). Describing in a letter to

Flamsteed the solar eclipse of 1 May 1706, which he had observed at Bern, Stanyan related how the Sun, emerging after the eclipse, 'was preceded by a Blood red streak of Light, from its Left Limb; which continued not longer than 6 or 7 Seconds of Time; then part of the Sun's Disk appeared' etc. Flamsteed, communicating the letter to the *Transactions*, comments: 'The Captain is the first Man I ever heard of that took notice of a Red Streak of Light preceding the emersion of the Sun's body from a total Eclipse'; and he inferred from the observation the existence of a lunar atmosphere.

The spring of 1715 found the astronomers of western Europe preparing to observe the solar eclipse due to take place on 22 April of that year. The eclipse was to betotal in the London area; and the Royal Society instructed Halley to arrange for observations to be made from the roof of their house in Crane Court. In preparation for the eclipse, Halley put out a broadsheet chart (Pl. 13) of its predicted track across southern England bearing the title: 'A Description of the Passage of the Shadow of the Moon, over England, In the Total Eclipse of the Sun, on the 22nd Day of April 1715 in the Morning.' To the Chart a legend was attached, part of which ran:

The like Eclipse having not for many Ages been seen in the Southern Parts of Great Britain, I thought it not improper to give the Publick an Account thereof, that the suddain darkness wherein the Starrs will be visible about the Sun, may give no surprize to the People, who would, if unadvertized, be apt to look upon it as Ominous, and to Interpret it as portending evill to our Sovereign Lord King George and his Government, which God preserve. Hereby they will see that there is nothing in it more than Natural, and no more than the necessary result of the Motions of the Sun and Moon; And how well these are understood will appear by this Eclipse. . . . The Curious are desired to Observe it, and especially the duration of Total Darkness, with all the care they can; for thereby the Situation and dimensions of the Shadow will be nicely determined; and by means thereof we may be enabled to Predict the like Appearances for ye future, to a greater degree of certainty than can be pretended to at present, for want of such Observations, By their humble Servant, EDMUND HALLEY.

The observers were favoured with good weather; and Halley described the course of the phenomenon in a paper which also embodied reports sent in from many other quarters (*Phil. Trans.*

(1715), *29*, 245ff.). Halley set up at Crane Court a quadrant of about 30 inches radius, equipped with telescopic sights, and a pendulum clock adjusted to show mean time; and he carried out preliminary observations to determine the errors of these instruments. He also provided several telescopes for the use of Fellows and others who might wish to follow the eclipse.

On the appointed day Halley took up his station; and he succeeded in timing the various phases which mark the progress of an eclipse and which constitute a delicate test of the accuracy of the tables giving the motions of the Earth and the Moon. His account of the awe-inspiring phenomena which accompany a total eclipse may best be given in his own words: 'From this time [of the Moon's first contact with the solar disc] the Eclipse advanced, and by Nine of the Clock was about Ten Digits [ten twelfths of the Sun's diameter obscured], when the Face and Colour of the Sky began to change from perfect serene azure blew to a more dusk livid Colour having an eye of Purple intermixt, and grew darker and darker till the total Immersion of the Sun'. Emerging from behind the Moon, 'the Sun came out in an Instant with so much Lustre that it surprized the Beholder, and in a Moment restored the Day'. It was noticed that just before the Sun disappeared, the shrinking filament of the solar surface on the Moon's east side grew very faint and could be viewed (even through a telescope) without discomfort, but that the eye could not endure the brightness of the emergent Sun when totality was passed. Halley gives two explanations, one, the physiological reason we should accept today: 'The Pupil of the Eye did necessarily dilate it self during the Darkness, which before had been much contracted by looking on the Sun'. His alternative explanation presupposes the existence of a lunar atmosphere (which later astronomers have found reason to deny):

The Eastern parts of the Moon, having been heated with a Day near as long as Thirty of ours, could not fail of having that part of its Atmosphere replete with Vapours raised by the so long continued action of the Sun; and by consequence it was more dense near the Moon's Surface and more capable of obstructing the Lustre of the Sun's Beams. Whereas at the same time the Western Edge of the Moon had suffered as long a Night, during which there might fall in

Dews all the Vapours that were raised in the preceeding long Day; and for that reason, that part of its Atmosphere might be seen much more pure and transparent.

The last visible filament of the disappearing Sun had the shape of a horn 'whose Extremities seemed to lose their Acuteness, and to become round like Stars'. For a few seconds a portion of this horn was separated from the rest, a phenomenon attributable to the inequalities of the Moon's surface.

Halley's paper is notable for its scientific description of two characteristic solar phenomena visible only during the fleeting moments of a total eclipse. The phenomena in question are those now known as the *corona*, a luminous outer envelope of the Sun, and the solar *prominences*, which appear as brightly-coloured flames projecting from the Moon-covered disc. He was not the first to observe these spectacles. It must have been a prominence that Stanyan saw in 1706, while the corona was described by Plutarch and by Kepler. The Chinese are believed to have taken note of the corona, or perhaps of both phenomena, not later than 1000 B.C. (J. Needham, *Science and Civilization in China*, Cambridge, 1959, iii, 423). However, Halley's descriptions are worth giving in his own words; and here is his account of the corona:

A few Seconds before the Sun was all hid, there discovered it self round the Moon a luminous Ring, about a Digit or perhaps a tenth Part of the Moon's Diameter in Breadth. It was of a pale whiteness or rather Pearl colour, seeming to me a little tinged with the Colours of the Iris, and to be concentrick with the Moon, whence I concluded it the Moon's Atmosphere. But the great hight thereof far exceeding that of our Earth's Atmosphere; and the Observations of some who found the Breadth of the Ring to encrease on the West Side of the Moon as the Emersion approached; together with the contrary Sentiments of those whose Judgment I shall always revere, makes me less confident, especially in a Matter whereto, I must confess, I gave not all the Attention requisite. Whatever it was, this Ring appeared much brighter and whiter near the Body of the Moon than at a Distance from it; and its outward Circumference, which was ill defined, seemed terminated only by the extream Rarity of the Matter it was composed of; and in all Respects resembled the Appearance of an enlightened Atmosphere viewed from far: but whether it belonged to the Sun or Moon I shall not at present undertake to decide.

Halley noticed 'perpetual Flashes or Coruscations of Light' darting out from behind the Moon during the period of totality; and it was about two or three seconds before the Sun emerged that he noticed a prominence:

On the . . . Western side where the Sun was just coming out, a long and very narrow Streak of a dusky but strong Red Light seemed to colour the dark edge of the Moon, tho' nothing like it had been seen immediately after the Immersion. But this instantly vanished upon the first Appearance of the Sun, as did also the aforesaid luminous Ring.

The corona long continued to be regarded as an optical effect produced by the Moon's, or perhaps by the Earth's atmosphere; but Sir Norman Lockyer established about a century ago that it is indeed a solar appendage. The prominences have proved to be huge masses of incandescent gas, mostly composed of hydrogen, helium, and calcium, and frequently moving with prodigious speeds.

Such was the darkness that accompanied the total phase of the eclipse that Halley expected to see many more stars than were, in fact, visible in London. The planets Mercury, Venus, and Jupiter were all that could be seen 'by the Gentlemen of the Society from the Top of their House where they had a free Horizon'; and the only stars seen by anyone in town seemed to have been Capella and Aldebaran. 'Nor was the Light of the Ring round the Moon capable of effacing the Lustre of the Stars, for it was vastly inferior to that of the full Moon, and so weak that I did not observe that it cast a Shade'. But the portion of the atmosphere just above the horizon and lying outside of the Moon's shadow-cone gave a diffused light, and this may have prevented the appearance of the stars. For the darkness was more complete, with as many as 20 stars visible, in places nearer to the central track of the shadow, though the light of the ring round the eclipsed Sun (the corona) was the same for all these places.

Several Fellows and distinguished visitors observed the eclipse with Halley at Crane Court, among them Sir Thomas Parker, the Lord Chief Justice, who timed the end of totality by measuring the zenith distance of Jupiter, and the Chevalier de Louville, a French astronomer, who had come over specially for the occasion, bringing his micrometer with him. Halley's results showed good agreement with those of the distinguished observer

James Pound, which 'makes us the less solicitous for what was noted at the Royal Observatory at Greenwich', whence only the estimated duration of totality could be learnt. William Derham and Samuel Molyneux were other British observers who followed the eclipse, but the university astronomers were unfortunate: John Keill at Oxford had to contend with clouds, while at Cambridge, though the heavens were favourable, Roger Cotes 'had the misfortune to be opprest by too much Company'.

'I forbear', concludes Halley, 'to mention the Chill and Damp which attended the Darkness of this Eclipse, of which most Spectators were sensible and equally Judges. Nor shall I trouble you with the Concern that appear'd in all sorts of Animals, Birds, Beasts and Fishes upon the Extinction of the Sun, since our selves could not behold it without some sense of Horror'.

It seems that Halley had devised some procedure for calculating the circumstances of an eclipse in advance. Flamsteed contributed a section on the Doctrine of the Sphere to the *New Systeme of Mathematicks* (2 vols., London, 1691) which Sir Jonas Moore compiled for the use of the mathematical scholars of Christ's Hospital; and in his Preface (unpaged) he (Flamsteed) described a geometrical construction he had hit upon in 1676 for predicting the extent of a solar eclipse and when its various phases would occur; he had since discovered that Sir Christopher Wren and Edmond Halley had each independently devised such a procedure. 'In some Discourse I had with Mr. Halley, before he went to observe the Southern Constellations at St. Helena, he Mentioned the Construction of Eclipses as possible, but out of a tender Affection to his own Inventions, or for what other Reason I know not, he was pleased to conceal his Method both from me, who then thought it scarce possible, and, for ought I can understand, from all others.' It appears, incidentally, that Halley revised the geographical section in Moore's compendium.

3 *An Auroral Display*

The aurora borealis, though regularly visible in high northern latitudes and often figuring in the European popular literature of prodigy, scarcely attracted scientific notice before the beginning of the eighteenth century. It was Halley who gave one of the earliest sober accounts of a brilliant display of the northern lights, with an accurate description of the principal auroral forms and a critical discussion of the possible causes of the phenomenon. His paper also marked a further development in his views on the magnetic constitution of the Earth. His attention was first drawn to the problem of the aurora by a brilliant display which occurred on the evening of 6 March 1716. This illumination received from superstition the name of 'Lord Derwentwater's Lights', occurring as it did within a fortnight of the execution of the Jacobite nobleman. The Royal Society received accounts of the spectacle from many parts of Britain; and Halley, who had followed its later phases with his own eyes, was instructed to draw up a general account of the phenomenon and to explain at greater length his theory of its causation, which had found favour with some of the Fellows. Halley had never before seen an auroral display; and he missed the beginning of this one through being out for the evening at a friend's house. However, he drew upon the reports of other eyewitnesses in order to complete his graphic narrative (*Phil. Trans.* (1716), *29*, 406ff.).

The afternoon of 6 March had been warm and serene; but as it grew dark there appeared low in the north-east a dusky cloud edged with reddish-yellow light from which rose long, luminous rays perpendicular to the horizon and some of them extending nearly to the zenith. The cloud moved towards the west; and some of the rays, appearing to converge north of the zenith, formed a *corona* (Halley established this term), variously likened to the 'glory' round the Divine Name in Church decorations, to the insignia of the Garter, or to the dome of St Paul's Cathedral viewed from within. This phase soon passed; but for an hour and a half transient rays kept appearing even out of a clear sky until the diffused light in the north subsided on to the horizon as a bright *crepusculum*, the glow that heralds the dawn. At this point Halley was apprised of what was occurring: 'Upon the first Information of the thing we immediately ran to the Windows.' A thin vapour seemed to rise rapidly from the eastern horizon at regular intervals, six or seven times a minute. To judge from their situation, in the heart of the Earth's shadow, these vapours could not owe their illumination to the light of the Sun, as had recently been suggested in several quarters. Leaving the house to obtain a better view of the sky, Halley beheld similar appearances on the northern horizon. To the north-east the sky showed no luminosity 'but on the contrary what appeared to be an exceedingly black and dismal Cloud seem'd to hang over all that part of it'. However, it was not a cloud for the stars shone through it; and the observation may refer to a characteristic auroral form, the *dark segment*. Auroral arcs or bands are suggested by Halley's observation that the light 'fashioned it self into the Shape of two Laminae or Streaks, lying in a Position parallel to the Horizon, whose Edges were but ill terminated'. They were about one degree in breadth and joined at one end, and they hung one over the other for a considerable time, giving enough light to read by. A huge pyramid of light now appeared; and Halley reflected that if its situation relative to the background of stars had been simultaneously noted at, say, London and Oxford, a comparison of the results might have served to determine the height and distance of the apparition. He recommended that, should a similar display occur in the future, observers should set their clocks to London time and should note at the end of each half-hour the exact situation of any remarkable auroral feature, and particularly the azimuth of such a tall pyramid, with a view to determinations of their distances. By 11 o'clock, as no new manifestation had occurred, 'we thought it no longer worth while to bear the Chill of the night Air *sub divo*', though Halley continued to watch the remains of the spectacle from an upstairs window of his house until early the following morning. Partial repetitions of the display in the succeeding weeks are described in the pages of the *Transactions* immediately following (430ff.).

Records of previous lights of this kind, covering the period from 1574 to 1708, suggested that such displays occurred in groups, as indeed they do; and this observation served as a starting-point for the speculations which occupy the latter part of Halley's paper. It would appear that 'the Air, or Earth, or both, are sometimes, though but seldom and with great Intervals, disposed to produce this Phenomenon'. Halley was at first inclined to seek the cause in 'the Vapour of Water rarified exceedingly by subterraneous Fire and tinged with sulfureous Steams'. Such vapours were widely believed to be the cause of

Plate 13 Halley's chart of the solar eclipse of 22 April 1715 (*British Museum*)

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Plate 13 Halley's chart of the solar eclipse of 22 April 1715 (*British Museum*)

Plate 14a Halley's Comet depicted in the Bayeux Tapestry (*British Museum*)

Plate 14b Halley's Comet and Venus, photographed from the Union Observatory, Johannesburg, in 1910 (*Science Museum, London*)

earthquakes, which also have a way of occurring in groups with long intervals between. The northern lights might then well be 'produced by the Eruption of the pent Vapour through the Pores of the Earth' when the pressure was insufficient to shake the ground or to open a vent. But on this theory it was difficult to explain why the lights should sometimes be visible over an immense area (in the present instance, throughout northern Europe), and why they never appeared in the southern half of the heavens. A more likely agency was 'the Magnetical Effluvia, whose Atoms freely permeate the Pores of the most solid Bodies meeting with no Obstacle from the Interposition of Glass or Marble or even Gold it self'. Descartes had conceived a magnet as the centre of a double circulation of specially-shaped particles passing screw-wise through pores in its substance; and Halley cites the familiar experiment of using iron filings to map the magnetic circulation in the neighbourhood of a globular lode-stone. Now the Earth is a great magnet (or, as Halley asserted, *two* magnets, a core and an outer shell), and it is therefore the centre of a circulation of 'subtile matter' which

> no otherways discovering it self but by its Effects on the Magnetick Needle, wholly unperceptible and at other times invisible, may now and then, by the Concourse of several Causes very rarely coincident, and to us as yet unknown, be capable of producing a small Degree of Light; perhaps from the greater Density of the Matter, or the greater Velocity of its Motion: after the same manner as we see the Effluvia of Electrick Bodies by a strong and quick Friction emit Light in the Dark: to which sort of Light this seems to have a great Affinity.

This theory would explain why (as Halley supposed) the lights are seen only in the northern heavens, and more often in Iceland and Greenland (near the magnetic poles) than in Norway. The erect position of the luminous beams was an optical consequence of the vapours rising everywhere perpendicular to the Earth's surface. The apparent convergence of some of the beams into a pyramid, and the formation of the corona, were effects of perspective; each observer had his own corona. And if the beams rose sufficiently high, they would catch the direct light of the Sun and show a characteristic coloration. Halley also thought the motion of the lights from east to west was an effect of the Earth's rotation.

In seeking an explanation of the northern lights, Halley could thus think of no other possible agency besides water vapour and magnetic effluvia; and 'entities are not to be needlessly multiplied'. The lights bore no resemblance to any other known phenomenon except perhaps the tails of comets; but these visitants were evidently excited by the heat of the Sun, whereas the 'meteor' in question was commonly seen only in the polar regions and in winter. Another line of argument was suggested by Halley's conception of the Earth as consisting of an inner core with an outer shell. If the core was to be habitable, the interspace should be occupied by some luminous medium. And in his paper on nebulae, in the same number of the *Transactions*, Halley had shown reason to suppose that huge tracts of space *are*, in fact, occupied by a shining medium. What more likely than that some portion of this medium may 'transude through and penetrate the Cortex of our Earth, and being got loose may afford the Matter whereof this our Meteor consists'. The Earth being flattened towards the poles, the outer shell might be thinner in that region and the more likely to afford a passage to the luminous vapours which show such a marked preference for the northern heavens.

In their accounts of auroral displays which occurred early in the following year, Edmund Barrell and Martin Folkes pointed out that the vertex of the auroral arch seemed to be displaced westward from the geographical north. Halley, too, seems to have noticed this auroral displacement in the display of 1726 and to have related it to his magnetical theory of the phenomenon. Extracts from the Journal Book of the Royal Society quoted by L. A. Bauer (*Terrestrial Magnetism* (1913), *18*, 121) contain the following passage:

> Dr. Halley related a material circumstance observed in the late aurora borealis, which serves to confirm him in his former opinion, that the magnetical effluvia of the Earth are concerned in the production of the phenomenon, and that was from the situation of the luminous arch in the north and the tendency of the motion of the *striæ*, both which seemed to have a dependence upon the magnetical virtue. He said the arch was highest in that place where it crossed the magnetical meridian, and the *striæ* had a motion with an inclination like that of the magnetical dipping needle (10 November 1726).

It was later and independently observed by John Dalton, the chemist, that the auroral arches were symmetrically situated with regard to the magnetic meridian, and that the associated luminous beams were parallel to the local magnetic field as indicated by the dipping needle.

Halley did not employ the term 'aurora borealis' in his paper of 1716, though other observers were using it at this period; but he introduced it into the title of a paper describing another glimpse of the aurora which he enjoyed on 10 November 1719 (*Phil. Trans.* (1719), *30*, 1099f.). He had risen about 5 a.m. to observe a conjunction of Jupiter with a star in the constellation Virgo when he noticed transient white streaks in the sky converging on a luminous canopy situated about 14° to the south of the zenith. Similar appearances were visible on the evening of the same day. Other papers in the same number of the *Transactions* described the display as observed elsewhere in England and Ireland.

4 *Venus in Daylight*

During the summer of 1716 the planet Venus was visible in broad daylight for many days together. As there was a danger that the apparition would be treated as a 'prodigy' and popular fears exploited, Halley resolved to 'prevent the Superstition of the unskilful Vulgar' by furnishing a mathematical explanation of this comparatively rare phenomenon (*Phil. Trans.* (1716), *29*, 466ff.). He set himself the problem 'To find the Situation of the Planet in respect of the Earth, when the Area of the illuminated part of her Disk is a Maximum.' By the area is meant the angular size as seen by the terrestrial observer. He assumes:

(1) That such an area varies inversely as the square of the planet's distance from us.
(2) That if Sun, Venus, Earth form a triangle SVE (Fig. 24), with the external angle at V equal to θ, then the area of the illuminated part stands to the area of the whole disc of Venus in a ratio given by $\frac{1}{2}(1-\cos\theta)$. (This expression is the *phase* of the planet and its derivation is explained in the textbooks.)

(3) That in the triangle SVE, if SE$=m$, SV$=n$, and EV$=x$, then

$$\frac{(n+x)^2-m^2}{4\,nx}$$

also reduces to $\frac{1}{2}(1-\cos\theta)$, as may readily be proved. Hence, at all distances, the area of the illuminated part of the planet is proportional to

$$\frac{(n+x)^2-m^2}{4nx^3} \quad \text{or} \quad \frac{n^2+2nx+x^2-m^2}{4nx^3}$$

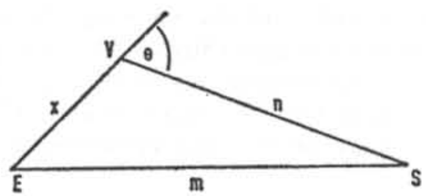

Fig. 24 Daylight visibility of Venus.

'Now that this should be a Maximum, it is required that the Fluxion thereof be equal to O,' which is equivalent to our procedure for finding the maximum (or other stationary point) of an expression by differentiating it and equating to zero. Thus (using Newton's notation of dotted letters for differential coefficients),

$$\frac{2\dot{x}n+2x\dot{x}}{4nx^3}-\frac{3\dot{x}\,(n^2+2nx+x^2-m^2)}{4nx^4}=0$$

$$\therefore\ 2nx+2x^2=3n^2+6nx+3x^2-3m^2$$

$$\text{whence}\quad x=\sqrt{3m^2+n^2}-2n$$

This leads to a geometrical construction for finding the relative positions of the Sun, the Earth, and Venus for which the planet exhibits its maximum illumination. Halley worked the problem numerically and he arrived at the conclusion that the uncommon brightness of the planet would recur every eight years; but by assuming implicitly that the orbits of the Earth and Venus were concentric, coplanar circles, he probably over-simplified matters.

Halley noticed that Venus, at maximum brightness, cast a shadow and outshone the combined light of all the other stars simultaneously visible with her, 'an irrefragable Argument to prove that the Disks of the fixt Stars are unconceivably small, and next to nothing; since shining with a native Light, so many of them do not equal the reflex [reflected] Light of one quarter of a Disk of less than a Minute Diameter'.

Chapter 13

The Pioneer of Stellar Astronomy

1 *New and Variable Stars; Nebulae*

It was known to astronomers in Halley's day that the stars do not all shine with an unvarying brightness; and there were already recorded instances of the appearance in the heavens of 'new' stars situated where (it was supposed) none had been visible before. Variable stars, as is now known, are of several different types. Some fluctuate in brightness with extreme regularity in periods of a few days; some, the long-period variables, go through less definite cycles occupying several hundred days, while others, the irregular variables, exhibit no recognizable periodicity at all. The typical 'new' or, more strictly, 'temporary' star, often called a *nova*, is not a new creation in the heavens but results from an enormous and rapid increase in the brightness of some previously known star, often a faint object or even one invisible to the unaided eye. Ptolemy records that Hipparchus of Rhodes observed a new star in the second century B.C. and that he was thereby prompted to construct the earliest historic star catalogue as a check on any future phenomena of the kind. And the earliest long-period variable to be recognized was Mira Ceti (the 'wonderful star' in the constellation of the Whale), discovered by David Fabricius in 1596.

In 1715 two short papers on this branch of astronomy were published in the *Philosophical Transactions* (1715), *29*, 226, 354. The first, in Latin and extracted from the *Miscellanea Berolinensia*, was by the German astronomer Gottfried Kirch, a former pupil of Hevelius; it reported his recent discovery of a hitherto unknown variable star in the constellation Cygnus ('in collo Cygni'), with notes by Halley on some observations he had since made of the object. Halley's own paper, printed later in the

same volume, is essentially a 'history' of new and variable stars discovered up to his time.

Owing, no doubt, to the growing vigilance of astronomers, Halley could find more records of changes among the stars in the immediately preceding century and a half than in all earlier ages. There was Nova Cassiopeiae, famous in the history of astronomy, which appeared in 1572, and the comparable orb which blazed forth from the foot of Serpentarius in 1604. 'These two seem to be of a distinct Species from the rest,' writes Halley, anticipating the classification of these objects into two types, the *novae* and the *super-novae*, and correctly relegating these two historic outbursts to the brighter class. However, Halley devotes most of his space to the long-period variables. The most remarkable was still Mira Ceti. It had been found to perform seven of its light-cycles in about six years, not always returning to the same maximum brightness; it was never totally extinguished, and though occasionally invisible to the naked eye it could always be picked up in a telescope. Next on Halley's list come two irregular variables, one in the breast of the Swan, observed in 1600 by Jansonius, which had fluctuated between the third and the sixth magnitudes; and one spotted by Hevelius in 1670 which had now disappeared from view. Last comes Kirch's long-period variable in the Swan's neck, to which reference has already been made. It was fainter than Mira and visible for a shorter period during its cycle; and it must have been visible when Bayer was charting that part of the heavens for his *Uranometria* of 1603, for he denoted it by the Greek letter Chi and assigned it to the fifth magnitude. It was in 1686, while he was looking for Hevelius's variable of 1670, that Kirch noticed the variability of Chi Cygni. His observations suggested that the variable kept to a period of about a year, a month, and a week, or just over 400 days; it can rise to about the fifth magnitude but does not always attain to the same maximum brightness. Halley observed 'Kirch's star' in 1714 and 1715, and he determined its position in the heavens.

From the new stars Halley turned to the nebulae:

> But not less wonderful are certain luminous Spots or Patches, which discover themselves only by the Telescope, and appear to the naked Eye like small fixt Stars; but in reality are nothing else but the Light coming from an extraordinary great Space in the Ether; thro' which a

lucid Medium is diffused, that shines with its own proper Lustre (*Phil. Trans.* (1716), *29*, 390ff.).

In this passage Halley grasped the physical nature of one important class of nebulae, anticipating the labours of Herschel and Huggins. 'Some of these bright Spots discover no sign of a Star in the middle of them; and the irregular Form of those that have, shows them not to proceed from the illumination of a Central Body'. Was this the answer to the riddle of Creation: How could light be created before the Sun?

Halley lists six nebulae with their supposed discoverers and dates of discovery (historians of astronomy would amend these particulars as indicated):

(i) The great nebula in Orion, Huygens, 1656 (Cysat, 1619).
(ii) The great nebula in Andromeda, Boulliaud, 1661 (Simon Marius, 1612).
(iii) A nebula between the head and the bow of Sagittarius, A. Ihle, 1665.
(iv) A nebula near the star Omega Centauri discovered by Halley at St Helena in 1677 (a globular star cluster).
(v) A nebula discovered by Kirch in 1681 near the right foot of Antinous, later included in the constellation Aquila.
(vi) A nebula in Hercules discovered by Halley in 1714 (a globular star cluster).

Halley doubted not that many more nebulae remained to be discovered. They appeared small, but being situated at distances comparable to those of the fixed stars, 'they cannot fail to occupy Spaces immensely great, and perhaps not less than our whole Solar System'; and throughout these spaces there prevails 'perpetual uninterrupted Day'.

2 *The Proper Motions of Stars*

The plane of the Earth's annual orbit round the Sun intersects the celestial sphere in the great circle of the *ecliptic* which the Sun appears to trace out in the course of a year. The plane of the Earth's equator intersects the sphere in the *celestial equator*. These two great circles intersect at an angle known as the *obliquity of the ecliptic;* their inclination to each other determines seasonal

changes. It was known in Halley's day that the obliquity had suffered a slow diminution through the ages owing to a change in the plane of the ecliptic; and since the ecliptic is the primary circle of the system of celestial longitudes and latitudes, the position of a star expressed in these co-ordinates should show a corresponding alteration. In particular the latitude of stars should increase or decrease according to their situation on the celestial sphere. And, generally speaking, the expected changes had become evident since the time of Ptolemy. But Halley discovered that the latitudes of certain stars had undergone a change in the opposite direction to that expected and could not therefore be regarded as merely reflecting a shift in the axis of reference.

This discovery arose out of an attempt that Halley made to re-determine the rate of precession of the equinoxes, the angular speed with which the equinoctial points, the two intersections of the equator and the ecliptic, travel round the latter (*Phil. Trans.* (1718), 30, 736ff.). He proceeded by comparing the declinations of certain stars in his day (their angular distances north or south of the celestial equator) with their values as determined by Hipparchus and recorded by Ptolemy; and he concluded that in 1,800 years the longitudes of the stars had increased by some 25° giving a precession of about 50 seconds per annum. 'But while I was upon this Enquiry [writes Halley], I was surprized to find the Latitudes of three of the principal Stars in Heaven directly to contradict the supposed greater Obliquity of the Ecliptick, which seems confirmed by the Latitudes of most of the rest; they being set down in the old Catalogue, as if the Plain of the Earth's Orbit had changed its Situation, among the fixt Stars, about 20′ since the time of Hipparchus. . . .' The three stars were the following (rough values of their longitudes and latitudes are given to locate them on the sphere): Aldebaran (called Palilicium, 65°, 5° south), Sirius (100°, 40° south), Arcturus (200°, 30° north). With such co-ordinates, the south latitudes of Aldebaran and Sirius should have decreased since Ptolemy, but they had, in fact, increased. The change in the obliquity should have made little difference to Arcturus, but, in fact, its north latitude had decreased by about half a degree. Halley's words are worth quoting:

The three Stars Palilicium or the Bulls Eye, Sirius and Arcturus do contradict this Rule directly: for by it, Palilicium being in the days of Hipparchus in about 10 gr. [degrees] of Taurus ought to be about 15 Min. more Southerly than at present, and Sirius being then in about 15 of Gemini ought to be 20 Min. more Southerly than now; yet *e contra* Ptolomy places the first 20 Min. and the other 22 more Northerly in Latitude than we now find them. Nor are these errors of Transcription, but are proved to be right by the declinations of them set down by Ptolomy, as observed by Timocharis, Hipparchus and himself, which show that those latitudes are the same as those Authors intended. As to Arcturus, he is too near the Equinoctial Colure, to argue from him concerning the change of the Obliquity of the Ecliptick, but Ptolomy gives him 33′ more North Latitude than he now has; and that greater Latitude is likewise confirmed by the Declination delivered by the above said Observers. So then all these three Stars are found to be above half a degree more Southerly at this time than the Antients reckoned them. When on the contrary at the same time the bright Shoulder of Orion has in Ptolomy almost a degree more Southerly Latitude than at present. What shall we say then? It is scarce credible that the Antients could be deceived in so plain a matter, three Observers confirming each other. Again these Stars being the most conspicuous in Heaven, are in all probability the nearest to the Earth, and if they have any particular Motion of their own, it is most likely to be perceived in them, which in so long a time as 1800 Years may shew it self by the alteration of their places, though it be utterly imperceptible in the space of a single Century of Years.

Halley found some supporting evidence for the motion of Sirius in the observations of Tycho Brahe, and for that of Aldebaran in an approach of the Moon to the star as observed and recorded at Athens in A.D. 509. Some doubt remained in his mind as to whether the obliquity had really changed by 20′ since Ptolemy's day; but as for the anomalous behaviour of these three stars, 'this Argument seems not unworthy of the Royal Society's Consideration, to whom I humbly offer the plain Fact as I find it, and would be glad to have their Opinion'.

Closely related to the fundamental process of determining the places of the stars is the problem of establishing the instants at which the Sun passes through the equinoctial and the solstitial points. The interval between two successive passages of the Sun through one of these cardinal points serves to determine the length of the tropical year, the all-important period which brings

back the seasons in due succession. It had generally been considered easier to determine the instant of an equinox than of a solstice, for at an equinox the Sun's declination (its angular distance from the equator) is changing with maximum rapidity, and the instant when it momentarily vanishes should be well defined. Ptolemy had determined such equinoxes; and Flamsteed had devised a procedure for doing so which still bears his name. Halley contended that it was easier to determine the true time of a solstice than of an equinox. True, the Sun's declination changes so slowly near a solstice as to make the instant when it passes through its maximum value difficult to determine. However, he had devised a method for fixing the time of the solstice by measuring the length of the noontide shadow cast by an upright stake on at least three days just before and after the solstice; and his procedure had the advantage of needing no correction for refraction or for solar parallax, nor a knowledge of the latitude of the observer and the obliquity of the ecliptic, as did the equinoctial observations (*Phil. Trans.* (1695), *19*, 12ff.).

3 *The System of the Stars; Stellar Parallax*

In Halley's day it was generally believed by natural philosophers that space extended without limit in all directions; but it remained a matter for debate whether the stars were distributed throughout the whole of this infinite space or whether they were restricted to a finite portion of it. Newton's authoritative views on this problem were made known in the course of his historic correspondence with Richard Bentley in the winter of 1692-3 (*Correspondence of Isaac Newton*, iii, Letters 398, 399, 403, 406). He argued that, if all the matter in the Universe were uniformly distributed throughout a finite region of space, it would coalesce under the mutual attractions of its gravitating particles to form a central mass. But, supposing matter to be diffused throughout an infinite space, then two possibilities would arise. If, by divine appointment, the particles were poised in a perfect equilibrium under their mutual attractions and no new motion were introduced, then this configuration would subsist eternally. Otherwise, the matter would coalesce, not into one great mass but into an infinity of such masses scattered at great distances apart throughout the whole of space. Thus might the Sun and stars have been formed; but the separation of incandescent from dark matter to form Sun and planets, and the adjustment of speeds to orbits in our system, were held to afford evidence of divine intervention.

In a paper devoted to the discussion of this problem, Halley inclined to the view that the stars were distributed throughout the whole of infinite space: 'The System of the World, as it is now understood, is taken to occupy the whole Abyss of Space and to be as such actually infinite' (*Phil. Trans.* (1720), *31*, 22ff.). This doctrine seemed to be confirmed by the discovery of ever fainter and more distant stars with every improvement of the telescope. Were the system of stars finite, though never so extensive, it would occupy no assignable proportion of 'the infinitum of Space, which necessarily and evidently exists'; and it would be surrounded on all sides by an 'infinite inane' (or void space). Under these circumstances the stars near the boundary of the system would move inwards under the gravitational attraction of those nearer the centre and would eventually unite with them to form a single mass. If, on the other hand, the system were of infinite extent, the individual stars would be subjected to attractions from all directions; and each of them (so Halley supposed) would remain at rest or move into a position of equilibrium.

Against this view two considerations had been urged. In the first place, it would imply that the number of the stars was greater than any finite number and that two points could be more than a finite distance apart; and this would seem to be absurd, seeing that all numbers are composed of units (Bentley had felt the force of this consideration). However, this argument could be used (or so Halley supposed) against the possibility of eternal duration, which cannot be completed by any finite number of days. Again,

> if the number of Fixt Stars were more than finite, the whole superficies of their apparent Sphere would be luminous, for that those shining Bodies would be more in number than there are [square] Seconds of a Degree in the area of the whole spherical Surface. . . . But if we suppose all the Fixt Stars to be as far from one another, as the nearest of them is from the Sun [and presumably of equal instrinsic brightness]; that is, if we may suppose the Sun to be one of them, at a greater distance their Disks and Lights will be diminish'd in the proportion of Squares, and the Space to contain them will be increased in the same proportion; so that in each Spherical Surface the number of Stars it might contain will be as the Biquadrate [fourth power] of their distances. Put then the distances immensely great . . . and . . . it will be found, that as the Light of the Fix'd Stars diminishes, the intervals between them decrease in a less proportion, the one being as the Distances, and the other as the Squares thereof, reciprocally. Add to this, that the more remote Stars, and those far short of the remotest, vanish even in the nicest Telescopes, by reason of their extream minuteness; so that, tho' it were true, that some such Stars are in such a place, yet their Beams, aided by any help yet known, are not sufficient to move our Sense.

In this passage Halley seems to have confused the linear and the angular dimensions of the stars: the total solid angle subtended by the heavens remains constant while the star discs appear smaller in proportion to the squares of their distances; on the other hand, the actual sizes of the stars are to be presumed equal even though the capacity of successive spherical shells centred on the observer increases as the square of the radius.

In the paper immediately following, Halley considers the question of what we should call the 'pack' of the stars in space (ibid., 24ff.). He found how many stars could be arranged on the surface of a sphere having the Sun at its centre, so that the interspaces between them should be equal to their common distance from the Sun. There was room on the sphere for 12 stars with some space to spare, or not quite enough for 13, these stars being arranged rather in the manner of the twelve vertices of the icosahedron (the twenty-faced regular solid). The number 13 is comparable with the number of stars indisputably of the first magnitude; and though the actual arrangement of these in the sky is somewhat irregular, it seemed reasonable to regard them as our nearest stellar neighbours. A second sphere concentric with the first but with twice the radius would have four times the surface area and might accommodate 4×13 or 52 stars, forming a second order of brightness; and successive spheres of radii 3, 4, 5, . . . 10 times the radius of the first might be expected to contain 9, 16, 25, . . . 100 times as many stars as the first, or 117, 208, 325, . . . 1,300 stars. This tenfold increase of distance might, Halley thought, reduce a star from the first to the sixth magnitude—an interesting anticipation of Sir John Herschel's discovery that, on an average, a star of the first magnitude *is* about one hundred times as bright as one of the sixth. A sphere having one hundred times the unit radius could accommodate 130,000 stars each having one ten-thousandth part of the light of a star of the first magnitude. It seemed doubtful whether the eye, however assisted, could perceive such a star; 'but 100 times the distance of a Star we see, is still Finite: from whence I leave those that please to consider it attentively, to draw the Conclusion'.

Halley, then, grasped in some degree the argument that, if the whole of infinite space were uniformly sown with stars comparable to the Sun, the whole sky would be at least as bright as the Sun's surface. That, in fact, the sky is dark at night is known as 'Olbers's Paradox' after the German astronomer Heinrich Olbers, who discussed this problem about a century after Halley.[1] Olbers believed in an infinite and probably star-filled universe, though he rejected the argument that a finite assemblage of stars would necessarily fall in to the common centre to form a single mass. For, after all, the planets of our system do not coalesce with the Sun, and they would not do so even if there were no surrounding stars. Halley had been at fault in postulating only gravitational forces and neglecting 'projectile forces' (which could maintain a system of stars in revolution about a common centre such as is indeed suggested by their proper motions). Olbers cleared up Halley's confusions about the luminosity of the night sky; but the paradox remained. If a uniformly star-filled space be divided up into concentric spherical shells of equal thickness and having the observer at their common centre, then the stars comprised in successive shells cover equal areas of the sky and send equal amounts of starlight to the observer; and if there are an infinite number of such shells the sky should be infinitely bright. This would still be true even if the stars were regularly organized into systems. Halley had given a false explanation of why the sky is dark at night; Olbers preferred to assume that starlight suffered a slight absorption as it travelled through a tenuous cloud of gas filling all space, which would render all stars beyond a certain distance invisible. Sounder

[1] *Astronomisches Jahrbuch* (1826), 110ff.; *Gesammelte Werke*, Berlin, (1894-1909), i, 133ff.

physical ideas have rendered this explanation untenable: the energy absorbed by such a cosmic cloud would raise it to a temperature at which it would send out as much radiation as it received. Modern attempts to resolve Olbers's Paradox are related to contemporary cosmological doctrines. The stars are conceived as organized into systems comparable to the galactic system of which the Sun is a member; and the problem is then to reconcile the paucity of radiation from the night sky with a broadly uniform distribution of these 'extragalactic nebulae' throughout space. The most straightforward explanation refers to the so-called 'expansion of the Universe'. The light coming to us from the distant nebulae is reddened in a manner suggesting that they are receding from us at speeds proportional to their distances. In consequence this light loses part of its energy; while nebulae so remote that their speeds of recession approach the speed of light are on the point of ceasing to communicate with us altogether, and thereafter we receive no further radiation from them. In some such way the radiation from the stars may be reduced to the observed level of intensity.[1]

Viewed from the Earth, even the nearest star subtends an angle much too small for the human eye to resolve. However, as we have just seen, astronomers long continued in the belief (persisting at least into the eighteenth century) that the stars present the appearance of small but measurable discs. Galileo, indeed, examining the stars through his newly-invented telescope, found that the instrument deprived them of their surrounding fringes of rays, so that they seemed to be magnified correspondingly less than other celestial objects. But they still appeared to him as discs inviting numerical estimation; two centuries later these discs were pronounced by Herschel to be 'spurious' and thereafter explained as due to the wave-structure of light. Halley was so far ahead of his time as to contend that the apparent sizes of the stars were due to an optical illusion, 'the great strength of their native Light forming the resemblance of a Body, when it is nothing else but the spissitude of their Rays'. This passage comes from a paper in which Halley criticized the claim of the French astronomer Jacques Cassini to have deter-

[1] See H. Bondi, *Cosmology*, second edition, Cambridge, 1960, 19ff.

mined the distance and size of the bright star Sirius (*Mém. Acad. R. des Sci.* (1717), 256ff.).

Cassini had commenced in 1714 to observe the daily transit of Sirius across the meridian, using a three-foot telescope fixed in direction so that the star's image normally ran along a horizontal wire in the field of view. Sometimes Sirius was just above and sometimes just below the wire. These fluctuations seemed to show something of the seasonal rhythm that an annual parallax of the star should exhibit; and Cassini, estimating the angular breadth of the star as 5″ and that of the wire as about the same, deduced a parallax of 6″ for Sirius. The star's latitude being 39° 33′ south, its distance from us came to about 22,000 times the diameter of the Earth's orbit.

Halley questioned Cassini's claims on three grounds (*Phil. Trans.* (1720), *31*, 1ff.). The apparent diameter of Sirius he held to be an optical illusion; even the brightest stars vanished instantly when the Moon passed in front of them and did not take several seconds to fade out as they should do if their diameters were in any reasonable proportion to that which Cassini had assigned to Sirius. Again, a three-foot telescope seemed a small instrument with which to measure a parallax of 6″. Lastly, the apparent altitude at which Sirius crossed the meridian was subject to a correction of nearly two minutes of arc on account of refraction; and this correction could itself vary according to the barometric pressure by 7 or 8 seconds, which was more than the alleged parallax amounted to, 'the Refractions being always proportional to the density of the Medium, as we have all seen it often demonstrated by Mr. Hauksbee, both in Vacuo, and in a doubly and trebly condensed Air'. Halley thought an attempt might be more hopefully made upon the parallax of the star Vega which has the advantage of a greater celestial latitude and which passes within one degree of the zenith at Paris, thus suffering but little refraction.

In determining the parallax of a planet it was convenient to have available the exact places of a selection of stars situated in the part of the heavens through which the planet was to pass about the time of its opposition and capable of serving as markers for defining its position as seen from various places of observation. In preparation for an opposition of Mars due to

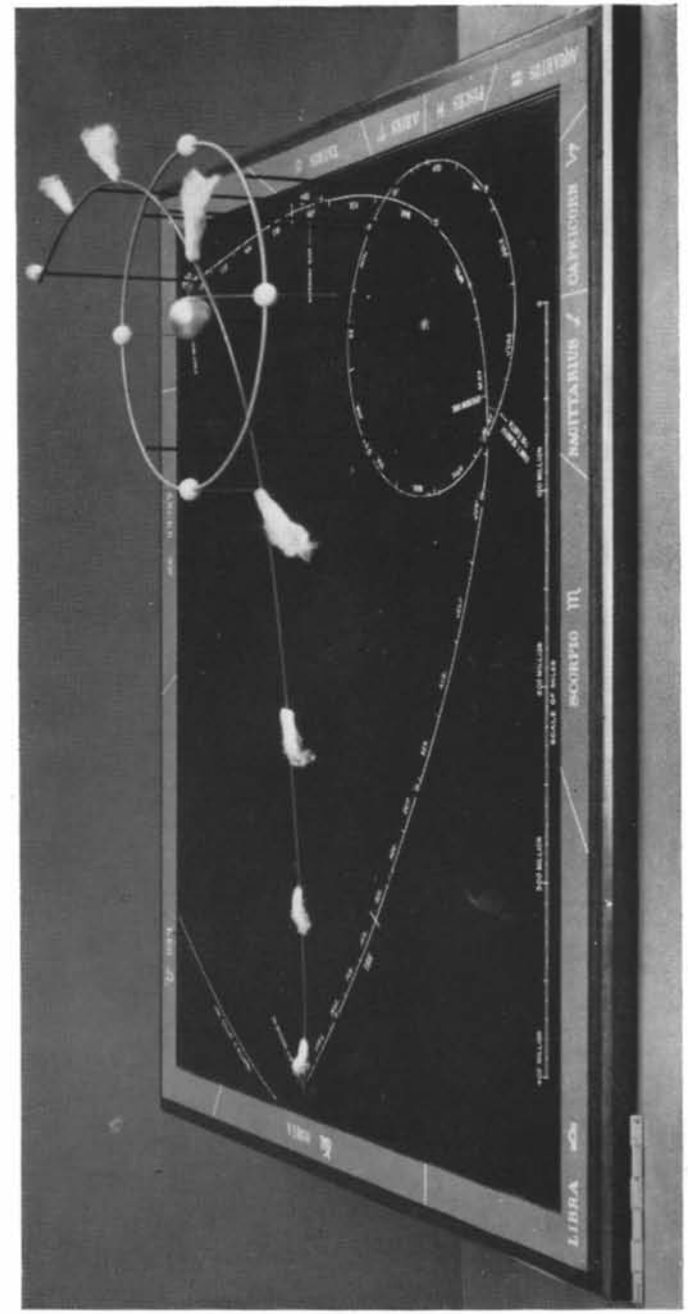

Plate 15 Model of part of the orbit of Halley's Comet, showing its relation to that of the earth about the sun (*Crown Copyright reserved, Science Museum, London*)

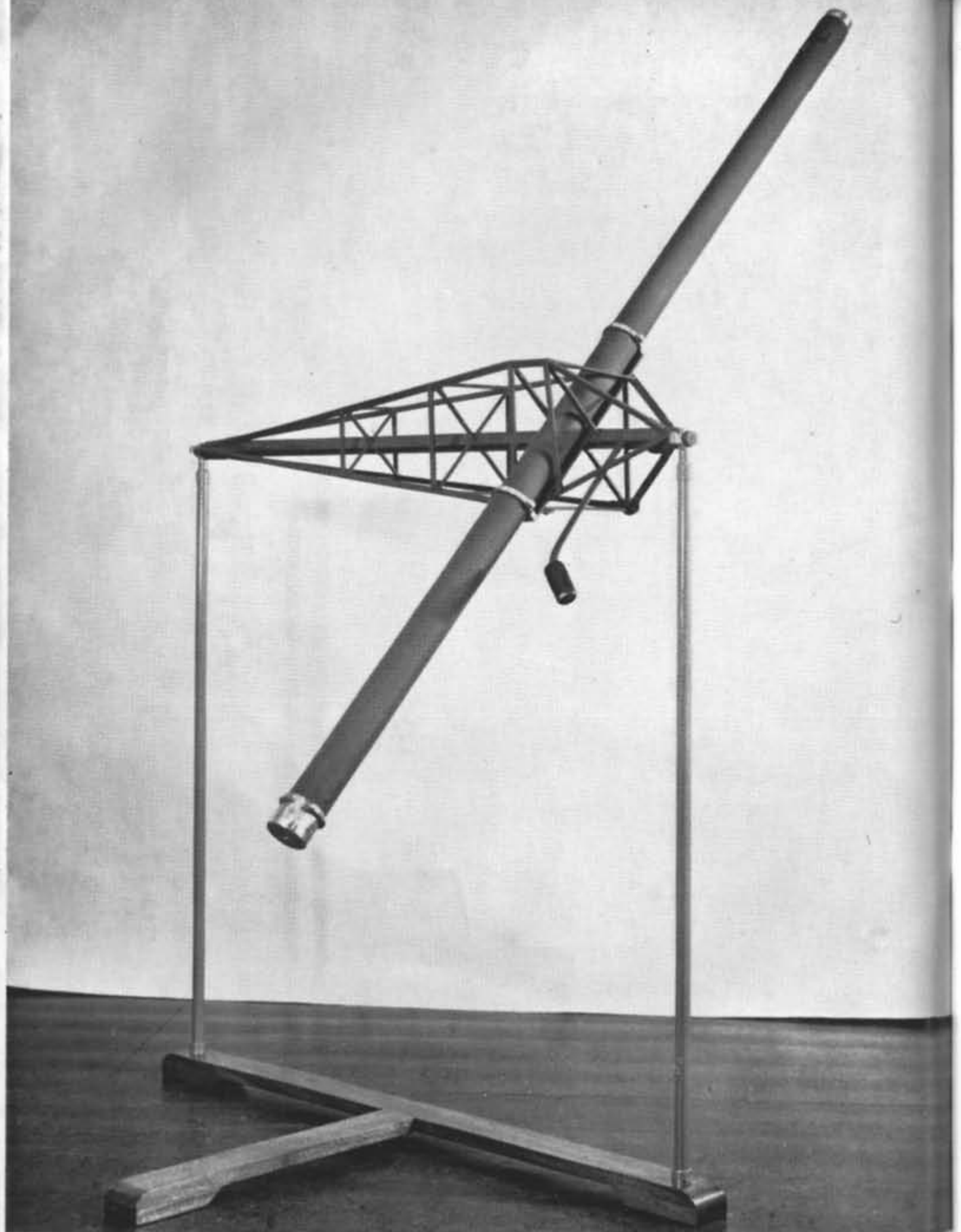

Plate 16 Transit instrument used by Halley from 1721 to 1725 (*Trustees of the National Maritime Museum*)

take place in October 1721 Halley determined and listed the places of eight stars near which the planet would then be passing and which were not given in Flamsteed's catalogue (*Phil. Trans.* (1720), *31*, 113). For this purpose he adopted a procedure introduced by Jean-Dominique Cassini and more recently employed, on occasion in Halley's presence, by the astronomer James Pound and his nephew James Bradley. The field of view of a telescope was divided up by four threads in the focal plane intersecting one another at angles of 45°. To determine the differences in the co-ordinates of two stars, A and B, close together on the sphere, A was allowed to run along the horizontal thread while B followed on its parallel course (Fig. 25). The difference of their right ascensions was given by the difference of their times of transit across the vertical thread; and the difference of the declinations could be deduced from the time interval between the transits of B across the two diagonal threads; the further B was north or south of A, the longer was this interval. In this manner the places of a number of small stars could be linked up with that of a standard star or planet whose right ascension and declination at the time of observation had been determined absolutely.

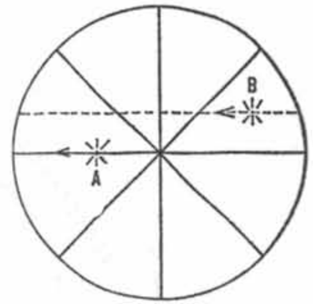

Fig. 25 Finding the angular separation of two stars.

For the purpose in hand, Halley established his preparatory list of the places of the selected stars by comparing them with the known positions of Venus and, independently, of Mercury at times when these bodies were traversing that portion of the zodiac. He thought that if the places of the fainter stars could be catalogued for the whole of the zodiacal belt this would enable twilight determinations of the positions of Mercury or of a comet in the Sun's vicinity to be made, by reference to such stars, with the same certainty as if one were observing far from the horizon, whereas for want of such a catalogue absolute measurements had to be made near the horizon, and these were rendered precarious by the presence of low-lying vapours producing irregular refractions.

4 *Atmospheric Refraction*

When a ray of light passes from an (optically) rarer to a denser transparent medium it is deviated towards the perpendicular drawn to the surface of separation of the two media at the point of incidence of the ray. The Earth's atmosphere can be regarded as consisting of concentric spherical layers of air becoming progressively denser as the Earth's surface is approached; and a ray of light from a celestial body follows a characteristic curve in travelling to the eye of a terrestrial observer (Fig. 26). The star's apparent place in the sky, as seen by the observer, depends only upon the direction in which the ray actually enters his eye;

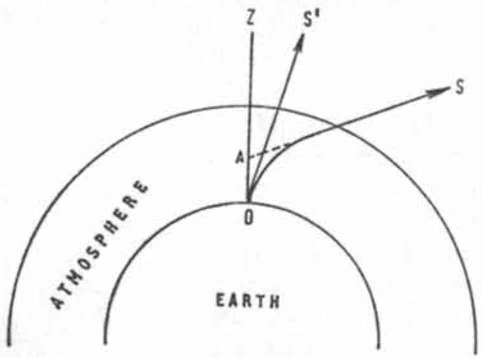

Fig. 26 Atmospheric refraction. Light from the star S enters the atmosphere from the direction SA, but owing to refraction the observer O sees the star in the direction OS'.

and the effect of this phenomenon of *atmospheric refraction* is to make a star appear at a greater elevation above the horizon than it would if the Earth possessed no atmosphere. With every improvement in the precision of his instruments, it became more urgent for the astronomer to know what correction to apply in order to convert the apparent zenith distance ZOS' of a star S into the true zenith distance ZAS. Since the sixteenth century this refinement has been effected by means of *refraction tables* which serve to show, for each stated apparent altitude, the appropriate correction for refraction. Such tables were at first constructed from ingeniously combined observations; later some theoretical basis for them was sought.

The ancients were aware of the phenomenon of refraction; but

the first astronomer to take serious account of it was Bernhard Walther of Nuremberg, who flourished about 1500. In the late sixteenth century Tycho Brahe drew up the earliest empirical refraction table. He believed that refraction was not operative right up to the zenith, but faded out at some arbitrary elevation; and his table of solar refractions differed from the stellar one, being modified to include corrections for a grossly exaggerated solar parallax. No progress could be made on the theoretical side until the sine law of refraction (Snell's Law) had been announced by Descartes in 1637. In 1662 J.-D. Cassini calculated a refraction table on the assumption that the Earth was enclosed by an atmospheric layer of limited height and of uniform density throughout. The results agreed well with observation up to a zenith distance of nearly 80°, despite the artificiality of Cassini's hypothesis. Further progress awaited the discovery of Boyle's Law, which serves to determine the variation of atmospheric density with height, under isothermal conditions. Even then the precise connection between the density and the refracting power of air was the subject of conflicting hypotheses, though the practical difference between them was of little importance for observations not far from the zenith. It became generally accepted that refracting power (refractive index—1) is proportional to the density of the air; already Jean Picard had connected diurnal and seasonal variations in the refraction with differences in the atmospheric temperature.[1]

In 1721 Halley communicated to the Royal Society a refraction table which he declared to have been drawn up by Newton and 'such as I long since received it from its Great Author' (*Phil. Trans.* (1721), *31*, 169ff.). The table extends over the range of apparent altitudes from 0° to 75°. Halley does not state how it was constructed, whether purely from observations or by calculation; and light was first thrown upon this problem when Francis Baily published certain correspondence between Newton and John Flamsteed, the Astronomer Royal (F. Baily, *An Account of the Rev. John Flamsteed*, London, 1835, 139ff., from which the following quotations are taken).

[1] See C. Bruhns, *Die astronomische Strahlenbrechung in ihrer historischen Entwickelung*, Leipzig, 1861.

Already in his *Principia* of 1687 (Book I, Section xiv), Newton had explained refraction as a consequence of an attraction exerted by a medium on the corpuscles of light; and on this hypothesis he traced the path of a light-ray passing through the successive layers of the Earth's atmosphere and calculated the amount of the resulting refraction, employing, it would seem, methods of approximate quadrature. Writing to Flamsteed on 17 November 1694, Newton enclosed a table of refractions which he had computed with the aid of some of Flamsteed's observations. There are three parallel columns giving the refractions for midwinter, for midsummer, and for a middle degree of heat corresponding to autumn and spring: 'You may communicate this table to Mr. Halley, if you wish,' he wrote. However, on 4 December, Newton wrote, 'The table of refractions I sent you I do not design to publish. 'Tis not so accurate as it may be made.' Flamsteed replied (6 December) 'I am glad I did not impart your table of refractions to any body . . . since you were not pleased to impart the foundations on which you calculated it to me.' Newton writes on 20 December, 'The foundation of the table of refractions I concealed not as a secret, but omitted through the haste I was in.' He then gives the calculation in full: it is based upon the arbitrary assumption that the density of the atmosphere decreases uniformly in proportion to increase of distance from the Earth's centre. However, Newton had second thoughts: 'The theorem of refraction I sent you has this fault that it makes the refracting power of the atmosphere as great at the top as at the bottom' (15 January 1695). And again: 'To make a new table of refractions has taken up almost all my time ever since the holidays: and I have hitherto lost my pains in fruitless calculations, by reason of the difficulty of the work' (26 January). Newton turned to the hypothesis of the constitution of the atmosphere set forth in the *Principia* (Book II, Prop. 22), where the density of the air is everywhere proportional to the pressure, and the refractive power proportional to the density: 'I have now finished the table of refractions, and send you enclosed a copy of it' (15 March 1695). Baily found no trace of this table; but James Ivory asserted that analysis of the figures 'fully establishes that Halley's table is no other than the one which Newton computed on the supposition that the densities in the atmosphere are proportional to the pressures' (*Phil. Trans.* (1838), 183).

The first *published* investigation of astronomical refraction on Newtonian physical principles was that of Brook Taylor in his *Methodus Incrementorum* of 1715; he established the differential equation of the path of the refracted light-ray but found the construction of a refraction table beyond his powers. Newton had surmised that 'the rarefaction and condensation of the air by heat and cold seems to have a much greater hand in the phenomena of refractions, than we are yet aware of' (F. Baily, op. cit., 151). The rule connecting refraction with temperature was given later by Tobias Mayer. The two sets of corrections, for barometric pressure and for temperature, were embodied in Lacaille's empirical refraction table of 1755.

Halley supplements Newton's table with some notes on the incidence of astronomical refraction and on the history of its investigation. He points out that the angular separation of any two stars is affected by refraction, which raises both along vertical circles converging towards the zenith. When the stars are at approximately equal elevations above the horizon their horizontal distance apart is contracted by refraction by about 1″ per degree, be they high or low in the sky. This contraction increases as the arc joining the stars becomes more steeply inclined to the horizon, particularly when they are at a low elevation. Halley recalls how Hevelius, to demonstrate the excellence of his sextant, measured the separations of eight stars distributed round the sky, and showed that the sums of the successive differences of right ascension and of celestial longitude each amounted to exactly 360°. But the impression made upon Halley was the opposite of what Hevelius intended, for the totals should have fallen short of 360° by at least six minutes of arc; probably the scale of the sextant wanted about one minute of its correct length.

Chapter 14

The Astronomer Royal

1 *Halley at Greenwich*

UPON the death of John Flamsteed, Halley was chosen to succeed him as Astronomer Royal; the appointment was dated 9 February 1720. Among the supporters of his candidature was the Lord Chancellor, Sir Thomas Parker, later Earl of Macclesfield, one of the little band who had gathered at Crane Court to observe the total solar eclipse of 1715. Halley found the Royal Observatory 'wholly unprovided of Instruments, and indeed of every thing else that was moveable', all having been (quite lawfully) removed by the executors of his predecessor. It was over eighteen months before he could begin serious work: his first recorded observation was made on 1 October 1721. In the following November he resigned his Secretaryship of the Royal Society.

Having obtained a Treasury grant of £500 to spend on equipment, Halley, in 1721, installed at the Observatory a *transit instrument*, probably the first to be seen in Britain. This type of instrument consists essentially of a telescope fixed at right angles to a horizontal, east–west axis. With the rotation of the axis, the telescope traces out the meridian, to which it is restricted. The instrument is designed primarily for timing the passage of a star across the meridian, the exact instant of the transit being determined by noting when the star image crosses an illuminated wire (or wires) in the field of the telescope. This observation serves either to determine the sidereal time or (if that is known) to ascertain the right ascension of an uncatalogued star or other celestial object. If, at the same time, the elevation of the telescope is noted, this gives the meridian altitude of the star, enabling its secondary co-ordinate, the declination, to be estimated. The transit instrument appears to

have been invented towards the close of the seventeenth century by the Danish astronomer Olaus Römer, following his return to Copenhagen from Paris, where he had been closely associated with Picard and Cassini at the Observatoire. The more elaborate *transit circle*, equipped with reading-microscopes for estimating the declination and also devised by Römer, still forms part of the fundamental apparatus of the typical observatory.

Halley's transit telescope was $5\frac{1}{2}$ feet long and $1\frac{3}{4}$ inches in aperture; and, according to Lalande, it had been constructed by Robert Hooke (Pl. 16). The axis of rotation was correctly set by adjusting one of the bearings in which the pivots of the axis rotated and testing with a spirit level. The axis of the telescope was set perpendicular to the axis of rotation by adjusting the cross-wires in the field of the instrument until their intersection continued to cover the same distant meridian mark even when the telescope was reversed upon its bearings like a theodolite (see R. Smith, *A Compleat System of Opticks*, Cambridge, 1738, ii, 321ff.). Halley's meridian mark was 'on the park wall, near Admiral Hosier's house'. For night observations the central wire of the telescope (there appears to have been originally only one wire in the field) was illuminated by means of a candle shining through a piece of horn covering an aperture in the tube's side.

In 1725 Halley installed at Greenwich a large *mural quadrant*; this was the handiwork of George Graham, one of the greatest of eighteenth-century instrument-makers (Fig. 27). It consisted of a graduated brass arc 8 feet in radius, mounted on an iron framework which was bolted to a freestone wall so as to lie in the plane of the meridian with its limiting radii respectively horizontal and vertical. It was traversed by a telescope, pivoted at the centre of the arc and turning in its plane, and it was used in conjunction with a clock to determine the right ascensions and declinations of heavenly bodies in the same way as a transit circle (see R. Smith, op. cit., ii, 332ff.). It could take the transit of any object between the zenith and the south point. Halley began, but had not the funds to complete, the construction of a similar instrument designed to cover the northern quadrant of the meridian. The quadrant was allowed to become seriously out of repair and adjustment during the latter part of Halley's reign at Greenwich.

Halley's equipment also included two clocks and a 'monitor'; the latter was a time-piece audibly ticking out seconds, so enabling the observer to estimate, by the 'ear-and-eye method', the precise instant at which a star crossed the upright wire in a transit instrument. The clocks of the period had the property of

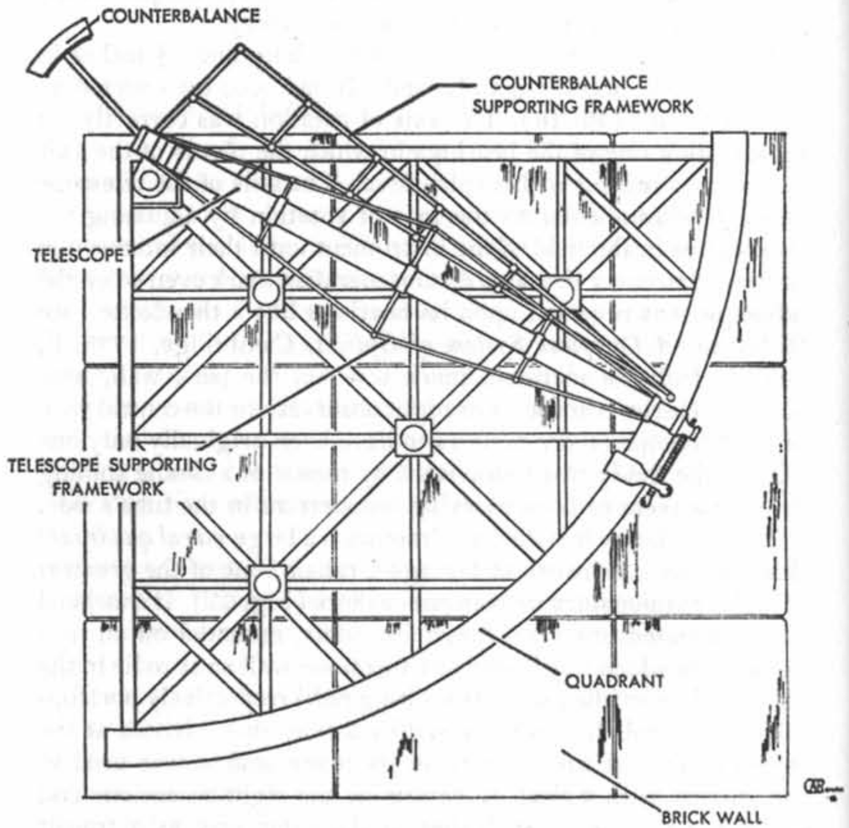

Fig. 27 Mechanism of Halley's mural quadrant.

stopping when they were being wound; for this reason and so that they might be a check on one another, it was desirable to have several in use. Halley's clocks appear to have had no compensation for variations in temperature; but shortly after his death this was supplied in the form of 'gridiron' pendulums of John Harrison's invention.

During his years of service at the Royal Observatory, Halley concentrated mostly upon timing transits of the Moon across the meridian, with a view to improving the existing lunar tables and thus promoting the determination of longitude at sea. Nearly a century after his death, the manuscript registers of his day-to-day observations at Greenwich, contained in four quarto volumes, were examined by Francis Baily, a profound student of the history of fundamental astronomy. Baily found these records badly and sometimes confusedly written and intermixed with computations and extraneous matter. It was not always clearly stated with which instrument an observation had been made; and it was difficult to establish a regular rate for the clocks for any length of time. On one occasion a clock had stopped during an observation; twice the irregular going of a clock was attributed to the pendulum bob's striking against the clock case. Baily concluded that no useful purpose would be served by reducing and publishing Halley's observations, and that star places in his day would be better determined by working back from a modern catalogue. At his suggestion, however, a fair manuscript copy of the registers was made and deposited with the Royal Astronomical Society in 1832. Thus the great bulk of Halley's Greenwich observations remain unpublished. However, three of them were communicated to the Royal Society and printed in the *Philosophical Transactions*. They relate to a solar eclipse on 27 November 1722 (*Phil Trans.* (1722), *32*, 197); a transit of Mercury on 29 October 1723 (ibid. (1725), *33*, 228ff.), and an eclipse of the Moon on 15 March 1736 (ibid. (1737), *40*, 14). The transit, predicted by Halley back in 1691 and now compared with his observation of the transit of 1677, served to establish an improved value for the planet's mean rate of motion and to fix the position of the node where its orbit crosses the ecliptic.

On 2 March 1727, at the last meeting of the Royal Society over which Newton presided, Halley was reminded (as Flamsteed had so often been) that the Astronomer Royal was expected to furnish a fair copy of all the observations he had made in the previous year. However, he excused himself on the ground that (with an eye on the 'great reward' offered by Parliament) he wanted time to finish the theory he designed to build on these observations for the determination of longitude 'before others might take the advantage of reaping the benefit of his labours'.[1]

2 *'The So-much-Desired Longitude'*

The Moon, in performing its monthly circuit of the heavens, occasionally interposes itself between us and some bright star or planet. It can be of scientific value to time such an 'occultation', noting the precise instants of the 'immersion' of the luminary (its disappearance behind the 'limb' or edge of the Moon's disc) and of its subsequent re-appearance or 'emersion'. Such an observation fixes the relative positions of the two objects as they appear in the sky and may help to determine the Moon's orbit. Or if the event is observed from two stations, the difference in the local times of its occurrence serves (after certain corrections) to indicate the difference in longitude of the stations. A star, reduced by its immense distance to a mere point of light, requires but an instant to vanish and later to re-appear, but a planet, possessing a visible disc, takes an appreciable time to fade out and later to regain its former lustre.

The Leipzig Ephemerides of Gottfried Kirch had predicted two occultations of the planet Jupiter by the Moon for the spring of 1686. The first was observed on the evening of 31 March by Halley and Robert Hooke at Gresham College, the planet disappearing behind the lunar disc a few minutes after the two objects had risen over Shooter's Hill. It proved difficult to time the planet's immersion owing to the irregularities of the Moon's limb, where its mountainous surface is seen in profile; but Jupiter occupied about a minute and a third in emerging from its temporary obscuration. The second occultation took place in unfavourable weather on the morning of 28 May; the immersion was observed by Edward Haines, F.R.S., at Totteridge in Hertfordshire, and the emersion in London by Halley, who appealed for particulars of observations made elsewhere of these

[1] See F. Baily, 'Some Account of the Astronomical Observations made by Dr. Edmund Halley, at the Royal Observatory at Greenwich,' *Memoirs of the Royal Astronomical Society* (1835), *8*, 169ff.; and S. P. Rigaud, 'Some particulars respecting the principal instruments at the Royal Observatory at Greenwich, in the time of Dr. Halley,' ibid. (1836), *9*, 205ff.

and similar phenomena, as being of great use for the determination of longitudes (*Phil. Trans.* (1686), *16*, 85ff.).

Halley, who had the problem of the longitude constantly in mind, was convinced that it could best be solved by concerted observations of occultations of stars by the Moon; these celestial events were 'momentaneous' and not subject to the uncertainty attaching to the times of onset of eclipses of the Moon or of Jupiter's satellites. And when the Moon was not too bright, an occultation could be observed on board ship through a small telescope even if the star was a faint one. However, it was of little use finding the longitude of a ship at sea when that of the port for which she was bound remained unknown; and it was to be wished that princes would determine the longitudes of the chief ports and headlands within their realms. But this task would probably be left to the enterprise of private persons; and in the hope of assisting their efforts, Halley, in 1717, published an 'Advertisement to Astronomers' giving notice that, during the next three years, the Moon would pass each month through the star cluster known as the Hyades, in the constellation of the Bull, affording frequent opportunities for concerted observations of occultations of members of the group from widely separated stations. The Moon's motion and parallax were sufficiently known for the necessary reductions to be made. Halley's paper concludes with a catalogue and chart of the Hyades and their neighbours, comprising 23 stars in all (*Phil. Trans.* (1717), *30*, 692ff.).

Failing an occultation, the near approach, or *appulse*, of the Moon or of a planet to a star afforded an opportunity for measuring the angular separation of the object from the star at a stated time and thus of refining upon the tables of lunar or planetary motions. Halley had used this procedure for fixing the position of the comet of 1680. True, the places of the fixed stars had not been determined with all hoped-for precision; but as the star catalogues improved, so the value of such observations would grow. Only a few stars were available for comparison with the planets, and the opportunities for recording such appulses were rare. There was need for a more extensive catalogue covering the region of the zodiac, where the planets move. Upon his appointment to Greenwich, Halley made preparations for supplying the deficiencies of the existing catalogues in this respect; and in a paper on the subject he reported having determined the places of two telescopic stars. One of them was the star to which Galileo saw Jupiter approach in March 1610, as reported in his *Sidereus Nuncius* (the earliest such telescopic observation on record); the other was a star in Scorpio located from an observation of Saturn made in 1662 and recorded by Riccioli (*Phil. Trans.* (1721), *31*, 209).

On 12 May 1720, shortly after his appointment as Astronomer Royal, Halley laid before the Royal Society a plan for enlarging the star catalogue (based on Flamsteed's observations) which he had published in 1712, by inserting the places of all zodiacal stars plainly visible through a 5-foot telescope. Since these stars lie along the track of the Moon in its monthly circuit, the determination of their positions would facilitate finding the longitude at sea by the method of lunar distances. At the same meeting, Newton remarked that his lunar theory had been based chiefly upon observations made at new and at full moon; it was necessary to find the errors at the quadratures and octants of the Moon's path. He suggested what Halley had long had in mind: a study of the discrepancies between the Moon's place as observed and its place as predicted according to Newton's theory, the comparison to extend over an 18-year 'sarotic' period, after which the errors might be expected to repeat themselves. 'And this would be a ready means to examine how much the theory may err from the observations made at any other time' (*Memoirs of the Royal Astronomical Society* (1835), *8*, 171).

Halley's continuing interest in the determination of longitudes was attested by short notices which he communicated from time to time. They refer to the longitudes of Port-Royal in Jamaica (from a lunar eclipse observed by Captain Candler on that station, by Kirch in Berlin and, imperfectly, by Halley at Greenwich), of Cartagena (by Jupiter's first satellite), and of Vera Cruz (from a solar eclipse observed by Joseph Harris). He also reported observations of latitude and magnetic variation taken on board the *Harford* in her passage from Java Head to St Helena in 1731-2. (See *Phil Trans.* (1723), *32*, 235f., 237f.; (1728), *35*, 388f.; (1732), *37*, 331ff.)

3 *Lunar and Planetary Tables; the Long Inequality of Jupiter and Saturn*

In a paper published in 1731, Halley quoted at length from the appendix on the determination of longitude at sea which he had contributed some twenty years before to the second edition of Street's Tables (*Phil. Trans* (1731), *37*, 185ff.). Since that date there had appeared, in 1712, the ill-starred edition of Flamsteed's observations, and, in 1713, the second edition of Newton's *Principia*, which contained a lunar theory (based again upon data furnished by Flamsteed) serving to indicate the positions of the Moon with errors rarely exceeding a quarter of those found in the best lunar tables previously available.

Halley had thus found himself well equipped to take up anew the compilation of a synopsis of discrepancies, this time between Flamsteed's observations and the Moon's places as computed from Newton's theory. He found that for whole months the errors rarely amounted to as much as two minutes of arc (which might represent the limitations of the observer's vision); where they reached five minutes or more was in the Moon's third or fourth quarter, which Flamsteed observed more rarely. It was disappointing that, though Flamsteed had been at Greenwich long enough to have followed the Moon through two of its 18-year periods with plenty of time to spare, he had contented himself with sporadic observations, missing months at a time and, indeed, the whole year 1716. True, 'what he has left us must be acknowledged more than equal to all that was done before him, both as to the Number and Accuracy of his Accounts'; but Flamsteed's observations were not systematic enough to supply Halley with the material for the synopsis he had in mind. Then, in 1720, King George I had bestowed upon him 'the agreeable Post of his Astronomical Observer,' with a special injunction to apply himself to the correction of lunar tables and star places 'in order to find out the so much desired Longitude at Sea, for the perfecting the Art of Navigation'. He had hoped to complete his synopsis but had found the Royal Observatory wholly unprovided with instruments, as elsewhere related. The delay was the more vexatious 'on account of my advanced Age, being then in my sixty-fourth Year, which put me past all Hopes of ever living to see a compleat Period of eighteen years Observation. But, Thanks to God, He has been pleased hitherto to afford me sufficient Health and Vigour to execute my Office in all its Parts with my own Hands and Eyes, without any Assistance or Interruption, during one whole Period of the Moon's Apogee' (of about nine years).

During this period Halley had observed nearly 1,500 meridian transits of the Moon and had compared them, together with above 800 of Flamsteed's transits, with the Newtonian tables. Halley hoped speedily to publish the results, together with other tables already printed off. He hoped to be spared to complete the 18-year 'sarotic' period; but already 'I presume I am able to compute the true Place of the Moon with Certainty, within the Compass of two Minutes of her Motion, during this present Year 1731, and so for the future. This is the Exactness required to determine the Longitude at Sea to twenty Leagues under the Equator, and to less than fifteen Leagues in the British Channel.' Since the Moon travels 360° round the sky in roughly 30 days, it moves through 2 minutes of arc in 4 minutes of time. Halley claimed that his tables were correct to 2 minutes of arc, and provided sailors could measure 'lunar distances' of stars to that order of accuracy, they should be able to tell the Greenwich time to 4 minutes; and that represented twenty leagues (a degree of longitude) on the equator and little more than half as much in our latitudes. The limits within which a ship's position could be fixed thus depended in part upon the accuracy with which the Moon's angular distance from neighbouring stars could be measured. And the invention, just at this period, of John Hadley's sea quadrant (soon to develop into the nautical sextant) afforded grounds for hope that the Moon's place might now be determined at sea with the same 'exactness' as the tables gave it—to within two minutes of arc.

A solution of the problem of the longitude was eventually found to lie in John Harrison's invention of the marine chronometer. Halley was present at a meeting of the Board of Longitude in 1737 at which Harrison demonstrated the first of his famous series of timekeepers, designed to provide an automatic compensation for the effects of a ship's motion, and destined ultimately to supersede the use of 'lunar distances' in the determination of

longitude at sea. Meanwhile, Halley had lived to complete his planned investigation of the Moon's motion through a whole 'sarotic' period.

Ten years after Halley's death there appeared under the editorship of John Bevis, his *Astronomical Tables with Precepts both in English and Latin* (London, 1752). We are told in the Preface how, already in 1717, he had sent to the press a set of tables giving the motions of the Sun, Moon, and planets. They were printed off in 1719; but when Halley was appointed Astronomer Royal in the following year, he deferred publication so that he could compare the lunar tables with the results of the observations that he hoped to make at Greenwich and include the necessary corrections. His plan, as we know, involved observing the Moon through a 'sarotic' period of about 18 years; 'And relying on the uncommon vigour of his constitution he undertook this laborious work himself in the 64th year of his age, and beyond all expectation compleated it'. For the rest he relied on Flamsteed's star catalogue and Flamsteed's all-too-few solar observations. Bradley's revolutionary discoveries of the aberration of light and the principal nutation of the pole came too late to be taken into account. Hence Halley's Tables, when eventually they received posthumous publication, did not mark much of an epoch in the progress of fundamental astronomy.

In the preface to his catalogue of southern stars Halley had referred to the deficiencies of the existing planetary tables, giving, as an example known to astronomers, the progressive and unaccountable changes in the mean motions of Jupiter and Saturn. Saturn was steadily losing speed and Jupiter was showing an acceleration, so that each of these planets departed more and more from its computed place in the heavens. In his (posthumously published) *Astronomical Tables* of 1752, Halley allowed for these anomalies by introducing into the expression for the longitude of each planet a 'secular equation', that is to say, a term (additive for Jupiter and subtractive for Saturn) which increased in value as the square of the time measured from a selected epoch, just as the distance fallen by a stone under the uniform acceleration of gravity increases as the square of the time of fall. In a 'Remark' on his Tables, Halley offered the suggestion that the phenomenon might be due to a gravitational attraction between Saturn and Jupiter: 'it is more than probable that this is owing to the mutual actions of the greater Planets upon one another, disturbing the centripetal forces of the Sun.'

The problem of this so-called 'long inequality' of Jupiter and Saturn at first defied analysis on Newtonian principles despite the prizes offered by the Académie des Sciences for its solution. It was investigated by the youthful Laplace; in 1773 he established that the mean motions of any two planets cannot suffer any permanently increasing changes arising from their mutual attractions. The inequalities of Jupiter and Saturn must accordingly be reversible and periodic. And, in fact, in that same year, J. H. Lambert showed that the relative acceleration of the two planets had lately been reversed. Later, Laplace traced the phenomenon to the near-commensurability of the mean rates of motion of the two planets which causes an almost negligible long-period term in the expression for the disturbing force between them to become considerable when doubly integrated with respect to the time (for the purpose of computing the accumulated displacements of the planets in their orbits), thus giving rise to an important inequality with a period of more than 900 years.

4 *Last Years; the End; Halley the Man*

About 1738, Halley suffered a partial paralysis of his right hand; thereafter he was assisted in his work by one Gael Morris. For the maintenance of the Greenwich instruments he had relied much upon the skill of James Bradley, his fellow Savilian Professor, who was destined to succeed him as Astronomer Royal. Until about a year before his death Halley made a weekly practice of coming up to London by river and meeting a few friends at Child's Coffee-House before the Thursday meeting of the Royal Society. It has even been claimed, though on disputable grounds, that he was the original founder of the Royal Society Club. The supporting document, despite inaccuracies, may preserve an authentic tradition of the astronomer's closing years: 'Dr. Halley never eat any Thing but Fish, for he had no Teeth' (Sir A. Geikie, *Annals of the Royal Society Club*, London (1917), 6ff.). In a letter to Gael Morris, John Machin, the

astronomer, wrote (3 May 1739): 'Dr. Halley comes sometimes to Batson's of a Thursday' (Rigaud, *Correspondence*, i, 341).

Halley continued his observations up to within a few months of his death; then, 'his paralytic disorder gradually increasing, and thereby his strength wearing, though gently, yet continually, away, he came at length to be wholly supported by such cordials as were ordered by his Physician [Dr. Richard Mead], 'till being tired with these he asked for a glass of wine, and having drank it presently expired as he sat in his chair without a groan on the 14th of January 1742 [25 January new style] in the 86th year of his age' (*Biographia Britannica*, iv, 2516). Six days later he was laid to rest, by his own wish, in St Margaret's churchyard at Lee, not far from Greenwich, where his wife had been buried in 1736. Today the old village church is a ruin standing beside a busy highway and the tomb is overgrown with brambles and thistles.

Some idea of Edmond Halley's character and personality can be gained from the study of his writings, even of the brief quotations from his papers and letters scattered through the foregoing pages. A more vivid impression was formed by his contemporaries. His physical appearance was described in the biographical sketch that appeared some fifteen years after his death and to which allusion has already been made: 'In his person he was of a middle stature, inclining to tallness, of a thin habit of body, and a fair complexion, and always spoke as well as acted with an uncommon degree of sprightliness and vivacity' (*Biographia Britannica*, iv (1757), 2517). Halley's personal qualities, his frankness and affability, his sincerity and disinterestedness, his freedom from jealousy and chauvinism, his tact and good judgment, commended him alike to princes and to men of low degree, wherever his travels led him. He made himself acceptable to the succession of contrasting and sometimes contending monarchs under whom he flourished; he maintained cordial relations with men as different and as difficult as Hevelius and Newton, with the extraordinary assortment of characters who made up the membership of the Royal Society, with noblemen and with Admiralty officials. Apart from some initial difficulties for which he was in no way responsible, he made the *Paramour* a happy ship and brought his men home in good health from his hazardous voyages. Even the morose Flamsteed, whose relations with Halley had been unhappy, paid a left-handed tribute to his engaging qualities: 'I hate his ill manners, not the man; were he either honest or but civil, there is none in whose company I could rather desire to be' (Newton's *Correspondence*, iii, 203).

In his political convictions Halley appears to have been a Tory; and even after the Revolution of 1688 he made no secret of his grateful sense of the favours he had received from the fallen house of Stuart. King William III appreciated the political harmlessness of these sentiments, and he did not hesitate to place Halley in command of a warship. Halley fared equally well under Queen Anne and her Hanoverian successors: like Newton, he was 'for the King in Possession'. Yet in an age of place-hunting, he sought neither riches nor patronage; and he refrained from exploiting the great financial interests involved in all that he effected for the improvement of navigation.

Halley was superbly well-informed, his knowledge and interests embracing but extending far beyond the limits of the accepted classical and mathematical curriculum of his time. He was tireless in the pursuit of facts and bold and ingenious in theorizing about them. In an age bedevilled by contending philosophies and prescriptions for the advancement of natural knowledge, he grasped, proclaimed and practised the scientist's role as we understand it today: 'All that we can hope to do is to leave behind us Observations that may be confided in, and to propose Hypotheses which after Ages may examine, amend or confute.'

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