that impels it towards the earth, may be continually drawn aside towards the earth, out of the rectilinear way which by its innate force it would pursue; and would be made to revolve in the orbit which it now describes: nor could the moon without some such force be retained in its orbit.'[138]

Then, through rigid mathematical demonstrations, Newton derived from Kepler's laws the forces determining the motion of the planets. His Proposition I (Cajori ed., p. 40) deals with the law of areas: if a revolving body is subject to a centripetal force directed to a fixed point, the areas described by radii drawn to that point will be proportional to the times in which they are described. For the demonstration, reproduced in Appendix C, Newton made use of equal finite time intervals in which the radius describes a triangle and after each of which the force gives a finite impulse to the body towards the centre. Then he proceeded: 'Now let the number of those triangles be augmented, and their breadth diminished in infinitum; and their ultimate perimeter will be a curved line: and therefore the centripetal force, by which the body is continually drawn back from the tangent of this curve, will act continually.'[139]

In these words we see that behind the geometrical form stands the spirit of his method of fluxions which pervades his geometry; it is the idea of considering quantities and motions not as definite abrupt values but as in process of originating, changing, or disappearing. Newton could be a renovator of astronomy because at the same time he was a renovator of mathematics. In his demonstrations he made use of geometrical figures of straight lines and triangles of finite size; but then he let the number of such parts be augmented and their size diminished *ad infinitum*, to fit a curved orbit and a continually working force; and he showed that the demonstrations then rigidly hold.

By means of the same figure, the reverse was demonstrated: when the succeeding areas are equal for equal time intervals, the working force is always directed to the same point. Thus Kepler's second law of the areas proportional to the time intervals proved that the planets are moved by a force directed towards, hence emanating from, the sun. For the case of a circular motion Newton showed that his method leads to the same formula for the centrifugal force as had been derived by Huygens.

Thereupon, Newton in Proposition XI derived in a general way the law of the centripetal force toward the sun from Kepler's first law that the orbit of a planet is an ellipse with the sun in a focus. By making use of the well-known geometrical properties of the ellipse, he found that the force was as the inverse square of the distance to the sun. Considering the fundamental importance of this demonstration for the history of

astronomy, we have reproduced it in Appendix D (p. 500). The same rate of variation with distance—as shown above—was found by comparing two different planets (supposed, for simplicity's sake, to have circular orbits) and applying Kepler's third law. This meant that different planets at the same distance from the sun have the same acceleration and that hence the attraction exerted upon them by the sun was independent of their substance. These conclusions gave a new significance to Kepler's laws; simple empirical regularities before, they now acquired unassailable certainty as consequences of a universal law of attraction. The attempts made in the seventeenth century to find other orbits or laws of motion for the planets now lost all sense.

The mathematical propositions found their application in the third Book. From observations of the Jupiter satellites it had been derived that Kepler's laws also held for them; hence the forces that kept them in their orbits were directed to the centre of Jupiter and were inversely as the squares of the distances from that centre. The same held for the satellites of Saturn. The planets were attracted in the same way by the sun, and the moon by the earth. The acceleration of falling bodies on the surface of the earth was computed from the orbital motion of the moon to be 15½ Paris feet, 'or, more accurately, 15 feet 1 inch $1\frac{4}{9}$ line'; whereas the same acceleration derived by Huygens from the length of a pendulum oscillating seconds amounts to 15 feet 1 inch $1\frac{7}{9}$ line. 'And therefore the force by which the moon is retained in its orbit becomes, at the very surface of the earth, equal to the force of gravity which we observe in heavy bodies there. And therefore (by Rules 1 and 2) the force by which the moon is retained in its orbit is that very same force which we commonly call gravity; for, were gravity another force different from that, then bodies descending to the earth with the joint impulse of both forces would fall with a double velocity.'[140] By Rules 1 and 2 he means the first of the 'Rules of Reasoning in Philosophy' (*Regulae philosophandi*) at the start of Book III: (1) We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances; (2) Therefore to the same natural effects we must, as far as possible, assign the same causes. These rules in modern times may look superfluous and artificial; but, in a century in which so many fantasies were offered as science, this admonition of intellectual discipline was not superfluous. And he concluded: 'The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can be no other than a gravitating force, we shall hereafter call it gravity. For the cause of that centripetal force which retains the moon in its orbit will extend itself to all the planets.'[141]

The moons of Jupiter gravitate towards Jupiter, the planets towards the sun. There is a power of gravity tending to all the planets; Jupiter also gravitates towards its satellites, the earth towards the moon; all the planets gravitate towards one another. All bodies are mutually attracted by a force between them that moves the greater body a little, the small body much. The weights of bodies towards different planets, hence the quantities of matter in the several planets, can be computed from the distances and periodic times of bodies revolving about them; they are found, if one is put for the sun, to be $\frac{1}{1067}$ for Jupiter, $\frac{1}{3021}$ for Saturn, $\frac{1}{169282}$ for the earth. The force exerted by a celestial body is composed of the attractions of its parts, i.e. of the smallest particles of matter. This universal gravity or attraction, afterwards called 'gravitation', is a general property of all matter; all particles attract one another in accordance with Newton's law of the inverse squares of their distances. Newton demonstrated that the total attraction of a spherical body is exactly the same as though all its mass were concentrated in the centre; Kepler's laws can hold for the planets because they, as well as the sun, are spherical bodies.

The theory of gravitation was not only a more universal formula than the empirical laws from which it had been derived, for it gave in addition explanations for a number of other phenomena. Newton demonstrated that, besides elliptic orbits, parabolic and hyperbolic orbits also led to the same law of attraction, so that by this law each of these conic sections was a possible orbit, with the sun always in the focus. This result could at once be applied to the comets; their mysterious sudden appearance and disappearance were in exact accord with the infinite branches of a parabola or a hyperbola. Kepler had supposed that comets ran through space and passed the sun along straight lines. Cassini had tried, without result, to represent the observations by oblique circular orbits. Borelli, however, in 1664 suspected that the orbits were parabolas. In 1680 a great comet appeared which came close to the sun and, having rapidly made a half-turn around it, went away in the same direction whence it had come. Dörffel, a minister at Plauen in Saxony, explained its course by means of a narrow parabola with a small focal distance.

Newton gave a theoretical basis to these suspicions by stating that the orbits of the comets must be conic sections; he assumed them to be widely extended ellipses of large eccentricity, which at their tops were so nearly parabolas that parabolas could be substituted for them. He indicated a method of deriving the true orbit in space from the observed course between the stars, and he applied it to the comet of 1680. By this method Halley computed parabolic orbits of 24 comets of which two had appeared in 1337, and in 1472, and the others in the sixteenth and seventeenth centuries. In his publication of the results in 1705, he

drew attention to the fact that three among them—the comets that had appeared in 1531, in 1607, and in 1682—had nearly identical orbits in space. Since both intervals were 76 or 75 years, he concluded that they were three successive appearances of the same comet, which in a good 75 years describes a strongly elongated ellipse about the sun. In a memoir of 1716 he returned to the question and pointed to comets that had been seen in the years 1456 and 1378 as possible appearances of the same body, and predicted its next return in 1758.

In his *Principia* Newton also derived the oblateness of a rotating sphere, especially of the earth. In this he had been preceded by Huygens, who, though his first computations in his diary were much earlier, about 1683 wrote a supplement to his discourse on the cause of gravity, sent it to the secretary of the Paris Academy in 1687, and himself in 1690 published both the discourse and the supplement, together with his treatise on light. In this supplement he put forward that, in consequence of the earth's rotation, a plumb line is not directed towards the centre of the earth, but is (in medium latitudes) by $\frac{1}{10}^{\circ}=6'$ inclined to the south. 'This deviation is contrary to what has always been supposed to be a very certain truth, namely, that the cord stretched by the plumb is directed straight toward the centre of the earth. . . . Therefore, looking northward, should not the level line visibly descend below the horizon? This, however, has never been perceived and surely does not take place. And the reason for this, which is another paradox, is that the earth is not a sphere at all but is flattened at the two poles, nearly as an ellipse turning about its smaller axis would produce. This is due to the daily motion of the earth and is a necessary consequence of the deviation of the plumb line mentioned above. Because bodies by their weight descend parallel to the direction of this line, the surface of a fluid must put itself perpendicular to the plumb line, since else it would stream farther downward.'[142]

In an Addendum written in 1690 Huygens computed an oblateness of $\frac{1}{578}$; this value was based on the assumption that gravity as proceeding from the vortices as its cause was constant throughout the body of the earth. Newton, however, now had a better theory; proceeding from gravity as the result of the attraction of all the separate particles, he found it to decrease regularly from the surface to the centre, where it vanishes. So he derived the ratio of the polar axis to the equatorial diameter to be 229 : 230, i.e. an oblateness of $\frac{1}{230}$. These theoretical derivations were strongly opposed by the French astronomers, who put their trust in their geodetical measurements. Careful determinations of the length of one degree of the meridian to the south of Paris had given a somewhat larger value (57,098 toises) than had been derived from the arc of Paris–Dunkirk (56,970 toises). Cassini and his colleagues concluded that the degrees became smaller when going north and that hence the earth must be elongated at the poles. This contradiction between theory and practice made the French astronomers sceptical toward the theory as a whole.

Newton dealt with other astronomical phenomena that now found their explanation in the theory of gravitation. First he pointed out that the attraction of the moon by the sun worked as a disturbing influence upon the moon's orbit and was the cause of the irregularities in the moon's course discovered by Ptolemy and by Tycho Brahe. He gave a first theoretical computation of the regression of the lunar nodes and found that it is strongest in the quarter-moons and zero at full and new moon. Then he showed how the tides are caused by the different ways in which the solid earth and the movable oceanic waters are attracted by the moon and the sun. The precession, that regular slow increase in the longitudes of the stars by a change in the position of the earth's axis of rotation, could also be explained by the attraction of sun and moon upon the flattened earth. By making a comparison with the nodes of imagined moons revolving along the earth's equator, he could even compute the right value 50″ per year (9.12″ by the sun, 40.88″ by the moon).

That the planets by their mutual attraction must disturb their motion he understood, of course, as a consequence of his theory: 'But the actions of the planets one upon another are so very small, that they may be neglected. . . . It is true that the action of Jupiter upon Saturn is not to be neglected . . . the gravity of Saturn towards Jupiter will be to the gravity of Saturn towards the sun as 1 to about 211. And hence arises a perturbation of the orbit of Saturn in every conjunction of this planet with Jupiter, so sensible, that astronomers are puzzled with it.'[143] All the other mutual influences are so slight that he assumed the aphelion and the nodes of the planets to be fixed, or at least nearly so, and he even concluded: 'The fixed stars are immovable, seeing they keep the same position to the aphelia and the nodes of the planets.'[144]

Newton, by his theory of universal gravitation, gave to the knowledge of the motions of the heavenly bodies so solid a basis as never could have been suspected. In this great scientific achievement the two formally opposite principles of Bacon and Descartes are unified: he proceeded from practical experience, from rules deduced from observations by precise computation, and out of them constructed a general theoretical principle which permitted him to derive all the separate phenomena. And all these deductions were demonstrated by the most exact and acute mathematics. No wonder that, after traditions had been vanquished and the difficulties of the new mode of thinking overcome, his

compatriots exalted him as an almost superhuman genius. Honours were bestowed upon him; from 1703 until 1727, the year of his death, he was President of the Royal Society. His appointment in 1696 as 'Warden of the Mint' (in 1699 promoted to 'Master of the Mint') was not simply a post of honour or a lucrative sinecure. He and his colleague, the philosopher John Locke, together with the ministers Somers and Montague, by their energetic measures in minting good silver coin, repaired Britain's deplorable monetary system, a necessary basis for the expansion of its commerce which ensued.[145]

Newton in his *Principia* did not restrict himself to an exposition of his new theory, which for us is the essential thing. For his contemporaries, criticism of the older dominant theory was equally needed. So the entire Book II is devoted to the motion of fluids and to the resistance which moving bodies experience in fluids: the first foundations for a scientific treatment of these phenomena. Here the vortices had to stand the test of science; the progress of half a century was the progress from vague philosophical talk to exact mathematical computation. The conclusion was, in Newton's words: 'Hence it is manifest that the planets are not carried round in corporeal vortices'; and in a verdict still more severe: 'so that the hypothesis of vortices is utterly irreconcilable with astronomical phenomena, and rather serves to perplex than explain the heavenly motions'.[146]

Notwithstanding this crushing criticism of the vortex theory, most Continental scientists remained sceptical towards the doctrine of gravitation. This appears most clearly in what Huygens wrote in 1690 in the above-mentioned Addendum to his Discourse on the cause of gravity. In comparing their different results on the oblateness of the earth, he said: 'I cannot agree with the Principle which he supposes in this computation and elsewhere, viz. that all the small particles, which we can imagine in two or many different bodies, attract and try to approach one another. This I cannot admit because I think I see clearly that the cause of such an attraction cannot be explained by any principle of mechanics or by the rules of motion.'[147] Of course; for in his opinion the weight of heavy bodies was caused by their being pressed down by the whirling ether outside and not through influences from inside the earth, so that the celestial bodies themselves did not act upon one another. He had nothing against Newton's centripetal force, by which the planets were heavy toward the sun, because he himself had shown that such gravity could be understood from mechanical causes. Long ago he also had imagined that the spherical figure of the sun, as well as that of the earth, could be explained by this gravity; but he had not extended its action as far as to the planets, 'because the vortices of Descartes that formerly appeared to me very probable and which occupied my mind, were opposed to it. Nor had I thought of that regular decrease of gravity, namely, as the inverse square of the distance; that is a new and very remarkable property of gravity, for which it would certainly be worth while to seek the reason.'[148]

Here it appears that what to Newton was a solution, to Huygens was a new problem. Perceiving now by Newton's demonstrations that this gravity counterbalanced the centrifugal forces of the planets and exactly produced their elliptical motion, Huygens had no doubt that Newton's hypotheses on gravity were true, as also Newton's system founded thereon. It must appear all the more probable, since it solved many difficulties that gave trouble in the vortices of Descartes; for instance, as to why the eccentricities and inclinations of the planetary orbits always remained constant and their planes passed through the sun, and their motions accelerated and retarded, as we observe, which could hardly happen if they were swimming in a vortex about the sun. And now we see also how the comets can traverse our system; it was difficult to conceive how they could have a movement opposite to the vortex which was strong enough to drag the planets along. But by Newton's theory this scruple has been removed, since nothing prevents the comets from travelling in widely extended ellipses about the sun.

'There is only this difficulty,' Huygens continued, 'that Newton . . . will have celestial space to contain only very rare matter, in order that the planets and comets meet with less impediment in their course. This rarity accepted, it seems to be impossible to explain the action either of gravity or of light, at least in the way I always used.'[149] In 1678 Huygens had already expounded, and in 1690 printed, a theory of light as a vibration, a wave motion propagating through the world ether, and in this way he had explained the phenomena of reflection and refraction. Newton had developed the entirely different theory—which was not published until 1704—that light consists in ejected particles passing through space with great velocity. Refraction of a ray obliquely falling upon a glass surface—which in Huygens's theory was due to slower propagation of the waves—in Newton's theory was easily explained by the consideration that the light corpuscles were bent toward the normal by the attraction of the denser glass matter. There was thus a profound difference in the supposed underlying world structure. For Newton, space was empty or nearly so; the light corpuscles, as well as the planets, run their course unimpeded, and gravity works through empty space from one body to another. Huygens could not agree with Newton's attraction because his theory of light required that space be filled with ether.

So in his 'Addendum' he returned to discussions of the nature, the

fineness, and the tenuity of the whirling particles surrounding the earth. Newton, he said, argued to prove the extreme rarity of the ether in order that the motions due to gravity be not hampered by its resistance; but this substance, instead of hampering the motion, causes gravity. 'It would be different if we should suppose gravity to be an inherent quality of bodily matter. But I do not believe that this is what Newton accepts, because such an hypothesis would remove us far from the mathematical and mechanical principles.'[150] In the same trend of thought, Leibniz, after reading the *Principia*, wrote to Huygens (October 1690): 'I do not understand how he conceives gravity or attraction; it seems that to him it is only a certain immaterial and inexplicable virtue, whereas you explain it very plausibly through the laws of mechanics.'[151]

Here the profound basis of the controversy comes to light. Huygens admitted the exactness of Newton's computations and formulae; but they offered him no explanation. They gave no answer to the questions posed by him and his French colleagues: what is the origin of attraction? why is it that two bodies without any contact are driven toward one another? If space is filled with matter, this matter, by its contact, by pressure and attraction, transfers the motion; we see how streaming water and blowing wind drag objects with them; these are mechanical forces, easily understandable. An attraction from afar, over empty space, is entirely foreign to mechanical action.

Did Newton and his partners not see this difficulty? Certainly they did; but it did not worry them. Fundamentally, Newton, according to the general trends of thought at that time, agreed with Huygens. That he felt the same need of explanation as his contemporaries appears from a letter written in 1678 to Robert Boyle, the master of chemistry and discoverer of the law of the 'spring' of gases. Here he tried to give gravity a cause in the ether pervading all gross bodies and consisting of particles of different degrees of fineness; but his notions about things of this kind, he said, were so undigested that he was not well satisfied with them; 'you will easily discern whether in these conjectures there be any degree of probability.'[152] That he considered attraction at a distance no sufficient explanation may be seen from the letters he wrote (1692–93) to Richard Bentley, who was in correspondence with Newton on account of a series of lectures, in which he (Bentley) demonstrated the existence of God and refuted atheism by means of the law of gravitation. Newton, who was deeply occupied with theological questions and had often written on biblical subjects, in his first letter showed his agreement with this trend of thought: 'Why there is one body in our system qualified to give light and heat to all the rest, I know no reason, but because the author of the system thought it convenient. . . . To your second query I answer that the motions, which the planets now have, could not spring from any natural cause alone, but were impressed by an intelligent Agent. . . . To make this system, therefore, with all its motions, required a cause which understood, and compared together, the quantities of matter in the several bodies of the sun and planets and the gravitating powers resulting from thence . . . and to compare and adjust all these things together in so great a variety of bodies, argues that cause to be not blind and fortuitous, but very well skilled in mechanics and geometry' (letter of December 10, 1692).[153] And in his third letter of February 25th he wrote on the attraction: 'It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon, and affect other matter without mutual contact; as it must do, if gravitation, in the sense of Epicurus, be essential and inherent to it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers.'[154]

In the second edition of his *Principia* (1713), in a 'General Scholium' added to the end of the third Book, to refute the criticisms that he had introduced occult qualities into natural philosophy, the same opinions were expressed in a more reticent way: 'Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centres of the sun and the planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of the solid matter which they contain. . . . But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy . . . and to us it is enough that gravity does really exist and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea.'[155]

That this was not the last word of his Natural Philosophy appears in

the way he then continued: 'And now we might add something concerning a certain most subtle spirit which pervades and lies hid in all gross bodies; by the force and action of which spirit the particles of bodies attract one another at near distances' and electric bodies operate and light is emitted, 'and the members of animal bodies move at the command of the will, namely, by the vibrations of this spirit, mutually propagated along the solid filaments of the nerves. . . . But these are things that cannot be explained in few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the laws by which this electric and elastic spirit operates.'[156] With these words the books of the *Principia Mathematica* close.

These sentences show that his mind was also capable of imaginative flights. But his theory remained entirely free from them. In his theory, only those relations appear which are demonstrable by exact mathematics; this is its essential characteristic. By means of the laws of gravitation, the phenomena can be derived and predicted by computation; this is the purpose of science. We meet here again with the contrast between the practical mind of the English and the theoretical mind of the Continental scientists. The latter racked their brains about the question concerning from what fundamental truths their theories followed. The former did not care and were content if they could work with the theories and derive practical results. Doubtless this was, as already pointed out, a consequence of the general mode of thinking of these peoples, rooted in their living conditions. The same personal liberty and daring energy which in the centuries that followed drove the English middle class towards commercial and industrial world power made the English scholars in their 'experimental philosophy' the pioneers of science.

Pioneers of scientific method, indeed. What in Newton's work presented itself as resignation, not asking for deeper causes but boldly applying it to further results, became the principle of modern science; a law of nature is not an explanation of the phenomena from established primary 'causes'.

CHAPTER 27

PRACTICAL ASTRONOMY

The great extension of oceanic navigation in the seventeenth century brought the need of ever more perfect astronomical data. The harbours in the far continents, as well as the coasts and the islands, had to be explored, surveyed and mapped out by means of astronomical measurements. On the open sea also, for safe navigation, the sailors had to determine longitude and latitude. Finding the geographical latitude was no difficult problem; it could be done by measuring the greatest altitude reached by the sun or by a star in the meridian, provided, of course, that the declination of the celestial object was known. This meant that a good catalogue of stars with their declinations and good tables of the sun must be available. For the sun, the moon, and the planets there were Kepler's Rudolphine Tables, and for the northern stars Tycho's catalogue. But gradually greater demands were made, and the astronomers had to meet them. The governments now felt it their duty to foster astronomy in the interests of traffic.

As to the southern stars invisible in Europe, at first only the rough positions of a good 300 stars were available, as determined by two Dutch sailors, Pieter Dirksz. Keyzer, in 1595, at Bantam, and Frederik de Houtman in 1600, during his captivity in Achin. It was to remedy this defect that the British Government in 1676 sent young Halley out to the island of St Helena, where he succeeded in measuring accurately the positions of 350 southern stars. His results were published as a supplement to Tycho's catalogue, and Hevelius added them to his own catalogue. On this voyage and on some later voyages Halley made a number of meteorological and magnetic observations, constructed a map of the tides, and devised an explanation of the trade winds and monsoons as a consequence of the earth's rotation, which he published in 1686.

The finding of the terrestrial longitude was a far more difficult task; it remained a famous and embarrassing problem down to the twentieth century. The longitude of a point on earth is defined and found as the difference in time between that point and an adopted zero meridian. In the seventeenth and eighteenth centuries, Paris was taken as this

zero point, to conform to France's economic and political rank and its nautical tables. Later on, when England became dominant on the seas, Greenwich Observatory came increasingly into use. Local time can be determined by astronomical measurements of solar or stellar altitudes. The difficulty is how to know on the high seas the standard time of the zero meridian. Galileo had proposed to use the eclipses of Jupiter's satellites, which are seen all over the earth at the same moment, and the States of Holland seriously negotiated with him in 1635. But the tables of their movement were too imperfect for an exact prediction of their phenomena. More promising was the use of the moon which every day progresses in its orbit by nearly 13.2° (i.e. $\frac{1}{27}$ of the circumference of the sky). An error of a few minutes in measuring the longitude of the moon produces an error 27 times larger in the deduced terrestrial longitude of the place of observation; but sailors were content, at the time, if they knew their position to a couple of degrees. Vespucci had already made use of the observed place of the moon to derive the difference in longitude between South America and Spain; Pigafetta, the companion of Magellan, had applied the same method, and in the sixteenth century Werner at Nuremberg and Gemma Frisius at Louvain had recommended it. The French Government of Richelieu in 1634 put up a prize for a good solution of this problem and appointed a committee for the purpose. Morin claimed the prize for his proposal to measure the distance from the moon to several stars and to build for this purpose an observatory to establish by observation the exact course of the moon. The prize, however, was denied him by the committee because the idea was not new and Morin was not able to make practical proposals for its execution. The method became practical only by the further development of astronomical practice in the following centuries.

Tycho had determined his differences in right ascension of the stars by computing them through trigonometrical formulae from distances measured in an east-west direction. So did Halley in St Helena. Earlier, Joost Bürgi at Cassel had tried to measure right ascensions directly by means of the moments the stars passed through the meridian; but this attempt was frustrated by the irregular running of the clocks. Huygens's invention of the pendulum clock solved the problem in 1656. Galileo had already perceived that a pendulum is isochronous, i.e. that (for small oscillations) the oscillation time was independent of the oscillation width. Hence it must be possible to measure durations by the number of equal oscillations of a pendulum, e.g. seconds. Many persons, first Galileo himself, and afterwards his son Vincenzo, tackled the problem of how to combine pendulum and clockwork. Huygens found a practical construction in which the pendulum regulates the turning of the cogwheels and a small impulse from the wheel at every oscillation keeps the pendulum swinging at constant width. The clock, built as an automatic counting apparatus, indicating the number of oscillations, became an accurate instrument for measuring time. This means that the clock became an astronomical instrument, an essential aid in all future astronomical measurements. The instrument of observation most fitted to be used in combination with it, constructed first in a primitive form by Römer at the Copenhagen Observatory, was the transit instrument, consisting of a telescope fixed at right angles on a horizontal axis directed exactly east-west. In revolving the axis, the line of vision of the telescope described the meridian. The moment of transit of a star, as seen through the ocular, across a vertical wire in the focus, indicates the right ascension. One second of time corresponds to as much as 15″ in the sky; but by observing the transits over a number of parallel vertical wires, the error of the time of transit can be diminished to a small fraction of a second (plate 8).

The invention of accurate clocks provided a new means of deriving the longitude at sea; on a timepiece going with exactly the right speed (at least, since the last harbour) the time of the zero meridian could be read directly. A pendulum clock, of course, could not serve the purpose on a rolling vessel; here a portable timepiece regulated by a spring, called a 'chronometer', was needed. Hooke, together with Tompion, 'the father of English watchmaking', had first devised practical constructions, though they were not entirely successful. Then Huygens solved the problem, following the self-same principle as that of his pendulum clock, by introducing the spring balance as a regulator. If such a chronometer, carried by the vessel on a long voyage, should go one minute wrong, this would give an error of only ¼° in longitude.

In the seventeenth century astronomy began to be a government affair. Formerly, in Tycho's time, princes had often endowed astronomical pursuits, which were personal hobbies of single individuals. In the next century, under royal absolutism, astronomy, besides being a personal scientific activity of a class of wealthy enlightened citizens, eager for knowledge, also took the form of state employment. The practical application of astronomy to the needs of navigation and geography induced the rulers to found observatories.

When Picard, in a dedication to the king, with which he introduced his *Ephemerides* in 1664, had pointed out that in France there was no instrument with which to determine latitude, the king in 1667 ordered an observatory to be built in Paris (pl. 8), later to become, at the same time, the seat of the newly-created Académie des Sciences, in which its sessions were held and experiments made. Picard was the leading expert on the practical astronomy then required, the determination of stellar positions. Yet the versatile Domenico Cassini, who had won fame by

several discoveries, was called to Paris to be the first director and to enhance the glory of the prince by new discoveries; Picard became the chief observer. From 1679 onwards he published the *Connaissance des temps*, the first nautical almanac which has continued to our own day, and he felt the need to give it a firm basis of reliable observations. He proposed to have a quadrant made, 5 feet in radius, provided with a telescope, in a fixed position in the meridian to measure altitudes and times of transit. But the ostentatious edifice devoured so much money that the chief instrument was postponed and was not ready until 1683, one year after Picard's death. His successor, La Hire, thereafter used it in a regular observing practice. The observations served to correct the tables and to compute new ephemerides; but they were not collected into a new stellar catalogue, nor were they published, again through lack of money. Preserved in the archives, they could be consulted occasionally for special purposes thereafter.

In England the proposal by a French visitor at Court to have observations made for navigation purposes induced Charles II to ask for a report from the Royal Society. One of the members of the committee appointed was John Flamsteed (1646–1719), who, from his youth on, had occupied himself with astronomical observations. He it was who wrote the report in which was expounded the need of an observatory, where the positions of the celestial bodies could be determined regularly. The king then ordered, in 1675, that an observatory should be built on a hill at Greenwich, part of one of his country resorts. Flamsteed was appointed 'our astronomical observer' with a salary of £100 a year; 'Astronomer Royal' has since remained the title of the directors of Greenwich Observatory. His task was 'to apply himself with the most exact care and diligence to rectifying the tables of the motions of the heavens, and the places of the fixed stars, so as to find out the so much desired longitude of places for perfecting the art of navigation'.[157] There were no instruments. Flamsteed had to provide those himself. From his friend Jonas Moore he could borrow a sextant of 7 foot radius, and in the years 1676–88 he used it to measure distances between many stars. It was Tycho's old method which he used, with the only difference that his sextant was provided with two telescopes with cross-wires, one fixed, the other movable along the graduated arc, so that two observers were needed. Since he could not obtain from the government the money for a better instrument more suited to fundamental work, he constructed, at his own expense and with the aid of his ingenious assistant, Abraham Sharp, a mural quadrant of 7 foot radius to be used in the meridian. It was not really a quadrant but an arc 50° larger, to embrace the entire meridian from the southern horizon to the celestial north pole. The accuracy achieved with this instrument was due in large part to Sharp's skill, for he was expert at making accurate divisions on instruments.

From 1689 Flamsteed, with unremitting assiduity, notwithstanding his frail health, observed right ascensions and declinations of the stars, the sun, the moon and the planets. He was not content with accumulating observations; he reduced them to final results fit for publication. Not until 1725, after his death, however, did this *Historia coelestis Britannica* appear, a catalogue of 3,000 stars, exceeding all former catalogues in number and accuracy. Here the stars were arranged in each of the constellations according to right ascension (not according to longitude, as with Tycho); they were numbered, and these numbers afterwards were used as names of the stars. We speak of the star 61 Cygni, because it was No. 61 of the Swan in Flamsteed's catalogue. Based on this catalogue was an atlas of star maps, first published in 1729 and often reprinted afterwards.

Practical astronomy now became the regular business of specially appointed experts, often state officials, whose duty consisted in making astronomical observations. Though sensational discoveries might occur now and then, the main occupation was patient and devoted routine work. It was, however, a routine that continually renewed itself and struggled to attain greater accuracy through perpetual improvement of instruments and methods. This was the basis of the triumphal progress of astronomy in the following centuries.

The most needed and most promising work was the observation of the rapidly moving bodies—the moon and the planets. It appeared that the Rudolphine Tables did not give an exact course; small deviations showed everywhere. This could be met partly by improved numerical values for the elliptical elements. But did the planets obey Kepler's laws exactly? They did so sufficiently to confirm the truth of these laws; but there were differences. Halley in 1676 remarked that Saturn went more slowly and Jupiter more rapidly than was indicated by the tables; their periods of revolution had changed since Kepler's time. In view of all these deviations which were revealed by new accurate observations, the idea arose that the laws determined an average course only, analogous to the periodic fluctuation of temperature with the seasons, with irregular chance variations superimposed. Fortunately, Newton's law of universal gravitation appeared at the right time to establish that, by the attraction of the sun alone, Kepler's laws would be exactly followed but that, by the attraction proceeding from the other planets, deviations must occur, apparently capriciously, yet determined by natural causes.

The determining of the positions of the fixed stars, that had to serve as the basis of all study of the movements in space, brought to light new

phenomena. Again it was Halley who, in 1718, comparing modern results for the latitudes of Aldebaran, Sirius and Arcturus with the data of Ptolemy, completed by those of Hipparchus and Timocharis, found that they now stood half a degree farther south than they should according to the ancient data. 'What shall we say then? It is scarcely credible that the Ancients could be deceived in so plain a matter, three Observers confirming each other. Again their 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.'[158] So the so-called 'fixed stars' did not occupy a fixed position in the heavens; they had their proper motion in the celestial sphere, hence also in space. This unexpected result provided a new aspect of the world. Here was a new reason for observing the stars again and again, and more and more accurately.

CHAPTER 28

ASTRONOMERS ON THE MOVE

With the extension of navigation over the oceans, expeditions of astronomers now took place to solve special problems. Halley's expedition to the island of St Helena has been mentioned above. The journeys often had their origins in discussions in the newly-founded Academies. A strong initiative proceeded from the Paris Academy because its members, as *pensionnaires* of the king, could appeal to the Treasury for extra expenses.

In 1671 Academicien Richer was sent to the French colony of Cayenne to make observations of the planets and stars, with a special view to the solar parallax. But the expedition is better known for a secondary result than for its original purpose. Richer took with him a pendulum clock that had been well regulated in Paris. After his arrival in Cayenne it appeared that the clock was slow by two minutes per day, and its pendulum had to be shortened by $\frac{1}{380}$ of its length to keep pace with the earth's rotation. He soon understood that this was because gravity was diminished by the centrifugal force of the earth's rotation, which at the equator was stronger than in Paris; after his return to Paris the former length of the pendulum had to be restored. The diminution of gravity by centrifugal force could be computed exactly; it was $\frac{1}{580}$, considerably less than Richer had found. Huygens and Newton considered the difference as a proof of their theoretical result that the earth was flattened at the poles. We mentioned above that the French astronomers contested this opinion and, on the basis of geodetic measurements in France, believed the earth to be elongated at the poles.

When in the first part of the eighteenth century the theory of gravitation began to appeal to French scientists, they understood that the small difference between southern and northern France could not be decisive for the figure of the earth. The Paris Academy resolved to send out an expedition to measure a meridian arc near the equator. If the earth was flattened, the curvature of the meridian must be stronger, hence an arc of one degree must be shorter, the nearer one came to the equator. In 1735 Bouguer and La Condamine went to Peru (the northern part, now called Ecuador), and in the elongated plain of Quito, directed north-

south, they measured an arc of three degrees. Their instructions were to measure, moreover, an east-west-directed arc of longitude; on a flattened earth it must be longer than the north-south degree of latitude, so that, theoretically, the problem could be solved by measurements in one region only. In this land of gigantic north-south mountain chains, the Andes, it was impossible, however, to measure an east-west arc; moreover, longitude differences could not be measured as exactly as latitude differences. Because of many difficulties and dissensions, the astronomers did not return until 1743. But the result they brought back was decisive; at Quito, Bouguer found 56,753 toises for one degree difference of latitude, distinctly smaller than the value of 57,057 found in France.

In the meantime, shortly after their departure from France, Maupertuis proposed to send out a second expedition to the far north, in order to make the evidence still more convincing. Thus in 1736 Maupertuis, accompanied by Clairaut and some other young scientists, went to Lapland. In the vicinity of Torneå, under many hardships from the excessive cold, they measured, partly alongside and partly on the frozen river, an arc of 0° 57′. The result of these measurements, 57,438 toises for an arc of 1°, by comparison with the result for France, also afforded a sufficient proof of the oblateness of the earth. Because Maupertuis returned directly to France in 1738, he could present himself as the man who had demonstrated the truth about the figure of the earth. The results, however, on closer consideration, showed a lack of numerical agreement; from the comparison Torneå-Paris, a flattening was derived of $\frac{1}{114}$; from Quito-Paris a far smaller flattening of $\frac{1}{279}$. Later results established that the latter value was nearly right; the observations in Peru had been made with great care and accuracy. In Lapland the difficulties had been far greater; after a severe winter, when the mercury in the thermometers was frozen and the metal instruments could hardly be touched, the observers went home as soon as possible. A remeasurement in the nineteenth century showed that the difference of latitude, found by Maupertuis, was too small.

Other expeditions in the eighteenth century dealt with the problem of the solar parallax. This is the fundamental quantity determining the distance of the earth from the sun and, hence, all distances and dimensions in the solar system. Tycho Brahe had used the traditional value of antiquity, 3′. Kepler had derived, from Tycho's observations of Mars, that it could not be greater than 1′. About 1630 Vendelinus made another attempt with Aristarchus's old method, this time by using a telescope to determine the exact moment that the lunar disc was bisected by the illumination boundary. He found that it took place when the moon stood at ¼° less than 90° from the sun, an amount 12 times smaller than Aristarchus had given; hence the solar parallax was $\frac{1}{12}$ of Aristarchus's value, i.e. 15″. With the irregular boundary line of the illuminated lunar surface, greater accuracy could not be reached by this method.

Richer's expedition to Cayenne, mentioned above, was made especially to measure the parallax of Mars, which in the autumn of 1672 approached the earth at a distance of 0.37, so that its parallax was nearly three times that of the sun. For this purpose the declination of Mars and of the adjacent stars was measured, while Cassini at the same time made these measurements at Paris. The difference came near to the limit of accuracy then attainable. Cassini deduced that the parallax of Mars could not well be above 25″ and that the solar parallax could not exceed 10″; as a final result, $9\frac{1}{2}''$ was assumed. For the first time the solar parallax had been determined by direct measurements, though with the relatively considerable uncertainty of some few seconds, $\frac{1}{3}$ or $\frac{1}{4}$ of its amount.

The same method was once more used, later on, by the diligent Lacaille, who in 1751 went to the Cape of Good Hope and remained there for two years, observing a large number of southern stars. He also made observations of the solar and the lunar parallaxes, the former by using Mars in opposition, as well as Venus near the lower conjunction. For the lunar parallax he got an accurate value, 57′ 5″. For the solar parallax, however, the European observatories had let him down by neglecting to make corresponding observations, so that his results—10.2″ from Mars, 10.6″ from Venus—were of little value.

In the meantime however, a much more promising method had been disclosed. During his stay on St Helena, Halley had observed a transit of Mercury over the sun. With such a transit the solar disc was the background for the black disc of the planet; its positions relative to the sun, on account of its parallax, must be different for different places on earth. Halley had even attempted to derive a solar parallax from the observed moments of Mercury's ingress and egress. Of course, this was highly inaccurate, and the result, 45″, was entirely valueless. But it led him to the reflection that, if it had been Venus instead of Mercury, the conditions would have been far more favourable.

When Venus in lower conjunction is seen before the sun, its motion relative to the solar disc is so slow that it needs 7 hours to cover the solar diameter and wants 14 seconds of time for a progress of 1″. Its relative parallax, 22″, in medium conditions produces about the same value for the difference in the length of the chords described, which makes a difference of five minutes in the moments of ingress and egress. If the observer should make an error of, say, three seconds in such a moment, it would produce an error in the solar parallax of only $\frac{1}{100}$ of its

amount. No wonder that Halley, when recommending this much superior method in 1691, used these words: that this will be the only kind of observation that in the next century with the highest precision will disclose the distance from the sun to the earth; what was tried in vain with different methods of parallax measurements. In this way the use of subtly graduated instruments would be entirely avoided.

In order to pass across the solar disc in its lower conjunction, Venus must be near to one of the nodes of its orbit. Since five periods of Venus are nearly eight years, there is a lower conjunction again eight years later at a longitude 2.4° different, i.e. at a latitude 8.5′ different, which, as seen from the earth, amounts to a difference of 22′ in apparent latitude. Since the solar diameter is 30′, two successive transits may be visible with an eight-year interval; thereafter the conjunction has moved in longitude too far away from the node. After more than a century a new set of two transits happens in the vicinity of the other node. Kepler

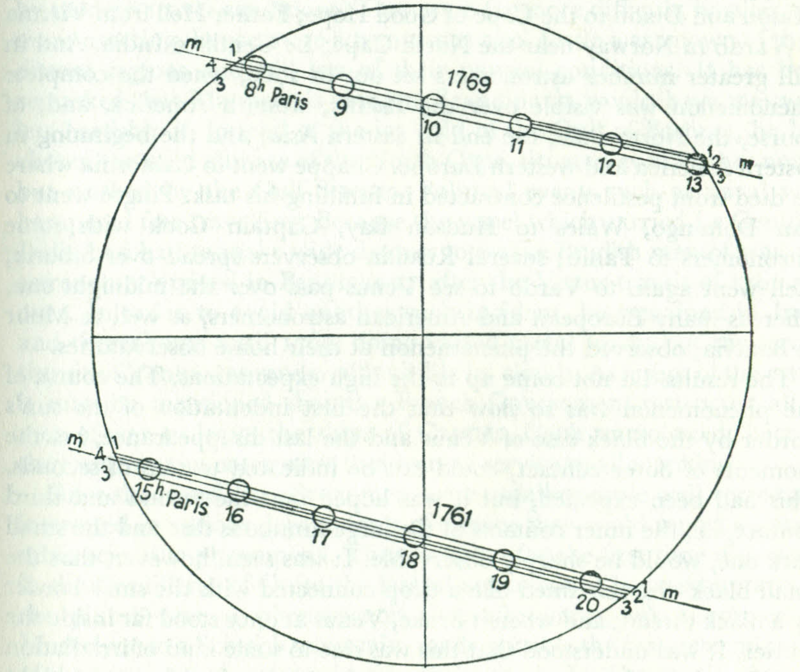


Fig. 29. Transit of Venus across the solar disc
m=track of the planet by its central point

1761:	1. Rodrigues	2. Paris	3. Tobolsk	4. Tahiti
1769:	1. Tahiti	2. Batavia	3. Vardö	4. Paris

had already alluded in vague terms to the special and remarkable character of these phenomena. Its first observation was made on December 4, 1639, by Jeremiah Horrox, who described it enthusiastically in a letter to his friend and collaborator, Crabtree. The next transits on June 6, 1761, and June 3, 1769, were those indicated by Halley, who again emphasized their importance in 1716; he pointed out the desirability of occupying as many observing stations as possible, not only far to the north and the south but also to the east and the west, where only the ingress or the egress would be visible.

When the time came near, his appeal met with a large response. A number of French and English astronomers journeyed to far-distant and little-known places. In 1761 the phenomenon could be seen in its entirety over Asia and the north polar regions; on the Australian islands the beginning only was visible; in western Europe and the Atlantic the end only. Pingré went to the Island of Rodrigues in the Indian Ocean; Chappe d'Auteroche to Tobolsk in Siberia; Maskelyne to St Helena; Mason and Dixon to the Cape of Good Hope; Father Hell from Vienna to Vardö in Norway near the North Cape; Le Gentil to India. And in still greater number astronomers set out in 1769, when the complete phenomenon was visible over the Pacific, western America, and, of course, the North Pole; the end in eastern Asia; and the beginning in eastern America and western Europe. Chappe went to California where he died from pestilence contracted in fulfilling his task. Pingré went to San Domingo, Wales to Hudson Bay, Captain Cook with some astronomers to Tahiti; several Russian observers spread over Siberia; Hell went again to Vardö to see Venus pass over the midnight sun, whereas many European and American astronomers, as well as Mohr at Batavia, observed the phenomenon at their home observatories.

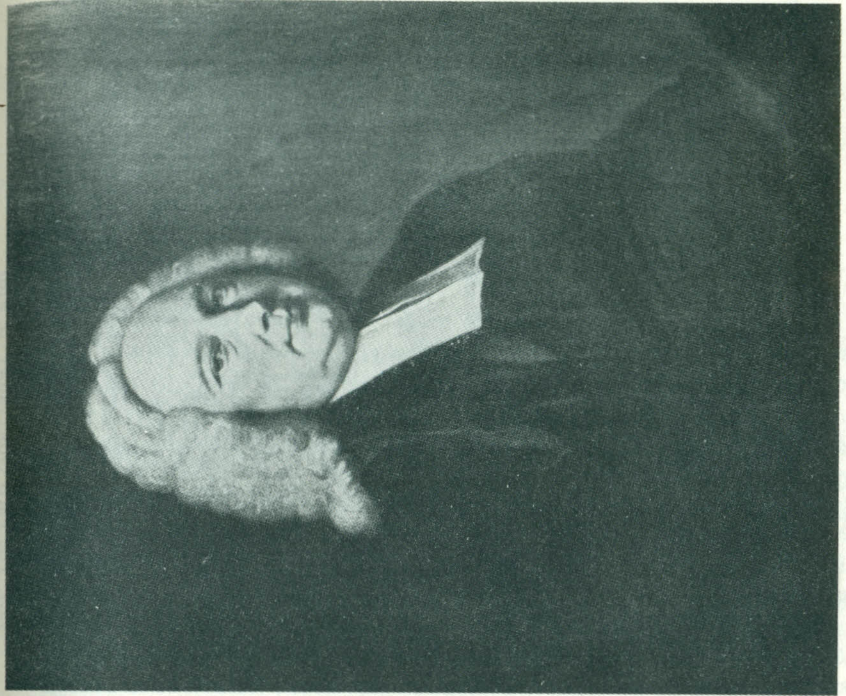
The results did not come up to the high expectations. The course of the phenomenon was so slow that the first indentation of the sun's border by the black disc of Venus and the last disappearance, i.e. the moments of outer contact, could not be indicated to tens of seconds. This had been expected, but it was hoped that the second and third contact, i.e. the inner contacts of the large luminous disc and the small dark one, would be sharply observable. It was seen, however, that the small black disc remained like a drop connected with the sun's border by a black thread; and when it broke, Venus at once stood far inside the border. It was understood that this was due to some kind of irradiation or diffraction. How was one to know, then, what had been the right moment of inner contact? Observers standing beside one another differed by tens of seconds in their estimates, especially when they used different telescopes. Bewildered by the unexpected appearance, they had to be content with noting any time between the different moments.

Father Hell was even suspected, probably unjustly, of having afterwards changed and doctored his noted results.

The results for the solar parallax, deduced by different computers from different combinations of observations, consequently diverged far more than Halley had optimistically expected. Moreover the geographical longitude of a number of stations was badly known and had to be derived by the observers themselves by means of Jupiter's satellites or, in 1769, from a partial eclipse of the sun. Thus all sorts of values between 8.55″ and 8.88″ were computed and published for the solar parallax. This, however, meant enormous progress in our knowledge. Instead of by many seconds, the separate results differed by some tenths of a second only, whereas formerly the solar parallax and the distance of the sun were uncertain to ⅓ or ¼ of their value. So it may be said that the transits of Venus in the eighteenth century answered their purpose completely.

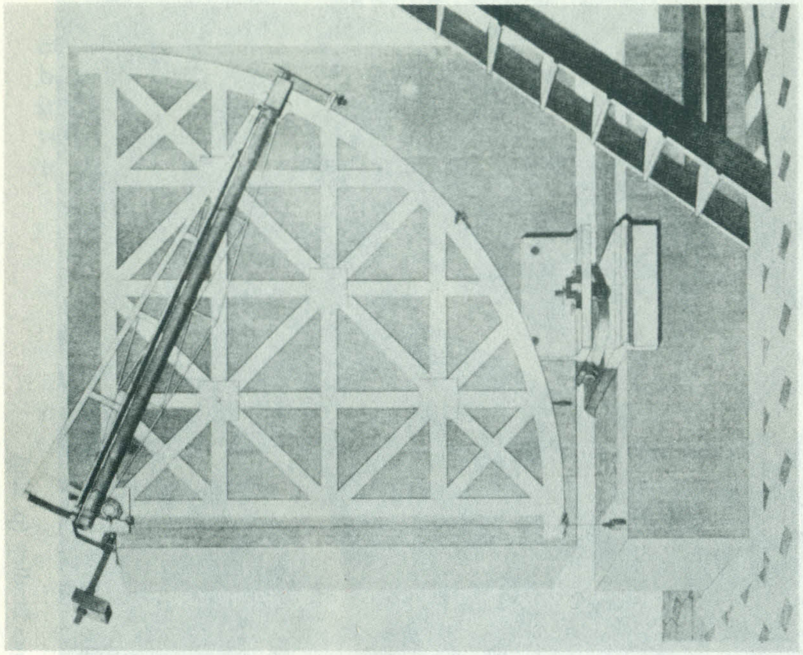
Astronomical expeditions in the eighteenth century cannot be judged by modern travel conditions. They were far more difficult, harsher and more exacting, hence more adventurous also. Little was known of those distant regions and still less of their natural conditions. It has been remarked that Maupertuis going to Scandinavia would have met with less hardship if, instead of the icy cold of the Gulf of Bothnia, he had chosen the mild climate of the North Cape, situated even farther north, but washed by the Gulf Stream. Political events such as naval wars hampered free travelling. Because the vessel which carried Le Gentil to India had had to make wide detours to escape English men-of-war, the astronomer arrived in Pondicherry after the Venus transit of 1761 was over. In order to avoid another such incident, he remained in India and thereabouts until 1769, doing varied useful work; but the second transit of Venus was rendered invisible by clouds. As a sign of the times, it may be mentioned that the French Government instructed all its men-of-war to leave the ships of Captain Cook unmolested, because they were out on enterprises that were of service to all mankind.

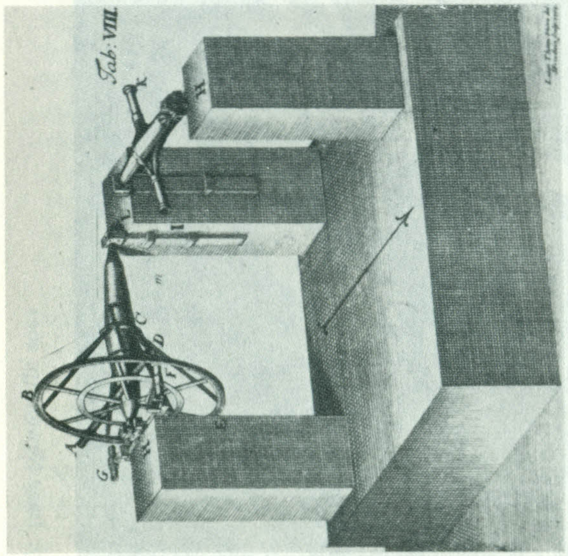
Since the travelling astronomers were usually people well versed and interested in various departments of science they could report on many other scientific phenomena. Thus Bouguer for the first time discovered in the Cordilleras of Quito the lateral attraction of large mountains on the plumb line, a phenomenon that afterwards, when applied by Maskelyne to a Scottish mountain, made possible the first determination of the mass and the mean density of the earth. La Condamine in 1738 gave the first description of the Cinchona tree in Peru, the source of the medicine quinine already known in Europe. Many of these astronomers published diaries and books on their travels and adventures, which found a large audience, just as did the more purely geographical works of discoverers like Cook and De Bougainville. Among the rising bourgeoisie in France, England and elsewhere there was increasing interest in all aspects of science and all knowledge of nature in foreign regions. The astronomical travels, adventures on the earth, and adventures in the heavens satisfied a considerable part of this curiosity.

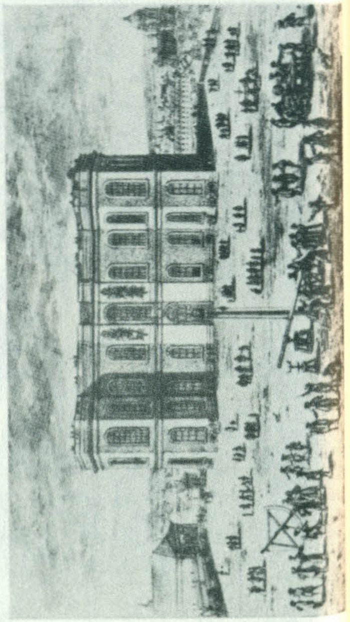


James Bradley (p. 289)

7. Isaac Newton (p. 262)







8. *Top left:* Römer's meridian instrument (p. 278)
Right: Bird's Quadrant (p. 292)
Left: The Paris Observatory (p. 278)

CHAPTER 29

REFINED PRACTICE

THERE was still something wrong with astronomical measurements. Accuracy surely had gradually increased, and the causes of error were more carefully investigated. Atmospheric refraction was a most annoying source of error. Cassini had derived a table of refractions from his observations; Newton had pointed out that the refraction depended on the temperature and the pressure of the air as indicated by thermometer and barometer. Several astronomers and mathematicians sought to improve theoretically and practically our knowledge of its dependence on the altitude of the star. Yet, even when corrected for refraction, the declinations of the stars, determined at Paris or at Greenwich, sometimes showed deviations of more than 10″—just as Picard had formerly perceived. Flamsteed thought that these differences, which seemed to depend on the season, were due to a parallax of the stars; but his colleagues pointed out that the variation in parallax with the season would have been different. By a yearly parallax a star is displaced towards the point of the ecliptic occupied by the sun; the actual deviations had a different character. Hooke had repeatedly measured the zenith distance of a star culminating near the zenith, so that refraction could play no part, and he found variations which he thought might be attributed to a yearly parallax.

In order to verify these results, Molyneux in 1725 had installed on his estate at Kew, near London, an instrument especially adapted to exact measuring. It consisted of a zenith sector comprising a few degrees only of a circle of large radius, 24 feet, erected in the meridian, so that the telescope attached could be pointed at a selected star (γ Draconis, the same as that used by Hooke) when it culminated near the zenith. Thus variations in its declination could be measured very accurately; the measurements were later found to have no greater error than 2″. Soon after the work was started, Molyneux stopped regular observing because he was appointed to the Admiralty; but his work was continued by his younger friend, James Bradley (1692–1762), since 1721 a professor at Oxford, who had taken part in all the preparations. When they started observations of the star in December 1725 they saw to their surprise that, although they had expected it to have reached its southernmost position, it continued to move southward until in March of the next year it had arrived 20″ farther in that direction. Then it returned, until in September it had moved 40″ northward, again to return south, reaching in December the same declination as a year earlier. It was an oscillating movement with the period of a year. But it could not be a parallax of the star, because in that case it would have been farthest south in December. To extend the observations to other stars, Bradley had another sector constructed of smaller radius (12 feet) and greater angular breadth (6° to either side of the zenith). He found that all the other stars showed the same periodical change, the extent being smaller according as they stood nearer to the ecliptic. In 1728 Bradley succeeded in finding the explanation in an apparent 'aberration' of the light rays: because the telescope is carried on by the earth in its orbital motion, whilst the telescope is traversed by the light rays with a 10,000 times greater velocity, it must be kept inclined by $\frac{1}{10000}$ towards the direction of the earth's movement. The discovery of this aberration was the first experimental proof that the earth has a yearly motion and that Copernicus was right.

This was not all. When Bradley continued his observations of γ Draconis during the following years, he perceived that there was a second oscillation: alternately for nine years its declination increased and decreased over a range of 18″. This variation, also occurring with all the other observed stars, was confirmed by information from Lemonnier in Paris. Bradley explained it in 1748 by a 'nutation' of the earth's axis, a small conical movement superimposed as a kind of small perturbation upon the large, slow, conical movement known as the precession. Its period of 18 years, which is the period of revolution of the nodes of the lunar orbit, in which its inclination to the equator oscillates between 18° and 28°, indicated the attraction of the moon upon the flattened earth as the cause of the phenomenon.

'The accuracy of modern astronomy,' said Delambre, 'owes much to these two discoveries by Bradley. This double service secures for him the highest place after Hipparchus and Kepler, and raises him above the greatest astronomers of all times and countries.'[159] Though this praise may sound too lavish, the first sentence is certainly true. So long as the position of the stars could deviate up to 30″ through these causes, it was a hopeless task to assure an accuracy down to less than 10″ by measurements. Astronomers could expect concord and results for the positions of the stars only by correcting the measurements for these two influences, aberration and nutation. Since the differences which now remained were due to unavoidable errors of observation it was worth while to reduce these errors by improving the instruments and methods of observation.

Instruments did indeed improve, chiefly because specialists with expert skill and knowledge began to construct them. Flamsteed had been obliged to make them himself with the aid of Sharp. Greater perfection was reached by George Graham, who had constructed the zenith sectors used by Molyneux and Bradley in discovering aberration and nutation. He used an ingenious method for engraving exact graduations upon his instruments, and he applied a screw for the exact reading of the smallest divisions. When in 1720 Halley succeeded Flamsteed as director of Greenwich Observatory, he found no instruments there; the instruments which Flamsteed had used were his own property and the heirs had removed them all. Halley could now order new instruments at the public expense, and all were provided by Graham. Bird, Graham's younger associate, who became his successor, afterwards made excellent new instruments in his shop not only for Greenwich but for a number of European observatories, so that the new standard of accuracy spread over the entire astronomical world. In 1767 he was given £500 by the Government for publishing in a book, for universal use, his accurate method for precisely graduating circles.

The making of precision instruments developed into a subtle handicraft by expert artisans because, chiefly for navigational needs, a market had developed with a regular demand. To determine the position of a ship on the high seas by measuring solar and stellar altitudes and moon distances, accurate instruments were needed that could be held in the hand on a rolling ship. In medieval times and afterwards people had managed with the cross-staff; but the most skilled pilot could not attain an accuracy greater than several minutes of arc. Newton had devised, about 1700, an instrument through which, by means of mirrors, the rays from two objects were brought into one telescope, each passing through half the objective. It was, however, not published and had not become known. The invention in 1730 by Thomas Godfrey, of Philadelphia,[160] seems to have remained unknown in Europe. In 1731 John Hadley published the description of a 'reflecting quadrant', later for practical reasons reduced to a reflecting sextant, in which the images of two celestial bodies (or of the horizon) were brought by reflection into exact coincidence as seen in the telescope. This sextant became indispensable to every pilot. Its regular manufacture to meet the needs of Britain's rapidly extending navigation of the seas raised instrument-making to a highly skilled and flourishing trade, and astronomy profited from that skill.

Because England in the eighteenth century took increasing precedence in commerce and navigation, the problem of longitude at sea became a matter of public importance. At Newton's suggestion (from 1701 he sat in Parliament as member for the University of Cambridge) the Government offered a high prize (£30,000) for anyone who should discover a reliable method of finding the longitude in the open sea with an accuracy of ½°. Minor prizes were afterwards offered for partial solutions of the problem.

One of the most promising methods was the improvement of timepieces, to keep the time of the zero meridian. Both the pendulum clock and the portable chronometer regulated by spring balances changed their speed with temperature, because of the expansion of the metal parts. Graham, mentioned above as an able instrument-maker, who had been Tompion's apprentice in clockmaking, in 1762 published a method of making the pendulum clock unsusceptible to variations of temperature by replacing the lens of the pendulum by a vessel filled with mercury. His younger colleague, Harrison, at about the same time constructed a gridiron pendulum which offered the same result by a combination of rods of different metals with different expansion coefficients. The same principle was then applied by Harrison to the spring balance of the chronometers, and he succeeded in making their speed nearly insensitive to temperature. After his timepieces had been tested on ocean travels in 1761 and 1765 and their excellence had been proved, he received first £5,000 and later £10,000, on condition that he should publish a description of his construction methods. Not only in England, but also in France, in part independently, able artisans, among whom J. A. Lepaute and Ferdinand Berthoud were the most famous, constructed ever better timepieces for nautical use.

The fact that the construction of instruments and clocks in England had now reached a level of fine precision had its consequences for technology as a whole. It was in the second half of the eighteenth century that the invention and continual improvement of spinning machines by Arkwright, Hargreaves, Crompton and others inaugurated the industrial revolution in England. In the history of technology it is pointed out that many of these inventors and would-be inventors had acquired their mechanical skill as apprentices in the clockmaking trade.[161] From the clockmakers they obtained their ability to realize their ingenious ideas in practical tools. Thus the needs and the practice of astronomy indirectly contributed to the rise of machine techniques in industry.

After Halley's death in 1742, Bradley was appointed to Greenwich Observatory. The observations he made with the instruments available did not satisfy him; so he ordered better ones to be made by Bird. They were a transit instrument with an 8 foot telescope, for the determination of right ascensions, and a mural quadrant of 8 foot radius for the declinations (plate 8). With these instruments, from 1750 until his death in 1762, he made extensive series of observations of the stars, the sun, the moon, and the planets, which surpassed in accuracy all

former work. Even more than to the precision of the readings, this accuracy was due to his carefulness in determining or eliminating the systematic errors proceeding from the instrument or from the condition of its surroundings. The small inclination of the horizontal axis of the transit instrument was regularly determined by a level; for the correct computation of the refraction, thermometer and barometer were regularly read. By such precautions he enabled later investigators to derive from the observations themselves the errors vitiating the results and to free the results from their influence. This was important because, whereas the positions of the moon were needed immediately, a careful reduction of the stars had to be postponed from lack of time.

After Bradley's death a conflict arose between his heirs and the British Admiralty as to the ownership of the 13 volumes containing the journals of observation and other manuscripts. After many years of proceedings before the courts, an agreement was reached that put all the papers into the hands of Oxford University for publication; so they were printed in full between 1798 and 1805. Then Bessel took the reduction in hand; when he published the results in 1818, he expressed in the title—*Fundamenta Astronomiae, ex observationibus viri incomparabilis James Bradley*—how Bradley's work, by its high quality, had become the basis of early nineteenth-century astronomy. In the catalogue of about 3,000 stars, in which the results are condensed, the uncertainty in both co-ordinates—right ascension and declination—is not more than some few seconds of arc.

Bradley established for Greenwich Observatory a standard of carefulness in working methods which was maintained by his successors Maskelyne and Pond. Though not equalled in his own time, his standards were nearly reached by such able and persistent observers as Tobias Mayer of Göttingen and Lacaille of Paris. In their catalogues of some hundreds of bright stars, the first improvements upon Flamsteed's, the star positions are found to have mean errors amounting at most to 4″ or 5″. Lacaille went with his instruments to the Cape of Good Hope in 1750, where he observed, besides the sun and the moon for parallax, nearly ten thousand southern stars. They were, of course, mostly telescopic stars, of the seventh and eighth magnitudes, so that now the stars of the southern sky were more completely observed and catalogued than those of the easily accessible Northern Hemisphere.

An important step forward towards even greater accuracy had been prepared at the same time by the improvement of the optical devices. To understand the development of the telescopes we have first to go back a century. In his optical studies between 1660 and 1670 Newton had discovered the different refrangibility of light of different colours.

Refraction of light through a prism is always accompanied by dispersion of the different colours in a spectrum. He perceived that this dispersion was the chief cause of the faulty and coloured images in the existing telescopes; since the different colours could not be converged into one focus, it was hopeless to try to obtain good images by improving the lenses. 'I saw that the perfection of telescopes was hitherto limited, not so much for want of glasses truly figured . . . as because that light itself is a heterogeneous mixture of differently refrangible rays' (letter from Newton to the Royal Society, February 6, 1672).[162] Thus Newton got the idea of using a concave mirror instead of a lens, because, with such a mirror, rays of all colours are strictly united in the same focus. Gregory had already devised a reflecting telescope, by which the rays reflected by the concave mirror, after a second reflection on a smaller concave mirror, reached the observer's eye through a circular hole in the centre of the larger mirror; but the optical artisans were not able to grind good mirrors for him. Newton himself set to work and, after having ground with great patience and perseverance a concave metal speculum, in 1671 he fitted it into a reflecting telescope that roused great interest when presented to the Royal Society. The image in the focus was reflected sideways by a flat mirror inclined 45° and viewed in a horizontal direction through the ocular at the side of the tube. It appeared that this small instrument, only 6 inches long, with 'something more than an inch'[163] aperture, allowed of a magnification of 40 times and showed the same details in the celestial objects as a refracting telescope of 3 or 4 feet in length.

The invention remained without practical use until about 1720, when James Short of Edinburgh succeeded in grinding concave mirrors in such a regular way that he could sell reflecting telescopes as an ordinary workshop product. Since, by the use of Gregory's construction, the observer looked straight in the direction of the object, they were easily handled. Since, moreover, because of their large opening, they gave bright images, they became the favourite instruments of amateurs and astronomers, far preferable to the larger refractors, with their pale and blurred images. It was not easy, however, to attach them to measuring instruments; so there the long narrow tubes with lenses remained in use; Bradley's 8 foot transit had an object glass of 1.6 inches only.

Newton thought that the removal of the colour dispersion was impossible; he assumed that the dispersion, though different for media of different density, was always proportional to the refraction. When, however, in the next century Euler had expressed doubts as to this proportionality, and the Swedish physicist Klingenstjerna by experiments had shown that it was incorrect, John Dollond in London, who was in correspondence with both, after much searching and experi-

menting, succeeded in 1757 in making a combination of glasses in which the colour dispersion was suppressed. Between two positive lenses of ordinary crown glass he placed a negative (concave) lens made of flint glass that possessed a stronger refraction and a far stronger dispersion. Thus an 'achromatic' system of lenses was realized that brought rays of different colours into one focus.

This invention was of enormous importance; it opened the way to an unlimited development of astronomical telescopes in the next century. Now that the colour dispersion was removed, the earlier work of Huygens could be continued in studying the most efficient figures of the lenses. John Dollond was a scientist and philosopher; his son Peter was a businessman. He founded a workshop, applied for a patent,* and soon his achromatic telescopes spread among astronomers and superseded the old single-lens and also the reflecting telescopes. Their size was limited to an aperture of 3 or 4 inches, because larger flint discs could not be supplied by the glassworks. But this size, with a length of about 4 feet, was handy and usable; it gave sharp and bright images of the stars over a larger field than did the reflecting telescopes. On the Venus expeditions both kinds of telescopes—achromatic and reflecting—were used side by side.

Dollond's invention brought highly important improvements to the astronomical measuring instruments. The narrow tubes with small objective lenses that were connected with these instruments produced images of the stars too poor to be exactly pointed. With the achromatic objectives these images became fine bright points of light, allowing greater enlargement by stronger oculars, so that they could be bisected by the cross-wires with greater precision. Now the telescopes of a given length could acquire larger aperture, making faint telescopic stars easily visible and measurable. Until now, star catalogues, besides stars visible to the naked eye, contained the brightest classes only among the telescopic stars. Now it became possible to use the instruments for measuring the multitude of faint stars also.

The best instruments came from the workshop founded by Jesse Ramsden, the son-in-law of John Dollond, all of them, of course, provided with achromatic telescopes. Maskelyne in 1772 had provided Bradley's instruments at Greenwich with achromatic telescopes; since the transits of the sharp star images were observed by him at five wires and estimated in tenths of seconds (Bradley had noted them at full,

* It appeared that many years earlier, in 1733, an achromatic telescope had been constructed by Chester More Hall in Essex, but he had not made it known; the licence for making and selling them was accorded to Dollond, because, as the judge observed, 'it was not the person who locked his invention in his scrutoire that ought to profit from such invention, but he who brought it forth for the benefit of the public'. (R. Grant, *History of Physical Astronomy* [London, 1852], p. 533.)

half, or third seconds only) his right ascensions, though smaller in number, advanced considerably in accuracy. Many observatories outside England were equipped by Ramsden with English instruments. When G. Piazzi was commissioned to found an observatory at Palermo, he had a 5 foot vertical circle constructed by Ramsden, which could rotate on a 3 foot horizontal circle; both were provided with microscopes for reading. With this instrument, from 1792 to 1802, he determined with great care the positions of 6,748 stars, a number which was later increased to 7,646. Thus a higher norm of accuracy and completeness in stellar co-ordinates was established, with no greater uncertainty than a few seconds. That Bradley, by his treatment of instruments and conditions, had achieved even more did not become manifest until later through Bessel's reductions.

CHAPTER 30

REFINED THEORY

With Newton's theory of universal gravitation, Descartes's vortex theory was essentially demolished. It is true that even in England the physics textbooks based entirely on Descartes remained in use; but the scholars at the universities, able mathematicians, studied Newton and taught the deductions contained in his *Principia*. To every new edition of the old textbooks further notes were added explaining Newton; so already in his lifetime his ideas gradually found general acceptance at the English universities.

This took place far more slowly on the Continent of Europe. In France, exhaustion resulting from war and the intellectual decline that set in under royal absolutism towards the end of the seventeenth century hampered scientific initiative. To begin with De Louville, an independent thinker, was the only one to come forward (about 1722) as an adherent of the new theory; for the other scholars the old doctrine was too powerful. But a change was on the way. The decline and defeat of French power by the rising English awakened a mood critical of the existing political and economic system. The increase in the social significance and power of the French middle class in the eighteenth century roused a spirit of resistance which regarded the free social conditions and political institutions of England as an example to be followed. So they were ready to accept also the spiritual basis of these conditions. One of the first spokesmen of these ideas of reform was Voltaire; in his 'Letters from London on the English' (1728–30), amongst such subjects as the Quakers, the English Church, Parliament, commerce, vaccination and literature, he also discussed English philosophy and science, especially Bacon and Newton. In his fourteenth letter on Descartes and Newton he wrote: 'A Frenchman coming to London finds matters considerably changed, in philosophy as in everything else. He left the world filled, he finds it here empty. In Paris you see the universe consisting of vortices of a subtle matter; in London nothing is seen of this. With us it is the pressure of the moon that causes the tides of the sea; with the English it is the sea that gravitates toward the moon. . . Moreover, you may perceive that the sun, which in

France is not at all involved in the affair, here has to contribute by nearly one quarter. With your Cartesians everything takes place through pressure, which is not easily comprehensible; with Monsieur Newton it takes place through attraction, the cause of which is not better known either. In Paris you figure the earth as a melon; in London it is flattened on both sides.'[164]

Notwithstanding his lighthearted style, Voltaire made a good comparison of Newton's theory with that of Descartes. More amply still, in 1733 he informed his compatriots in a special work entitled *Elements of the Philosophy of Newton*, on the theory of light and on gravitation. By now the French world had become more susceptible to the new theory. Whereas formerly both doctrines found expression alternately in the transactions of the Paris Academy, after 1740 papers based on the vortex theory disappeared completely. You could do nothing with it, you could derive nothing from it, whereas from Newton's theory precise results could be derived merely by using mathematics. It posed a clear and great task: proceeding from the fundamental law of attraction of all particles of matter, hence of all celestial bodies upon one another, to compute their movements and to test them by observation. The theoretical development of astronomy in the eighteenth century was entirely dominated by gravitation.

Now the scientists came forward who were to continue Newton's work. Not in England, where, once liberty and self-rule had been won, self-satisfied prosperity quenched higher aspirations; all effort was directed toward practical affairs. But on the Continent, especially in France, spiritual life was roused by a strong desire for social renovation. England remained at the forefront in practical astronomy, as in all practice. But on the Continent the traditional rationalist mode of thinking, under these new impulses, developed into an outburst of theory and a profusion of mathematical treatments of the natural phenomena. A series of brilliant mathematicians sprang up; among them, as the most outstanding figures, the Bernoullis and Leonhard Euler from Basel, Clairaut and d'Alembert in France; their work was continued and completed by Lagrange and Laplace.

First the mathematical methods had to be remodelled. Newton had given all his demonstrations in a geometrical way, illustrative but requiring great ingenuity in the handling. The eighteenth-century mathematicians developed the algebraic method of analysis, in which the difficult geometrical insight was replaced by simple calculation, so that more difficult problems, otherwise intractable, could be solved. Newton himself had laid the basis by his theory of fluxions, a method of investigating changes of quantities by considering them in the limiting case of infinitely small variations. The same fundamental idea had been

developed at the same time by Leibniz, but formulated in a different way, as infinitesimal calculus, by means of differential and integral formulae; in this form 'calculus' became the mathematicians' chief and most powerful tool in the centuries that ensued.

It was not only astronomical problems that occupied them. Newton had formulated the new principles of mechanics; his theory of motion established the general relations between forces, accelerations, distances and masses. The task of his successors was to apply these to all the different phenomena of moving bodies in nature. They developed new forms of the mechanical principles which were important for all kinds of motion, on earth and elsewhere. But astronomy received a large share of their exertions, first because of the difficulty of the problems posed, which was a stimulus to ingenuity, and secondly because the results of a fascinating theory in solving time-honoured problems could be verified by accurate observations. When bodies in space attract one another, the forces upon each of them—hence their accelerations—can be computed from their relative positions; by adding the consecutive accelerations, i.e. by integration, the velocities are obtained and from these, by a second integration, the changes in position. But the positions themselves resulting from this procedure are elements needed in the computation of the original accelerations. Thus the derivation of the course of these bodies was an intricate problem of analysis, designated as the solution of a system of differential equations.

For two bodies the solution was simple and was given by Newton. For three or more bodies no solution could be found. To the first eighteenth-century mathematicians, faced by the 'problem of three bodies', it proved to be unsolvable in a direct way; and thus it remained. Their disappointment can be heard in the complaint of one of the ablest among them, Alexis Claude Clairaut (1713–65), when he hit upon the problem: 'may now integrate who can . . . I have deduced the equations given here at the first moment, but I only applied few efforts to their solution, since they appeared to me little tractable. Perhaps they are more promising to others. I have given them up and have taken to using the method of approximations.'[165] And in the same way Leonhard Euler (1707–83), a mathematical genius, wrote in the Preface of his last great work on lunar theory, in 1772: 'As often as I have tried these forty years to derive the theory and motion of the moon from the principles of gravitation, there always arose so many difficulties that I am compelled to break off my work and latest researches. The problem reduces to three differential equations of the second degree, which not only cannot be integrated in any way but which also put the greatest difficulties in the way of the approximations with which we must here content ourselves; so that I do not see how, by means of theory alone, this research can be completed, nay, not even solely adapted to any useful purpose.'[166]

What these pioneers in the realm of celestial mechanics accepted as an unsatisfactory makeshift turned out to be the only way, however cumbersome and laborious, yet the most general method for solving such problems. First, the forces and accelerations are computed as they would be in the known unperturbed orbit; by integration, the deviations in position in first instance are derived. By these deviations in the position of the attracted planet, the forces and accelerations are changed by a small amount (small relative to their first values), and this gives rise to additional, still smaller, so-called 'second-order' deviations. Continuing in this way by further approximations, a final result is approached ever more closely. Since, in the first instance, the disturbing forces changed rather irregularly with the change in the relative positions of the bodies, they are split up into a number of periodical terms, depending in various ways on longitude, anomaly, nodes, and latitudes, of the disturbed as well as the disturbing body. Since in the higher orders all these terms react upon one another, their complete computation constituted from the beginning an entangled, nearly inextricable task, demanding years of work—in later times with higher standards, even an entire life of strenuous and careful work. Such work, then, was not a quiet, unimpassioned computing with ready-made formulae, as it may appear now in the textbooks. It was a forward-pressing search into the unknown country of theory, blazing a trail through the thicket now here, now there, full of adventure. Mostly the workers were driven along by actual problems, always obsessed by the question: Will it be possible to compute all the factual motions by means of Newton's theory? Is this law of attraction the precise universal law capable of explaining by itself all the phenomena observed? This question gave tension to the work of the eighteenth-century mathematicians.

The first practical problem was posed by the motion of Jupiter and Saturn. Kepler had already perceived, in 1625, that there was something wrong. Halley in his tables of 1695 had introduced a regular acceleration of Jupiter, a regular retardation of Saturn, of such an amount that after 1,000 years the planets would be displaced 0° 57′ and 2° 19′. If this variation should always continue thus, with Jupiter's orbit steadily diminishing and Saturn's increasing, serious consequences for the planetary system could result. What was the cause? Could it be a result of mutual attractions, as Halley suspected, and would it be possible to compute the phenomenon by means of Newton's law? The Paris Academy of Sciences put this up as the prize problem for 1748 and again for 1752. For Euler's answer on the first occasion, though it was awarded the prize for the many important results on perturbations

in general, did not solve the problem. Neither did his second memoir, in which he showed the possibility of 'secular' perturbations, always continuing in the same direction. Lagrange in 1763 published a quite new method of treating the problem of three bodies and applied it to the mutual action of Jupiter and Saturn; indeed, he found a secular term for both, but it was too small to be identified with the observed one. It was then that Pierre Simon de Laplace (1749–1827) entered the field with a careful investigation of all the smaller neglected terms of higher order in the mean motion of the two planets. He found that they cancelled out; the computed continuous acceleration and retardation turned out to be zero. Lagrange then extended this result by proving in 1776, in a general way, that the mutual attractions of the planets could not produce any secular progressive changes in the mean distances to the sun and the periods of revolution; they were subject to periodic variations only.

But what about the observed changes of Jupiter and Saturn? Some years later—in token of his embarrassment—Laplace wrote: 'After having recognized the constancy of the mean movements of the planets, I suspected that the changes observed in those of Jupiter and Saturn were due to the action of comets.'[167] Lambert opened a new track when he remarked in 1773 that the changes were different from what was assumed. A comparison of Hevelius's observations with the modern eighteenth-century results showed a retardation of Jupiter's course and an acceleration of Saturn's, just the reverse of what had been deduced from earlier observations. Hence the phenomenon was a periodical change. But a careful computation by Lagrange, including all terms containing the second power of the small eccentricities, did not reveal any term of the required amount. At last, in a memoir presented to the Academy in 1784, Laplace succeeded in solving the riddle, by the discovery that near commensurabilities of the motions produce large perturbations of long period. Five revolutions of Jupiter and two of Saturn are nearly equal, so that after 59 years (our well-known three conjunctions of ancient astrology) the two planets meet again at nearly the same place in the ecliptic. A couple of small terms of the third order, neglected because they contained the third power of the eccentricities, return after every 59 year period in the same way, and thus accumulate their effect into very perceptible changes of the longitude of the planets. This goes on until gradually the position of the conjunction shifts to other and opposite longitudes, and the effect is reversed after 450 years.

So, in reality, the perturbation is a long-period oscillation, with a period of 900 years, increasing to 49′ for Saturn and to 21′ for Jupiter. All the ancient and modern observations were now well represented by theory. The importance of this result was expressed by Laplace some years afterwards as follows: 'The irregularities of the two planets appeared formerly to be inexplicable by the law of universal gravitation—they now form one of its most striking proofs. Such has been the fate of this [Newton's] brilliant discovery, that each difficulty which has arisen has become for it a new subject of triumph, a circumstance which is the surest characteristic of the true system of nature.'[168] And in the next century Robert Grant wrote in his *History of Physical Astronomy*: 'By this capital discovery Laplace banished empiricism from the tables of Jupiter and Saturn, and extricated the Newtonian theory from one of its gravest perils.'[169]

Another famous object of computation was the comet whose return in 1758 had been predicted by Halley. It was now understood, as a result of Newton's theory, that the planets would disturb its motion by their attraction, so that it might return earlier or later than predicted. Between AD 1531 and 1607 the interval had been 76 years, between 1607 and 1682 somewhat less than 75 years; should the next period of revolution be 75 years, the comet could turn up in 1757. When the time came near, Clairaut set to work. It was not possible here to apply the methods used with the planets to find the perturbations for the entire orbit as a sum total of terms. It was necessary to follow the comet, computing its progress step by step as it was affected by the attraction of the planets over its entire course, the two preceding periods as well as the present one. Full of anxiety lest the comet could surprise him by coming early, he set to computing, aided by Mme Lepaute, a gifted mathematician, wife of the famous French clockmaker. They computed the perturbations by Jupiter and Saturn, struggling on day by day, hardly allowing themselves time for meals. 'The work I had entered upon was immense, and I was not able to state anything definite about the proposed object before the autumn of 1758.'[170] The comet, happily, did not surprise them, and he could reveal the reason, viz. that because of the attraction of Jupiter and Saturn, the comet would spend 618 days more on the last revolution than on the preceding one, so that its nearest approach to the sun was not to be expected before April 1759—with a margin of a month for different approximations. In a memoir to the Academy, read in November 1758, he said: 'I undertake here to show that this retardation, far from being injurious to the theory of universal attraction, is actually its necessary consequence, and that we can even go farther because I indicate at the same time its limits.'[171] The comet was first discovered at the end of 1758 by an amateur, Palitsch, living near Dresden in Saxony. It reached its perihelion in March 1759, and was visible until June. This prediction and computation of the return of Halley's comet was rightly considered a triumph of Newton's science.

The appearance of the comet was an episode. The motion of the moon, on the contrary, was the great and difficult problem which, as a touchstone of ingenuity, gave an impulse to numerous researches and new methods. The main irregularities in the moon's motion are, as Newton had described, perturbations due to the attraction of the sun. The perturbing force exerted by the sun is a rather large fraction ($\frac{1}{89}$ at new and full moon) of the attraction exerted by the earth. Since because the moon is so near small displacements are easily observed, the approximation in the computations must be extended right down to very small terms, which, by their mutual influence and dependence, engender a still more confusing multitude. The difficulty of the problem acted as a painful lesson to Clairaut at the very start of his researches in 1746, when he found for the progress of the moon's apogee only half the real amount, 20° per year instead of the observed 40°. Euler and d'Alembert found the same result. A universal formula in Newton's *Principia* had also afforded the same small amount; later on, among his unpublished manuscripts a computation was found giving the right value. Clairaut first supposed that Newton's law was not exactly true and had, for very short distances, to be completed by another small term with the inverse fourth power of the distance. But, in repeating his computation, the inclusion of a number of first neglected terms of higher order contributed so much that the first result was doubled; this also was confirmed by his colleagues. However, the true value could not be ascertained in this way with sufficient accuracy; though the motion of the lunar apogee was doubtless one of the most difficult lunar problems, in a certain way this held for all lunar perturbations. Clairaut and d'Alembert in 1754 published their lunar tables based entirely on theory and these, although they surpassed the tables of former times, did not exactly fit the moon's course. In 1745 and 1746 Euler had computed his first tables of perturbations; he gave an improved theory in 1755; and in 1772, for the third time, returned to the subject, computing further details. But agreement with observation remained unsatisfactory.

The reason was that theory could not determine the exact amount of the perturbations, though it was successful in indicating what perturbations must occur, with what periods, and how they depended on the sun, the nodes and the aphelia. For each period a large number of higher-order terms, forming an endless series, contributed to the result, so that their sum total remained uncertain. Yet the practical needs of navigation demanded tables of the moon exactly computed beforehand, and the theory of gravitation must be able to provide them.

Then it was that Tobias Mayer (1723–62) made a lucky hit by combining theory and practice. He had already acquired fame by his measurements, with primitive instruments, of the position of numerous lunar mountains, from which he derived the different librations of the moon. Then he was called to Göttingen in the kingdom of Hanover, where under the kings of England there was less narrow-mindedness than in the other petty states of Germany. He installed there an observatory and determined the positions, alluded to above, of the moon and the stars.

In 1755 he published solar and lunar tables. For the latter he took the most important perturbations from Euler's theory; but the amount of each he derived from practical data so as to render the observed positions of the moon as well as possible. By including no more than 14 terms, he secured the result that the errors in a few cases amounted to only $1\frac{1}{2}'$. When the moon at sea is used as a celestial clock indicating Greenwich time, an error of $1'$ in the moon's position means an error of $27'$ in the geographical longitude, i.e. an uncertainty of, at most, 27 sea miles in the ship's position. After his death, Mayer's new tables were examined by Bradley, by order of the British Admiralty, and compared with the Greenwich observations. When they had been corrected in some points, it appeared that their errors always remained below, mostly far below, $1'$. As an important aid in navigation, with instructions and methods prepared by Mayer himself, the tables were published by the Admiralty in 1770, and a grant of £3,000 was awarded to his widow by the British Government.

The problem for navigation practice was solved, but theory was still faced with a difficult and mysterious problem. When comparing the eclipses of antiquity and those of the Arabs with modern ones, Halley had in 1693 perceived that the moon's period of revolution, hence also its distance from the earth, had gradually diminished. This 'secular acceleration' of the moon was confirmed by Tobias Mayer; he first found for the amount per century $6.7''$; afterwards, in the London tables he used $9''$; still later Laplace derived $10''$. This means that the moon had advanced $10''$ after 100 years, $40''$ after 200, $90''$ after 300, relative to its position without this term, and that the path performed in a century ($100 \times 13\frac{1}{3} \times 360°$) increased by $20''$ per century. If this diminution in the moon's orbit continued into the future, the moon would finally descend upon the earth. In 1770 the Paris Academy offered their prize for research as to whether the theory of gravitation could explain the phenomenon; Euler, in his prize treatise, could find no such explanation and wrote: 'It appears to be established, by indisputable evidence, that the secular inequality of the moon's motion cannot be produced by the forces of gravitation.'[172] In a second treatise in 1772 he supplemented this conclusion by supposing that the term probably arose from the resistance of an ethereal fluid which filled celestial space.

Such resistance could indeed explain the acceleration, but in such a way that it made a final catastrophe inevitable. After many fruitless attempts by Lagrange and Laplace, the latter at last, in 1787, succeeded in discovering the real cause. By the action of the planets upon the earth, the eccentricity of the earth's orbit was continually diminishing during some ten thousand years; because the orbit became more circular, the mean distance of the sun increased, and its perturbing effect decreased. By the attraction of the sun, the moon's orbit was enlarged; this enlargement now gradually diminished through the decrease of the sun's effect. Laplace found by a theoretical computation the same amount of the acceleration, $10''$ per century, as was deduced from the eclipses. Thus again the uneasiness was removed, and the conviction that Newton's theory was capable of explaining all the movements in the solar system grew even stronger.

Laplace made a complete theoretical computation of the movement of the moon and the planets by means of mathematical developments. He collected all these researches in his great work, *Traité de mécanique céleste* ('Treatise on Celestial Mechanics'), which appeared in five volumes in 1799–1825. Here he treated all the motions in the solar system as a purely mathematical problem. He posed the problem in its most general form: each body in the world consists of small elements of mass attracting one another in accordance with Newton's law; the sum total of all these forces is the force exerted by the complete bodies, which in the case of a spheroidal figure deviates a small amount from a force proceeding from the centre. This general formulation made Laplace the spokesman, often attacked and criticized in the next century, of the mechanistic doctrine of the world. There is nothing in it of the atoms and molecules of later physics; Laplace introduced the small elements of mass only as a mathematical abstraction. Once given the initial condition of the system, i.e. the position and motion of every particle, the further course of the world was entirely determined and calculable, at least in theory. The solar system was taken here as an immense mechanism, steered and driven by the universal force of gravitation, running its calculable and predictable way into eternity. Often in these times it was spoken of as a gigantic timepiece that, once set in motion, goes on for ever—clocks then being the most perfect instruments made by human skill—in accordance with the general trend of thought that tried to understand the world as a most ingenious and perfect machine.

For the planets Laplace was able to derive the slow variations in the form and the position of the orbits, i.e. the secular perturbations of eccentricity, inclination, aphelion and node, for hundreds of thousands of years into the past and future. For the moon, he succeeded in representing its movement down to less than $\frac{1}{2}'$ by purely theoretical computation. Terms occurred in his theory of the moon—in variation of apogee and node—which were due to and depended on the flattening of the earth. Thus, conversely, he was able to derive this flattening from the values of those terms determined through observation; his result, $\frac{1}{305}$, was a welcome confirmation of the value derived from the expedition to Peru. Another term in the moon's motion, exceeding 2′, depends on the ratio of the distances of sun and moon to the earth and is called 'parallactic inequality', because it is thus connected with the solar parallax; from its empirical value Laplace derived a solar parallax of 8.6″, in close agreement with the results of the Venus expeditions. 'It is very remarkable,' he said, 'that an astronomer, without leaving his observatory, by merely comparing his observations with analysis, may be enabled to determine with accuracy the magnitude and flattening of the earth, and its distance from the sun and moon, elements the knowledge of which has been the fruit of long and troublesome voyages in both hemispheres. The agreement between the results of the two methods is one of the most striking proofs of universal gravitation.'[173]

This utterance is found in his *Exposition du système du monde* ('Exposition of the World System'), where in a popular, non-mathematical way the results of all the theoretical researches were explained. The outcome most important for the general reader was the assurance of the stability of the system. The secular perturbations of the major axes of the planetary orbits (as to the first power of the masses) are nil. Because, moreover, those of the inclinations and eccentricities remain within well-defined limits—a consequence of all planets revolving in the same direction—the inner structure will remain unchanged for ever.

However, it had not always existed in this form. The young German philosopher Immanuel Kant had already in 1755 developed the theory that all matter of the solar system had existed originally in the form of a widely extended nebulous mass. By its own inner forces, it had developed into a flat, rotating disc, which, by contracting, had produced the planets with their orbits. Newton had considered that for the existing universe to have originated in a purely mechanical way, without the intervention of a supreme intelligence, was incompatible with his world system. The progress of the eighteenth-century rationalism is now shown in that it ventured to tackle the problems of natural science. As a rational explanation of the fact that the planetary orbits are situated nearly in one plane and are run in the same direction, Kant's nebular theory was the first cosmogony with a scientific basis. Knowing nothing of Kant's work, Laplace propounded the same theory in his *Exposition*: that the solar system had originated by evolution from an entirely

different original state. Now a final state of things had been reached, which could be disturbed no more. Cosmic world theory corresponded to the human world which also, after a long development from original barbarism and ignorance, had now reached—or as people then supposed: nearly reached—its final state under the rule of freedom, reason and science.

Thus at the close of the eighteenth century astronomy could look back with pride on its achievements. With an accuracy formerly unknown, it could observe the motions of the celestial bodies, and through the fundamental law of the universe which had now been discovered it could compute and predict them. It is true that this universe was the solar system only. But its limits began to be overstepped.