BOOK III

ASTRONOMY SURVEYING THE UNIVERSE

CHAPTER 31

THE WORLD WIDENS

Throughout these centuries the fixed stars had roused interest only as a background for the motion of the planets. They were the fixed points for the determination of the changing positions of the moon and the planets. Some few details about themselves had been perceived now and then; thus a small change in position had been ascertained for some of them, so that they must have a proper motion. With some stars a periodical change in brightness had been detected, without giving great surprise, since the new stars of Tycho and Kepler had offered phenomena far more sensational. In 1596 David Fabricius perceived in the Whale a star of the third magnitude, which thereafter faded and disappeared; so he took it for another nova. But in 1638 Holwarda, of the Frisian University at Franeker, saw it again at the same place; he saw it disappear and then reappear and found that it alternately increased and decreased to invisibility in a period of eleven months. Tycho had observed the star, and Bayer had given it the Greek letter omicron; now it was named Mira Ceti, the 'miraculous one in the Whale'. Its fluctuations showed considerable irregularities; sometimes it attained the fourth magnitude only, sometimes the second, and once (in 1779) it shone as a first-magnitude star approaching Aldebaran. According to the Assyriologist Schaumberger, it probably had already been noticed in Babylon; some cuneiform inscriptions speak of the constellation Dilgan (i.e. the Whale and the Ram) in terms which meant to 'flare up' and 'extinguish'.[174] In the seventeenth and eighteenth centuries further discoveries followed; in 1672 Montanari at Bologna, who also in 1667 had perceived variations in Algol, discovered that low in the southern sky, in the constellation Hydra, a star fluctuated between the fourth magnitude and invisibility. In 1685 Kirch, in Berlin, found an analogous case in a fifth-magnitude star in the neck of the Swan, χ Cygni. Such discoveries show that there were observers who watched the stars attentively. But the interest was not yet sufficient to stimulate regular systematic observation.

Other objects belonging to this world of stars were the nebulous objects, called 'nebulae', of which the two most conspicuous had been perceived in the seventeenth century. The elongated nebula of Andromeda, easily visible to the naked eye, was first mentioned in 1612 by Simon Marius, and the nebula of Orion surrounding the fourth-magnitude star θ in the Sword, discovered in 1619 by Cysat at Ingolstadt, was in 1694 depicted by Huygens in his diary. The telescopes revealed many more and fainter ones. Because when they were first detected they were often thought to be comets and were announced as such, the French astronomer Messier, famous comet discoverer, compiled a list of over a hundred of these nebulae, which was published in 1771, to preclude false announcements of phantom comets. This was all that was known about the world of the stars. Their distance, too, was still unknown, since all attempts to measure their parallax had failed.

William Herschel (1738–1822), descendant of a German family of musicians (hence really christened Wilhelm Friedrich) from Hanover, had gone to England and at Bath had become a distinguished conductor and teacher as well as a composer. He had brought from Germany a keen interest in scientific and philosophical problems, which about 1773 turned into an increasing passion for astronomy. In that year he bought lenses for a telescope and rented a 2-foot reflecting telescope; but they did not satisfy him. He then began to make a telescope himself by casting and grinding a concave mirror. In his diary we find under September 1774: 'Attended 6, 7 or 8 scholars [i.e. music pupils] every day. At night I made astronomical observations with telescopes of my own construction.' Also on May 1, 1776: 'I observed Saturn with a new 7-foot reflector'; and on July 13, 1776: 'I viewed Saturn with a new 20-foot reflector I had erected in my garden.' By experimenting on the best metal mixture (copper with one-third tin) and grinding and polishing the surface into the right shape with the utmost care, he obtained mirrors of the most excellent quality, which produced perfectly round stellar images. On a direct comparison with the telescopes at Greenwich Observatory, they proved to be far superior. Next to this refined quality came, as a second factor, the increase in size, hence in brightness and resolving power; by using strong oculars, he could increase the enlargement from 200 and 460 times to the then unheard-of values of 2,000, 3,168 and 6,450 times. Assisted by his brother and by his sister Caroline, his devoted assistant first in his musical occupations and then in his astronomical work—in which she afterward achieved fame as a discoverer of comets—he made several 7 foot, 10 foot and 20 foot telescopes (the last-named with mirrors of 12 and 19 inches diameter) and employed them in intensive observation. His work represented considerable progress in astronomical technique, the result of skill and untiring devotion in striving to make the working apparatus as perfect as possible. Thus he had already become known in the astronomical world through his observations of the rotation of Mars and Jupiter and his micrometric measurements of the height of the lunar mountains, in the years 1777–81.

He set himself far-reaching goals—in the first place, to find the parallaxes of some stars. In a paper presented to the Royal Society, he expounded his ideas; since direct measurements of stellar positions had too large errors, he proposed to determine repeatedly the position of a bright star relative to a faint star closely adjacent to it. If the distance was some few seconds, the relative place and displacement could be estimated and expressed in diameters of the stellar discs—here is seen the importance of the regular, circular shape of these discs. Hence he had started to examine all the brighter stars attentively with his 7-foot telescope, to see whether they had faint companions nearby. Then, in 1781: 'On Tuesday the 13th of March between 10 and 11 in the evening, when I was examining the small stars in the neighbourhood of H [i.e. η] Geminorum, I perceived one that appeared visibly larger than the rest; being struck with its uncommon magnitude, I compared it to H Geminorum and the small star in the quartile between Auriga and Gemini, and, finding it so much larger than either of them, suspected it to be a comet.'[175]

With higher powers of magnification he saw the disc, increased in proportion to the power, measuring 3″ to 5″. This would not be the case with star images. During the following days it showed a slow direct motion along the ecliptic of nearly 1′ per day; it had no tail and showed a well-defined disc, which in the weeks which ensued increased in measurement, so that Herschel supposed that the object was drawing nearer to the earth. The discovery of this new and singular comet was immediately communicated to the Astronomer Royal, Maskelyne, and others, and it was also soon observed in France. When after several months an orbit was computed, it was found to be a circle 19 times larger than the earth's orbit. Hence it was a planet far outside Saturn, increasing by one the ancient time-honoured number of planets, enlarging the planetary system to double the former size. The fame of this discovery induced the King to award Herschel a salary of £200 to enable him to give up his remunerative musical trade and devote himself entirely to his passion for astronomy—though he had to add to it by grinding and selling mirrors for telescopes. It may be remarked here that among a list of 70 telescopes mentioned in 1795 as being sold to others, only one is known to have been used in valuable astronomical work: the 7-foot telescope bought by Amtmann Schroeter at Lilienthal. His gratitude to the King was shown by his giving the new planet the name of 'Georgium Sidus' (the star of George); in other countries, however, the name of Uranus came into use, and it has superseded the royal designation.

At Slough, near Windsor, where he next settled, Herschel started to make, with financial aid from the King, a still larger instrument, a 40-foot telescope with a mirror 58 inches in diameter. In 1789 it was finished, and Herschel described enthusiastically how for many hours he had observed Saturn better than before. In 1795 he gave a detailed description of the gigantic structure of heavy poles, erected on a foundation of masonry and wooden beams, in which the big tube hung and could be moved by a system of strong ropes and pulleys. It was admired and glorified as a wonder of science, and its picture was reproduced in books and magazines, and even on medals. However, it was seldom used by its author for observations. It seems that the handling of the clumsy colossus was rather laborious and that the images were not satisfactory, perhaps because the mirror was distorted by its own weight. All the important work of discovery in later years was achieved with the 20-foot telescope of 19-inch aperture (plate 9). It comprised much valuable work on bodies of the solar system, such as observations of sunspots and of the ring and belts of Saturn, as well as the discovery of the white polar caps of Mars, of two satellites of Uranus, and of two new satellites of Saturn. The main object of all Herschel's researches, however, was the world of the fixed stars.

First he completed his work on double stars and published his results in two catalogues, one of 269 objects in 1782 and another of 434 objects in 1784. For each of them the position of the faint star was given relative to the bright one; the angles of position and the larger distances were measured with the aid of a filar micrometer, and the small distances of a few seconds were estimated by comparison with the size of the stellar discs. Of course, they were not very exact, because of the coarseness of the filar micrometer. In fact, he said: 'The single threads of the silkworm, with such lenses as I use, are so much magnified that their diameter is more than that of many of the stars'[176]; and he emphasized 'how difficult it is to have screws that shall be perfectly equal in every thread or revolution of each thread'.[177] Therefore, he constructed his 'lamp-micrometer'. Two artificial stars produced by lamps shining through pinholes, at a distance of about ten feet, were viewed with the left eye, while the right eye looked at the stars in the telescope; they could be brought into every relative position so as to appear exactly like the double star.

In the planning of his research on small companions to bright stars, Herschel proceeded from the idea that small stars looked small because they were distant. He assumed that the 'magnitude' of a star was thus a direct indication of its distance, that a fourth-magnitude star stands at a

four-times greater distance than stars of the first magnitude. The faint companion could thus serve to find the parallax and distance of the bright star. However, the large number of cases in which small stars were seen in the closest vicinity to brighter stars far surpassed what was to be expected of faint stars through chance distribution. The idea that most of them must be real companions in space, with a small intrinsic brightness, must gradually have taken hold of him. The same thing had been observed by Chr. Mayer at Mannheim, who in 1777 published that he had seen planets belonging to bright stars; real planets illuminated by their sun, however, would be too faint to be visible. Many of Herschel's double stars, moreover, were so nearly equal that they could be due to chance distribution still less, and they certainly could not serve as objects for parallaxes. Surely he must soon have thought of them as real binary systems, for at the close of his first paper he added: 'it is much too soon to form any theories of small stars revolving round large ones.'[178]

Twenty years later he returned to the subject by measuring anew the relative positions of the components of a number of his double stars. In two papers, in 1803 and 1804, he described how, for about fifty of them, the position angle had changed by amounts between 5° and 51°. In a careful discussion he made sure that this change could not be caused by a motion of the sun or the proper motion of the chief star; the only admissible explanation was an orbital motion of the small star about the large one, or of both about their common centre of gravity. His discussion demonstrated—if such proof were needed—that Newton's law of mutual attraction also ruled the stars in the distant realms of space. The existence of another type of world system besides our single sun with its planets was here demonstrated: systems of two stars (some even of three or four stars) revolving about their common centre.

It was in the early eighties, too, that his attention was drawn to the problem of the sun's motion. In a paper in 1783 he said that, since we know that some stars are moving and that all stars certainly attract one another, we have to conclude that all stars are moving through space with the sun among them. A solar motion must reveal itself in an apparent opposite motion of the stars, which he called, because it depends on their distance, their 'systematical parallax'. He used the name 'apex' for the point of the sky whither the sun's motion is directed, and he pointed out that the stars situated sideways must show a maximum effect. From seven bright stars for which Maskelyne had given the yearly motion in right ascension and declination, then increased to 12 taken from Lalande, and afterwards augmented with 40 additional stars from Tobias Mayer, he deduced that the apex must be situated near the star λ Herculis. He even offered 'a few distant hints' concerning 'the amount' of the solar motion: the parallax of Sirius and Arcturus must be less than 1″; the apparent motion of Arcturus due to the translation of the solar system was no less than 2.7″ per year. 'Hence we may in a general way estimate that the solar motion can certainly not be less than that which the earth has in her annual orbit.' In 1805 and 1806 he returned to the subject, confirming his former result by making use of the accurate proper motions of 36 bright stars taken from Maskelyne. His attempt to determine the amount of solar motion could have no success, since he assumed that the distance of every star corresponded to its apparent brightness; consequently, he found a great number of bright stars (such as those which have very small proper motions) running alongside the sun in the same direction.

Perceiving with his telescopes the wealth of different objects in the heavens, he conceived the plan of collecting and cataloguing them, so as to make an inventory of the universe. Besides double, triple and multiple stars, and well-known groups like the Pleiades, he found numerous instances of what looked like nebulous spots in smaller telescopes, but which in his large telescopes appeared to consist of thousands of stars. Moreover, his telescope showed him numerous smaller nebulae; were they in reality also clusters of still smaller stars? In systematic 'sweeps' of the telescope over successive belts of the sky, he had assembled since 1783 all the curious objects he had come across. So he was able to publish in 1786 a 'Catalogue of one thousand new nebulae and clusters of stars', arranged in different groups according to their appearance and accompanied by short descriptions. In 1789 it was followed by a second catalogue of more than a thousand objects, and in 1802 a third list of 500 was added. There was no longer any confusion with comets; the nebulae had been advanced to celestial objects in their own right, as systems of suns or as worlds in the larger universe.

How large is this universe? What is its structure? Does it consist of an immense number of solar systems similar to our own? In 1750 Thomas Wright had already considered that what we see as the Milky Way encircling the sky was the projection of a gigantic system extending farthest in the plane of the luminous belt. Kant had adhered to that view. In 1761 Lambert elaborated a theory according to which the thousands of stars surrounding the sun constituted a system, and the Milky Way, because it consisted of a large number of such systems, was a system of higher order. There might be an even larger number of Milky Way systems in space, forming a still higher system. All these opinions, however reasonable they might appear, were mere speculation and fancy. Herschel was the first who attempted to determine the extent of the stellar system by systematic observations.

In the first of his two papers, 'On the Construction of the Heavens', published in 1784 and 1785, he states: 'On applying the telescope to a part of the *via lactea* I found that it completely resolved the whole whitish appearance into small stars.'[179] Hence the study of the Milky Way could restrict itself to counting the stars. Assuming the stars to be equally luminous and equally scattered in space, i.e. mainly standing at equal distances from one another, he could deduce from the number of stars counted in the field of his telescope how far the system of stars extended in that direction, i.e. the depth of the starry universe. So he called this counting 'Gaging (gauging) the Heavens' or, in short, 'star-gauges'. More than 3,000 of such counts were made, condensed in his second paper into values for a good 400 points of the sky. The result was the well-known disc—or lens-shaped star agglomeration, extending in the plane of the Milky Way over 800 times, perpendicular to this plane 150 times the mean distance of two stars, which was supposed to be the distance from Sirius or Arcturus to the sun. An often-reproduced figure of a perpendicular section of the system was added, in which the division of the Milky Way into two branches (about half its circumference) was also shown (pl. 9). 'That the Milky Way is a most extensive stratum of stars of various sizes admits no longer of the least doubt; and that our sun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gauged this shining zone in almost every direction and find it composed of stars whose number, by the account of these gauges, constantly increases and decreases in proportion to its apparent brightness to the naked eye.'[180]

Herschel did not restrict himself in these papers to the simple determination of distances and dimensions. In his telescope he had seen so many different forms of stellar agglomerations in gradual diversity that his thoughts were naturally occupied by the question of what was their origin. He explained that, by their mutual attraction, stars that formerly were evenly spread must concentrate into regular or irregular condensations, with voids left between them. He called it the 'formation of nebulae', indicating that what is seen as a nebula in the telescope consists of a cluster of very small and distant stars. In the irregularities and cloud forms of the Milky Way this clustering tendency was clearly visible. Extensive nebulae he called 'telescopic milky-ways'. Our Milky Way is of the same kind as other nebulae. 'I shall now proceed to show that the stupendous sidereal system we inhabit . . . consisting of many millions of stars, is, in all probability, a detached Nebula.' The chapter in his paper of 1785 containing this discussion is superscribed by the thesis: 'We inhabit the planet of a star belonging to a Compound Nebula of the third form.'[181]

It is easily understood that such speculations so widely extending the realm of the universe roused enthusiastic admiration in some of his contemporaries, but in many others met with sceptical doubt; no other astronomer had seen all these celestial marvels. His ideas of the evolution of stellar systems and clusters were still more strange to them. Herschel had been nurtured in his youth on the critical rationalism of the eighteenth-century Continental concept of nature that was entirely foreign to the free, but strongly conservative, English mind of those times. In the biography entitled *The Herschel Chronicle* his granddaughter, Constance Lubbock, writes: 'Herschel was the first to introduce a disturbing factor into this view of Creation by his suggestion that it was a long process, not a sudden and completed act. Perhaps one reason for the coldness with which these papers were received by the Royal Society may be the fact that thought was not yet so free in England as in France and Germany. At this time no one could hold any high position in either of the English Universities or in the teaching profession unless he took orders in the Church. The Astronomer Royal was a clergyman as well as Dr Hornsby at Oxford and the Professor of Astronomy at Edinburgh. Under these circumstances it was natural that there was a certain reluctance to show approval of Herschel's theories, which seemed to run counter to the accepted interpretation of the Biblical account of Creation.'[182]

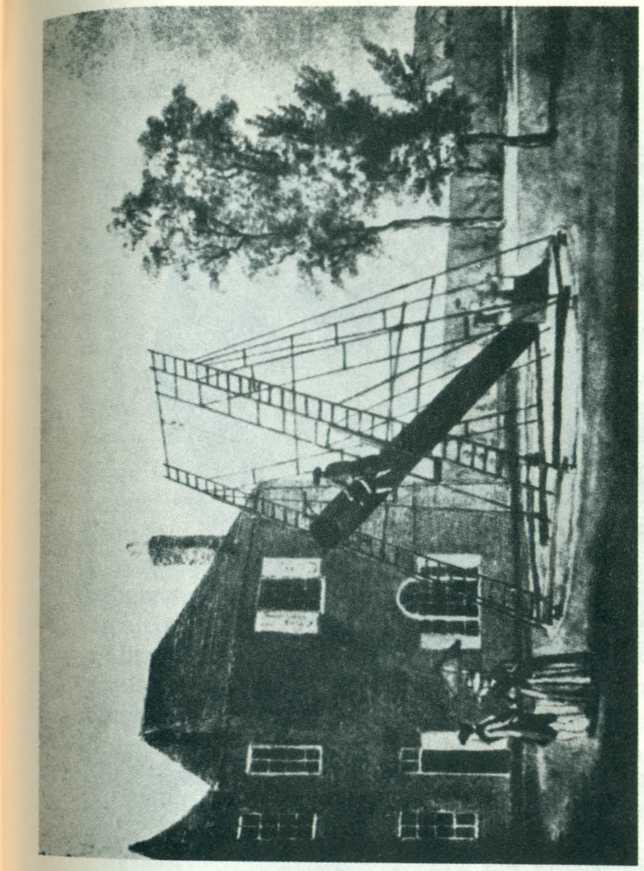
His ideas on clusters and nebulae, however, did not remain the same when, in continuing his observations, he discovered new kinds of celestial objects. He was struck by the appearance of stars surrounded by a faint milky luminosity, uniform throughout; if it were produced by countless faint stars, they must be excessively small, or if they were normal, the central star must be an enormous body; 'we therefore either have a central body which is not a star, or have a star which is involved in a shining fluid, of a nature totally unknown to us.' Thus he wrote in a paper 'On Nebulous Stars, properly so-called' in 1791; and he continued: 'What a field of novelty is here opened to our conceptions! A shining fluid, of a brightness sufficient to reach us from the remote regions of a star of the 8th, 9th, 10th, 11th, or 12th magnitude, and of an extent so considerable as to take up 3, 4, 5, or 6 minutes in diameter! Can we compare it to the coruscations of the electrical fluid in the aurora borealis? Or to the more magnificent cone of the zodiacal light as we see it in spring or autumn?'[183] That it can exist also without a central star was shown by the no less marvellous round or elongated nebulous discs, which, because of their uniform light, he had called 'planetary nebulae'. They could now be explained, 'since the uniform and very considerable brightness of their apparent disc accords remarkably well with a much condensed, luminous fluid; whereas to suppose them to consist of clustering stars will not so completely account for the milkiness or soft tint of their light'.[184] The same holds for the great

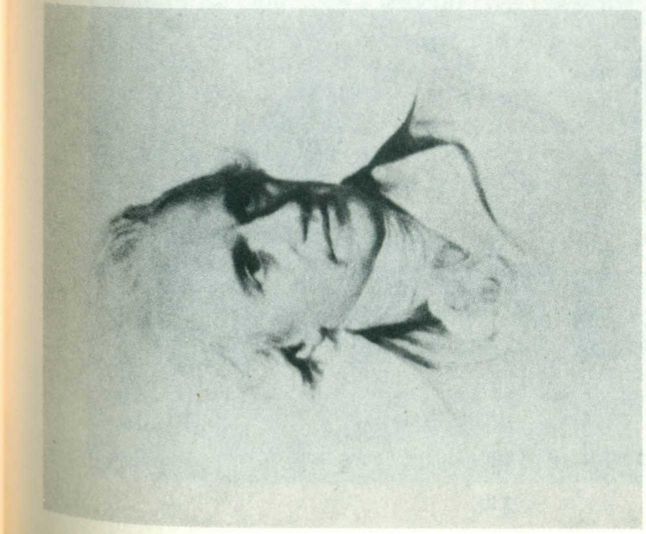
nebula of Orion, of which he wrote in 1802 that for 23 years he had seen many changes in its shape and lustre; and he added 'To attempt even a guess at what this light may be, would be presumptuous.'[185]

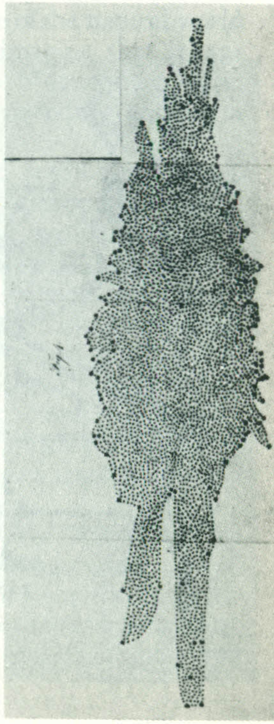
Now his ideas on evolution, too, were reversed. The faint shine of small nebulae not dissolved into stars, which formerly he had supposed to be far-distant agglomerations and, as such, the ultimate result of clustering forces, had been recognized as thin nebulous matter, which was now placed at the beginning of evolution as primitive matter, out of which stars are formed through condensation. We see some of these nebulous discs contracted in different degrees and with central stars of different brightness. 'When we reflect upon these circumstances, we may conceive that, perhaps in progress of time these nebulae which are already in such a state of compression, may be still farther condensed so as actually to become stars' (1811).[186] And some years later he wrote: 'We shall see that it is one and the same power uniformly exerted which first condenses nebulous matter into stars, and afterwards draws them together into clusters, and which by a continuance of its action gradually increases the compression of the stars that form the clusters' (1814).[187]

Herschel in all these researches dealt for the first time with the loftiest and most far-reaching problems of the structure and development of the universe. Other phenomena of the fixed stars at the same time drew his attention, especially the variations in brightness of some stars, which occupied two of his friends, amateurs like himself. In 1782 John Goodricke discovered the regular character of the variations of the second-magnitude star Algol (β Persei), whose variability Montanari had perceived a century earlier. Always, after a period of 2 days 21 hours, the star showed a decrease to the fourth magnitude, which Goodricke explained as a periodical obscuration by a dark body revolving about Algol in that period. Two years afterwards he discovered the regular variations of δ Cephei and β Lyrae; and Edward Pigott in 1785 found η Aquilae and a small star in the constellation Scutum to be variable. Herschel in these years also occupied himself with the magnitudes of the stars. He noticed that the sequence of brightness which he observed in a constellation often deviated strongly from the magnitudes assigned to them by Flamsteed's catalogue and also contradicted the sequence of Greek letters assigned to them by Bayer. He supposed that in many cases the stars had, perhaps gradually, changed their brightness. To investigate such changes he conceived the need for a designation of the relative brightness of the stars more precise than that given by the coarse datum of their magnitudes. So he devised a system of signs composed of points, commas and dashes to indicate their estimated greater or lesser differences in brightness. In this way he compared all the stars numbered by Flamsteed with each other; he published the comparison in 1796–99 in four catalogues, and two more catalogues, completing the constellations, were published from his manuscripts after his death. His expectation of discovering many variable stars thereby was not fulfilled; α Herculis was the only star in which he detected, in 1795, small fluctuations. His method of comparing small differences in brightness remained unnoticed until, long afterward, Argelander drew attention to it.

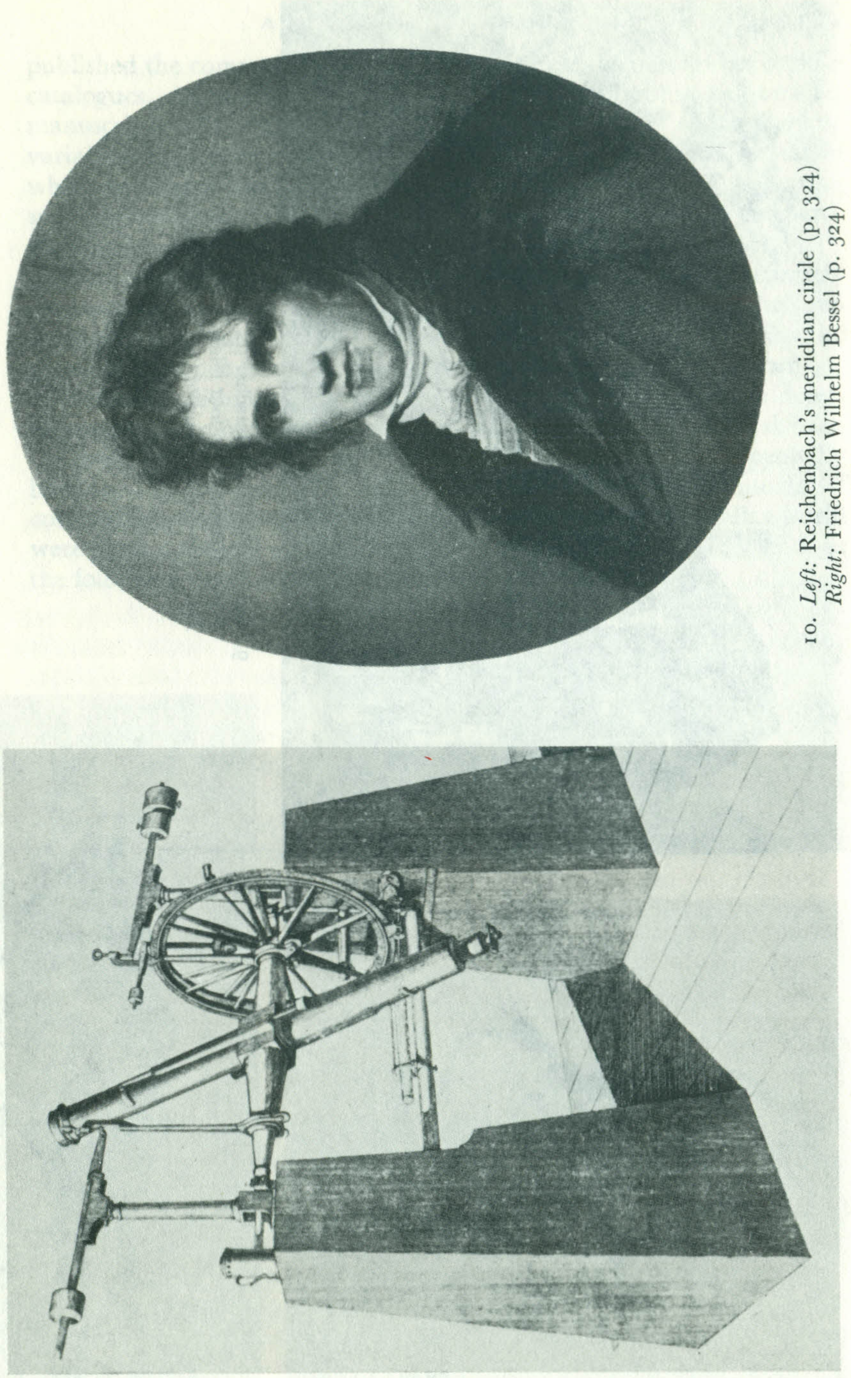
Through all these researches the world of the fixed stars was definitely incorporated into the realm of actual astronomy. That Herschel had been able to accomplish this was due in large part to the fact that he came into science from the outside as a self-taught man. Free from the burden of tradition, which for those educated in the profession determines the realm of their duties and the field of acknowledged activities, he could stray outside along untrodden paths. This has repeatedly happened in astronomy. Now, as the culmination of four centuries of constructive revolution, the gates to the wide spaces of the stellar world were flung open and the ways were cleared for the progress of science in the following centuries.







9. *Top left:* William Herschel (p. 312)
Right: Herschel's 20-foot telescope (p.314)
Left: Herschel's section of the Milky Way (p. 317)



10. *Left:* Reichenbach's meridian circle (p. 324)
Right: Friedrich Wilhelm Bessel (p. 324)

CHAPTER 32

THE TECHNICAL BASIS

'We cannot flatter ourselves that the instruments, even if still further perfected, will allow us to advance farther and to increase the accuracy of the measurements beyond one second of arc. It is quite possible that Bradley has fixed therein the limits of our knowledge.'[188] Thus in 1782 wrote the renowned historian of astronomy, Jean Sylvain Bailly, who in later years, as president of the Constituante, played a prominent part in the French Revolution. Admiration for the progress achieved—1″ is indeed a small quantity, it represents $\frac{1}{100}$ mm. on a circle of 2 metres radius—is here combined with the naïveté of the eighteenth-century citizen, for whom spiritual and scientific evolution—as also the political evolution of mankind in the near future—would be completed by the rule of reason and the knowledge of the natural world order, now well-nigh established. Who could imagine that it was only the prelude to an ever more rapid tempo of unending social development, carrying in its wake an equally unending growth of science?

The Industrial Revolution, with its profound social convulsions, began in England, in the second half of the eighteenth century. In the course of the nineteenth century it spread over the adjacent countries of Europe, over the United States of America and finally over the entire world. Technical progress was its basis; the small artisan's implements of old were superseded by more productive, ingeniously constructed machines, soon to be moved by the powerful steam engine. Out of handicraft and small business arose capitalistic big industry, controlling the economic system more and more, until finally it completely changed the aspect of the world. Man himself also changed; fierce competition on the part of the manufacturers brought about a new tension in social life and aroused a restless energy.

In the European world of absolutism and privilege, dominated by landed property and commercial capital, arose a strong middle class of industrialists and businessmen, the leaders of the new society. In fierce revolutions (as in France) or in thorough-going reforms (as in England) it built up its political power and its social dominance. Its fundamental ideals of personal initiative and unrestricted freedom of trade, thinking and action became the dominant principles of human life.

In this economic development exact science became at an increasing rate the basis of the new techniques. The old traditional working methods were replaced by the application of scientific discoveries. The rising middle class promoted the study of nature by founding universities and laboratories, because it felt that knowledge of nature was good and beneficial. Science was encouraged not only because of its technical use, to increase the productivity of labour, but also because it evoked a freer mode of thinking. It freed the mind from the bondage of tradition and became an inspiring spiritual power of knowledge and enlightenment, in which broader sections of the people gradually participated. Progress and enlightenment (*Aufklärung*) became the slogans of the new age.

That this interest in science was not due solely to its practical utility for technical progress is shown by the large share which astronomy held therein. Astronomy, next to mathematics, then the most developed and esteemed of the sciences, was the field most appropriate for the disinterested search for pure knowledge. New observatories were founded, sometimes from love of science by groups of wealthy private citizens (as the Harvard Observatory was founded in 1844 by the citizens of Boston), or in connection with universities as the first examples of what later for other sciences were called 'research institutes'. It was a token of the prominent place that pure science occupied in the mind of man in the nineteenth century.

The rise of big industry created the technical basis for the progress of astronomy. The construction of machines for factories and transportation demanded the development of new, highly perfected techniques of iron and steel, capable of producing exactly fitting parts and precisely round axles for rapidly rotating wheels. This perfected metal industry, the basis of all nineteenth century engineering, also made it possible to raise astronomical instruments from their former imperfection as products of handicraft to increasing perfection. Able technicians now came forward, and in their workshops, with all their skill and devotion, constructed carefully finished instruments out of the new materials. The renowned English firms of Ramsden, Cary and afterwards Troughton and Simms maintained the old standards; but now they found competitors in new German workshops. Influenced by the general European development, Germany in the last part of the eighteenth century experienced a new spiritual uplift in a flowering of literature, of music and of philosophy, which was followed, but not until half a century later, by an economic and political rise. The study of natural sciences participated in this rise, and in the new century German science was next in importance

to those of England and France. At the outset Germany played a role in the renewing and refining of precision techniques. Workshops for the construction of instruments were established; outstanding among these were the ones founded by J. G. Repsold at Hamburg in 1802, which remained in the forefront during the entire nineteenth century, and by G. von Reichenbach in 1804 at Munich. Usually such shops began with small instruments, altazimuths for geodetic work and sextants for navigation; then gradually they ventured on larger tasks.

From the German workshops emerged a new type of instrument for the determination of stellar positions. By fixing a graduated circle perpendicularly upon the horizontal axis of a transit instrument used for meridian transit, this was transformed into a 'meridian-circle' or 'transit circle', suited for measuring right ascensions and declinations at the same time. Now the old mural quadrants could be done away with. They had always been rather clumsy instruments. Because the errors of position and graduation were difficult to determine, the constructors attempted to keep these insignificant by heavy construction and solid foundations. With the meridian circle, on the other hand, precise finish and easy determination of remaining errors were aimed at. Accuracy of the declinations had formerly been obtained by using a large radius of the circle and then making it tractable by taking a quadrant instead of a complete circle. Now the radius was taken smaller and smaller, so that the circle was less deformed by temperature influences and by flexure through gravity. The errors of complete circles turning with the axis and read by microscopes in a fixed position were smaller and could more easily be determined or eliminated. The accuracy of the readings was secured by the sharply engraved division marks viewed through a microscope and read at first by a vernier, later on mostly by a micrometer. When one revolution of the screw corresponds to 1′ on the circle and the screwhead is divided into 60 parts, the tenth of a second can be directly read. The telescope attached in the middle of the axis was considerably improved by providing it with an achromatic objective of large aperture, at least four inches, so that the sharp, round images of the stars could be accurately bisected and stars down to the ninth magnitude could be seen in an illuminated field without difficulty. Through the strong magnification, a star was seen rapidly passing the wire reticle in the focal plane; by slowly moving the telescope, the observer could make the star follow the horizontal wire exactly; and listening to the ticking of the clock he estimated in tenths of a second the moments it passed the vertical wires. A different method of observing the time of transit came from America about 1844; instead of simultaneously looking at the star and listening to the clock, the observer simply registered the moments of transit across the wires on a chronograph by tapping the signal key in his hands, while the clock registered its seconds. Observation of the transits was thus made easier and more accurate; the accidental error of one transit was about 0.06 of a second of time only, and that of the combined result of many successive wires was accordingly smaller.

The refinement of the instrument would have been of no avail, in fact would have been impossible without the astronomer, who by the exigencies of his demands drove the technicians to persevere with improving the instruments, often himself directing their new designs. The pioneer in this field of precision astronomy in the first half of the nineteenth century was the Königsberg astronomer Friedrich Wilhelm Bessel (1784–1846). Like so many first-rate astronomers, he had come to astronomy from the outside. A clerk in a merchant's office in Bremen and wishing to extend his opportunities in trade by sea travel, he studied books on navigation and astronomical geography and delved ever more deeply into astronomical theory and practice. Gifted in mathematical theory and no less persistent in practical measuring and computing, he performed all his work with a thoroughness and accuracy far beyond the relatively coarse quality of the data. By his first published paper, a reduction of Harriot's observations of Halley's comet in 1607, highly praised by Olbers and Von Zach, he made his entrance into the guild of astronomers. Soon he gave up his business to become an assistant at Schroeter's private observatory at Lilienthal and in 1810 he was called to Königsberg to found there a new observatory. The masterpiece that gave him a leading place in the astronomical world was the reduction of the observations of Bradley, whose journals had recently been published in full. Because Bradley had carefully determined the errors of his instruments or noted the data from which they could be derived, this was the best material the previous century could afford. Bessel had not only to derive the instrumental errors from the observations themselves but also the astronomical constants needed for the reduction, such as aberration, nutation and refraction. So Bessel could rightly call his work, when it was published in 1818, *Fundamenta astronomiae*. The more so because the exactitude of his reduction, which went beyond the quality even of Bradley's work, put up a new and higher standard for the instruments as well as for the astronomers working with them. In Königsberg he installed in 1820 a new meridian circle made by Reichenbach, to set the example (plate 10); and after a full life of astronomical practice he added in 1841 a greater instrument by Repsold, provided with all new improvements.

Bessel proclaimed the principle that an astronomical measuring instrument never corresponds to its abstract mathematical ideal and therefore can give exact results as produced by an ideal instrument only by determining all its errors and applying corresponding corrections to the measured quantities—provided that the errors are constant because of the solid construction of the instrument. 'Every instrument,' he said in a popular lecture in 1840, 'in this way is made twice, once in the workshop of the artisan, in brass and steel, and then again by the astronomer on paper, by means of the list of necessary corrections which he derives by his investigation.'[189] The astronomer can measure with greater precision than the artisan can construct. He has to determine carefully all special quantities characterizing his instrument and all the small deviations from the ideal form and to compute the ensuing corrections. The optical axis of the telescope is not exactly perpendicular to the axis of rotation, which itself is not perfectly level and directed east-west; the steel pivots by which the axis rests in the V-shaped bearings are not exactly equal in diameter and not even exactly circular. The graduation marks do not have exactly equal distances, however small the deviations in the best instruments may be. There are margins of error everywhere of thousandths or even hundredths of millimetres. Many of these deviations depend on temperature, on weather, and on the time of day in general, or are changing gradually; so the observations have to be made in such a way that the errors may be eliminated or determined. Moreover, the observational results have to be corrected for the changes occurring in the heavens by precession, nutation and aberration. In order to facilitate their computation and to have them used by all astronomers in the same way and by the same amount, Bessel, beginning in 1830, published his *Tabulae Regiomontanae* (Königsberg Tables), which were used by every astronomer and at last became a constituent part of the astronomical almanacs.

Constructed and handled in this way, the meridian circle dominated astronomical measurement in the nineteenth century. It was the chief instrument of the numerous newly-founded observatories, of the smaller ones attached to almost every university, as well as of the large central institutes like Greenwich, Paris, Washington and Pulkovo—here supplemented by a 'vertical circle' for declinations. It was in use continually, and the catalogues of right-ascensions and declinations, in which the work of many centuries was brought together, appeared by the dozen. The standard of precision can be judged by the unit used, 0.01″ in the declinations, 0.001 seconds of time in the right ascensions. With this increase in exact work Bradley, who in Bessel's reduction had to supply all the elements, was now only needed to procure the proper motions of its 3,000 stars. A new reduction with modern data by A. Auwers, published in 1888, gave good positions—though these observations of one century earlier were of course not quite up to the new standard. Even this was no longer needed at the beginning of the twentieth century; Lewis Boss, in deriving the proper motions for a new star catalogue, had at his disposal observations of the new high quality extending over nearly a century. In all this nineteenth-century work England, which had led in the eighteenth century, was now following more slowly the progress on the Continent; not until after 1835, when G. B. Airy became Astronomer Royal, was Greenwich provided with a meridian circle of German model. This lag in quality was compensated by unbroken continuity in the observation of the stars, the sun, the moon and the planets. Greenwich could be compared with an old-established house of conservative routine, solid reputation and a fixed clientele, viz. all the world's navigation; before long, Cape Town and Washington shared in the work.

The object of this meridian work, the basis of precise modern knowledge of the universe, was the determination of the positions of the stars, chiefly in respect of accuracy and extent. Firstly, the positions of a small number of fundamental stars (36 most fundamental Maskelyne stars, or 400 almanac stars, or even 3,000 Bradley stars) were determined with the utmost precision, built up entirely independently without taking over anything from other sources. Secondly, the positions of the tens of thousands and more of telescopic stars were found by connecting them with the system of fundamental stars as their basis. The first task—to establish a reliable system—was the most difficult; the main efforts of the ablest observers were devoted to this work. The importance, on the other hand, of having proper motions of very faint stars was so great that Bessel himself, between 1821 and 1833, devoted many years of his Reichenbach circle to this work. It was called 'zone work', because the stars caught up in successively passing the meridian at the same place had nearly the same declination and formed a zone or belt. Such mass work on faint stars had already been done by Lalande in Paris between 1788 and 1803, with a more primitive instrument; other astronomers followed his example. Yet all these stars, picked up more or less by chance and irregularly distributed, did not constitute a complete collection. Completeness became possible only when, in 1871, the Astronomische Gesellschaft organized the work upon a co-operative basis. Thirteen observatories (afterwards increased to 16) were each assigned a zone of declination, 5° to 10°, or 10° to 15°, and so on, in which each had to observe, according to a common plan, all the stars in the ninth magnitude from lists prepared beforehand. It took scores of years before this programme of more than 100,000 stars was completed, because it also had to be extended down to the South Pole. For the stars of Bessel and Lalande and some others proper motions could now be derived; yet the basic thought in the work, directed more to the future than to the past, aimed at the amount of good proper motions

that would be available when it would be repeated in the twentieth century.

The methods of the meridian-circle work did not, of course, remain at the level first attained. Numerous difficulties stood in the way of the realization of the ideal of precise measurement, which compelled the astronomers to investigate and unremittingly to improve their working methods. When, according to the small differences in the separate results, a high accuracy, expressed by some hundredths of a second, seemed to be reached, the disappointment was great when much larger differences, of many tenths or even entire seconds, were found between the final results of different observatories. They were of a systematic character, and it was possible, by tables of corrections gradually changing with the position in the heavens, to reduce each catalogue to some average or standard catalogue constructed from the best data. However, no guarantee could be given that such a standard catalogue did not have systematic errors of the same order of magnitude, due to errors common to all the observations.

Sources of such errors, similar for different observers and instruments, were easy to discover. The right ascensions were entirely dependent on the constant rate of the timepiece. The chief astronomical clock of an observatory does not serve to give information about the time; it is an instrument for astronomical measurement, indicating the regular rotation of the celestial sphere. Able clockmakers, with the utmost care, tried during the entire century to provide astronomers with clocks of increasing perfection. Yet fluctuations in air pressure and temperature, especially along the pendulum, as well as small irregularities, vibrations and frictions can influence its rate; and a small difference in rate between day and night can cause considerable systematic errors in the right ascensions. To diminish them, the chief clock is often enclosed in an airtight case and suspended from a strong pillar in an underground room. The timepieces themselves were improved by allowing the pendulum to swing free from the other parts as much as possible. This principle was most perfectly realized in the clock constructed by Shortt which came into use about 1924. Here a pendulum (the master-clock) swinging entirely free, had nothing to do but to keep the other pendulum (the slave clock) going at the right pace, while, in its turn, it is kept going by regular small impulses from the latter.

Another source of systematic differences in right ascension are the so-called 'personal' errors already discussed and pointed out by Bessel. Observers always wrongly estimate or register the moment the star image crosses the wire by nearly the same amount, usually late by anything up to several tenths of a second. Every observer is in error to a different extent; with increased training this personal error does not become smaller but more constant. Experiments with artificial stars showed, moreover, that the error depends largely on the brightness of the star and on its apparent velocity across the wires. Thus systematic errors in right ascension, depending on declination and brightness, were produced. In order to remedy this evil, Repsold in 1889 introduced the travelling wire; the observer by slowly turning a screw keeps the wire constantly bisecting the moving star, while the frame automatically makes the electric contacts which are registered and afterwards read. This method of observing is not absolutely free from personal error—every observer has his special way of keeping the wire to the right or the left-hand side of the true position—but these errors are a tenth or less of the errors involved in the old method, and so are the variations caused by brightness. With the personal errors reduced to some few hundredths of a second, the sources of error for the right ascensions are, to a great extent, removed.

It was more difficult with the declinations. In describing the meridian from the south through the zenith to the north, the telescope has a differently inclined position relative to gravity and to the horizon for every declination. Owing to irregularities in the metal parts complicated flexures arise, not only of the circles but also of the telescope, which can only be imperfectly derived from measurements with special contrivance in horizontal and vertical positions. Far worse are the effects of the refraction in the atmosphere, which in earlier centuries was an impediment to a good determination of the stars' positions. Able mathematicians—beginning with *citoyen* Kramp in Cologne in 1800 (Year VII of the Republic), whose work was used by Bessel—developed and improved the theory of the refraction during the entire nineteenth century and computed its variation with the altitude of the star. But large uncertainties remained, especially near the horizon, chiefly because the decrease in temperature and density in the higher atmospheric layers was not well known. What this means is shown by the fact that at an altitude of 30°, where no astronomer hesitates to make good measurements, the correction for refraction amounts to 160″, so that, to compute it correctly to 0.01″, the result must be certain to $\frac{1}{16000}$ of its amount. Here we have a main source of systematic error. Nor is this the worst; far more serious are the unknown irregularities in the refraction for which we cannot account. The air layers of different density may be inclined, or there is (as often in clear frosty weather) a rise in temperature with height. The temperature inside the observing room is usually somewhat higher than outside, in spite of attempts to equalize them, and the dividing line follows the irregular course of walls and roof. All these influences vary with the different latitudes at which the observatories are situated but nowhere are they absent. So it is not surprising

that the most carefully derived catalogues show large systematic differences and that even the adopted standard system may be considerably wrong. So we can understand Kapteyn's complaint in 1922, shortly before his death: 'I know of no more depressing thing in the whole domain of astronomy than to pass from the consideration of the accidental errors of our star-places to that of their systematic errors. Whereas many of our meridian instruments are so perfect that by one single observation they determine the co-ordinates of an equatorial star with a probable error not exceeding 0.2″ or 0.3″, the best result to be obtained from a thousand observations at all of our best observatories together may have a real error of half a second of arc and more.'[190]

Kapteyn showed the probability that Lewis Boss's standard system, then considered the best, contained considerable errors because the proper motions derived for some zones of declination were directed too much to the north, and for others too much to the south. A new standard system, derived by Kopff at the Berlin Astronomical Computing Office from the best modern series of observations, in fact deviated from the Boss System at these points. To decide the question an entirely different method of observing was required, where flexure and refraction would play no role in the determination of declinations. For an observer on the earth's equator the celestial poles are situated on the horizon, and all the stars of the same right ascension with declinations from +90° to −90° are situated beside one another on, or a little above, the horizon. Declinations can then be measured as azimuths with a horizontal circle, free from refraction. Led by these considerations, the Leiden Observatory in 1931–33 sent two observers to Kenya in East Africa to measure declinations by means of an accurate azimuth instrument. Their results indicated the need for systematic corrections, as was expected (+0.6″ at −30°; 0″ at +10°; +0.3″ at +30° of declination), so that the Berlin system was shown to be nearer the truth. The way to a faultless system of declinations by improved repetition seems to have been opened up.

We now return to the beginning of the nineteenth century, to view the progress of technical knowledge in another domain—in optics. Dollond's introduction of the achromatic lens systems had brought about a great improvement in the measuring instruments. Because glass-making methods were quite imperfect, however, it was not until the nineteenth century that the telescopes themselves gained in perfection and power. This was due mainly to the work of a young and gifted Bavarian, Joseph Fraunhofer (1787–1826). In 1806 he entered the workshop of the Utzschneider and Reichenbach firm, where he was soon at the head of optical work. In 1817 the departments were separated, and in association with Utzschneider, a rich financier with strong scientific interests, Fraunhofer founded an Optical Institute at Munich. Two imperfections had to be overcome. In the first place, the values of the refraction indices for the different kinds of glass which were needed in computing the lenses were very imperfectly known, 'so that, with all accuracy in the computation of achromatic objectives, their perfection is doubtful and seldom comes up to expectations'.[191] Moreover, the materials were inadequate, especially English flint glass, 'which is never entirely free from striae'. He was initiated into the secrets of glass manufacture by his colleague, P. L. Guinand, an able French-Swiss glassworker. After twenty years of experiments Guinand had succeeded, in 1799, in fabricating faultless discs of flint 10 to 15 cm. in diameter; and afterwards he was able to produce even larger discs, up to 30 and 35 cm., by welding good small pieces in softened condition. His skill and craft traditions were preserved and maintained afterwards in the French glassworks.

Fraunhofer investigated deeply the refraction of light of different colours in various types of glass by means of exact measurements with a theodolite. He was continually hampered by the difficulty that the red or yellow light to which his measures related could not be exactly identical and recovered. At last, on experimenting with the prismatic solar spectrum, he discovered the fine black lines crossing it, which afterward were called by his name, 'Fraunhofer lines'. 'By means of many experiments and variations,' he wrote in 1817 in a paper for the Bavarian Academy, 'I have now become convinced that these lines and streaks belong to the nature of the solar light and are not caused by diffraction or appearances.'[192] He counted more than 500 lines, and the strongest, which he marked by the letters *A* to *H*, could now be used as precise marks for definite kinds of light. By measuring their indices of refraction for each kind of glass he was able to compute exactly the combination of lenses producing the most perfect achromatism. The optical industry here made the transition from skilled craft to scientific computation. The practical result, however, was not greater simplicity. By his experiment Fraunhofer found that the refrangibility of the different colours was not strictly proportional in the different kinds of glass; hence it was not possible to bring together all colours exactly into one focus; there remained small differences, producing the 'secondary spectrum'. Moreover, the theory of lens combinations was too complicated and difficult to supply the best forms through mere computation. Nevertheless, through a combination of practical intuition and theoretical knowledge, he succeeded in making excellent objectives for telescopes, up to 24 cm. (9 inches) in diameter. These objectives produced small, sharp, circular stellar images over the entire field of view, showing not only the faintest stars as well as larger mirror telescopes do but

also the most delicate details on the surface of the planets and the moon.

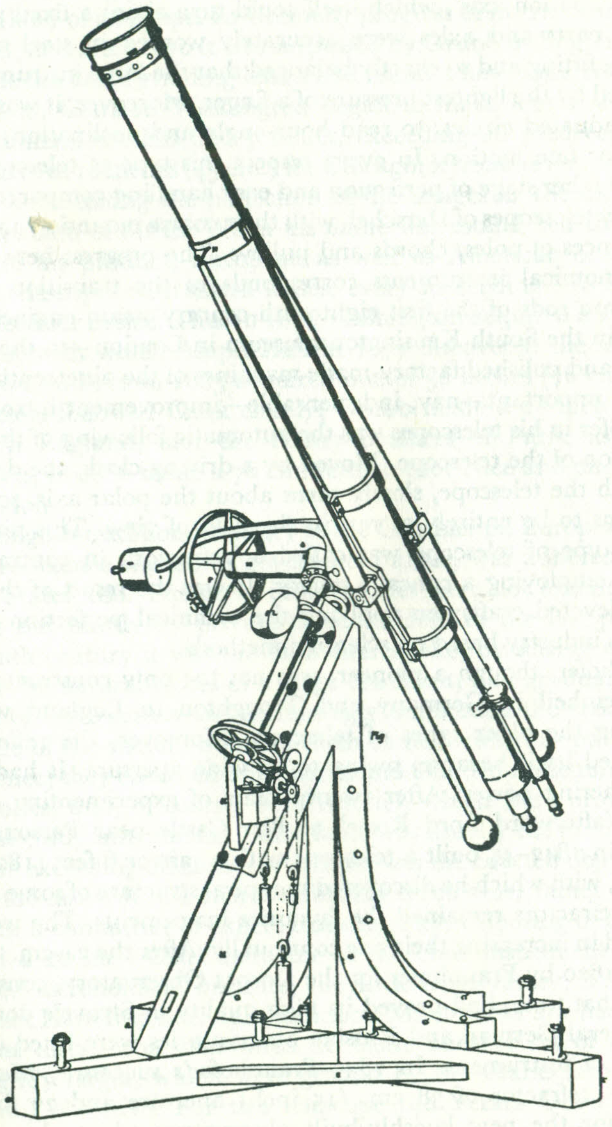


Fig. 30. Fraunhofer's telescope

The mounting of the telescopes was also improved. In the Fraunhofer or German mounting (fig. 30), the tube—in the first smaller telescopes of wood, later of metal—was attached at right angles to the end of a 'declination axis', which itself could turn about a fixed polar axis. All the parts and axles were accurately worked in steel and brass, perfectly fitting and so exactly balanced that the large instrument could be moved by the lightest pressure of a finger. Moreover, it was provided with graduated circles, to read hour-angle and declination, and with screws for fine motion. In every respect this type of telescope constituted a higher stage of perfection and easy handling compared with the unwieldy telescopes of Herschel, with their coarse mounting and moving contrivances of poles, chords and pulleys. The progress here apparent in astronomical instruments corresponds to the transition from the coarse iron rods of the first eighteenth-century steam engines—still to be seen in the South Kensington museum in London—to the carefully finished and polished factory-made machines of the nineteenth century. Another important—nay, indispensable—improvement introduced by Fraunhofer in his telescopes was the automatic following of the stars by the motion of the telescope. Moved by a driving clock, the declination axis, with the telescope, slowly turns about the polar axis, so that the star seems to be entirely at rest in the field of view. This nineteenth-century type of telescope was called a 'refractor', in contrast to the reflector employing a concave mirror. It was the result of the refined skill of devoted craftsmen applying the technical perfection of metal and glass industry based on scientific methods.

Fraunhofer, though a pioneer, was not the only constructor in this field; Steinheil in Germany and Troughton in England were also improving the older types of telescope. Moreover, the reflector still maintained itself because, owing to its wide aperture, it had a large light-gathering power. After twenty years of experimenting, William Parsons (afterward Lord Rosse) at Birr Castle near Parsonstown in Ireland, in 1842–45 built a telescope with a mirror 6 feet (182 cm.) in diameter, with which he discovered the spiral structure of some nebulae. Yet the refractors remained the favourite instruments. The workshops succeeded in increasing their size continually. After the 24-cm. telescope made in 1820 by Fraunhofer for the Dorpat Observatory, considered a giant at that time, had proved its high quality in Struve's double-star work, several German and Russian observatories were fitted out with this type of instrument. In 1839 Fraunhofer's successor, Merz, constructed a refractor of 38 cm. (15 inch) aperture and 22 foot focal distance for the new lavishly-built observatory which, by order of Tsar Nicholas I, was erected as a central scientific institute on the hill Pulkovo, south of St Petersburg. A similar instrument was constructed for Harvard Observatory at Cambridge, Massachusetts, when it was founded in 1843–47 by the citizens of Boston.

In the following years many observatories in Europe were provided with this kind of telescope. In Germany progress in instrument-building slackened, and after 1870 it was surpassed by Grubb in England. In the meantime the art of grinding lenses had found a new home in America, when Alvan Clark in Washington began to make lenses of unprecedented dimensions. His first product, exceeding all predecessors, was an objective of 18 inches (43 cm.) for Chicago; it became famous because his son, while testing the perfection of the images in 1862, discovered the companion of Sirius. When his name was established through the quality of his product, European as well as American observatories ordered telescopes with Clark lenses, every succeeding lens surpassing the predecessor in size. Thus in 1871 Washington acquired a 26 inch (66 cm.) lens, with which Asaph Hall in 1877 discovered the satellites of Mars, and Pulkovo in 1885 acquired one of 30 inches (76 cm.). Other constructors followed Clark closely; Grubb made a 25 inch for Cambridge in England, and the Henry brothers of Paris, astronomers themselves, in 1886 made a 76 cm. refractor for Nice and one of 83 cm. for Meudon.

The English tradition, contrary to the Continental European custom, that pure science was not a government affair, was adhered to in the United States also. The great observatories were not founded by universities but mostly by private persons. Here in the last part of the nineteenth century it was the millionaires who, by seizing and monopolizing the natural riches of a large continent, had gradually become masters of big business and increasingly dominated the economic and social life of the nation. Some of them came forward as patrons of art and science; they endowed universities and founded museums, libraries, laboratories, and, of course, observatories, which were provided with the most costly instruments. Since fame was usually the motive, a giant telescope exceeding all earlier telescopes was the coveted object. Thus it was in the last will of a rich Californian speculator, James Lick; after his death a committee of expert trustees, freely disposing of the money, ordered a 26 inch Clark refractor as the chief instrument of a well-equipped astronomical institute, the Lick Observatory, built in 1888 on Mount Hamilton near San Francisco. Mantois of Paris had furnished the glass discs; soon afterwards he made another set of glass discs, still slightly larger, which were ground by the Clarks into a 40 inch (102 cm.) objective. For this telescope the Yerkes Observatory was founded in 1897 at Williams Bay, Wisconsin, as an institute of the University of Chicago. These giant telescopes were constructed on the same model as the former smaller ones of Fraunhofer; they show the same slender, graceful build, with a length about twenty times the aperture, and the same type of mounting. They were executed with the same care and precision but on a four times larger scale. Everything in them is more colossal, hence heavier; yet they are equally easy to handle, masterpieces of constructive ingenuity. The comparison of these giants with their prototypes of the beginning of the nineteenth century illustrates the progress of that century in all departments of mechanics and science: in the casting of glass, the grinding of lenses, the construction of metal mountings, the control of mechanisms, the organizing of big institutes. America had now taken the lead in technical progress and, therefore, also in practical astronomy.

This great development in astronomical optics depended on the progress of glass technique, but, conversely, the increasing demands of scientific research stimulated this technique. In France the workshop of Mantois and the glass factory of St Gobain, later combined, succeeded in casting ever larger and better glass discs. In Germany the Carl-Zeiss Werke at Jena, founded by Ernst Abbe, gained fame for the excellence of its optical instruments based on thorough theoretical foundations laid by Abbe himself, as well as by its social structure. It was connected with the Schott glassworks, where, in continuous experiment, attempts were made to prepare new kinds of glass with special qualities, as desired by the scientists, e.g. to remove the secondary spectrum. Though not competing with the construction of giant telescopes, the Zeiss Works has provided observatories all over the world with new instruments ingeniously devised.

The limit of size, however, was now reached. Lenses of 40 inches diameter are so thick at the centre that their absorption of the stellar light begins to be effective, and they come near to causing deformity by their own weight. If larger glass discs were constructed, they could not be used in the same way.

Again we have to retrace our steps and return to Fraunhofer's workshop. His chief merit is not that he improved telescopes as instruments for viewing the heavens but that he made of them the most perfect measuring instruments. The small distances of close stars or the dimensions of small planetary discs can, of course, be measured more accurately than the co-ordinates of single objects; many sources of error common to adjacent points disappear in their relative place. For such measurements micrometers are used. Fraunhofer provided each of his telescopes with a filar micrometer, which, by careful construction and accurate finishing of every part, was now modelled into an instrument of the highest precision. A perfectly worked screw of small pitch moves a wire relative to a fixed cross-wire, and the distances are easily read at the divided screw-head. By turning the entire micrometer about the axis of the telescope, the fixed wire is brought into the desired direction of the two stars, and this direction can also be read. Thus the relative position of the two objects is given in polar co-ordinates, i.e. as position angle and distance.

For the use of such a micrometer it is, of course, necessary that the telescope must follow the stars exactly, so that they appear to be completely at rest in the field of vision; then the wires can be pointed precisely upon the centre of the stellar images. William Herschel had not been able to do this, because his telescope did not automatically follow the stars; so, in measuring a double star, he could only bring the wires to such a distance that, by estimate, it seemed to be equal to that of the stars travelling through the field. This explains the remarkable increase in accuracy now attained. The second decimal of a second of arc now was no superfluous luxury or product of averaging; it was the natural unit of which the errors of measurements were only a small multiple. With all the later refractors up to the American giants, the filar micrometers constituted an essential, constantly-used accessory that gave them their real utility.

Another type of instrument for exact measurements, too, was developed by Fraunhofer. In the eighteenth century Bouguer had measured the diameter of the solar disc by putting two equal objectives beside one another at such a distance that the two images of the sun formed by them were in exact contact; then, knowing the focal length of the objectives, he could derive the sun's apparent diameter by measuring the distance between their centres. Ramsden improved the instrument, called a 'heliometer', by taking half-lenses, which produce the same complete solar image. In moving them alongside one another the two images could be brought to coincidence as well as at great distance, and small distances could be measured as well as large ones. Fraunhofer took up the idea and put the two halves of a good objective into frames displaced by a screw, whereupon the distance was read by means of microscopes on an exactly divided scale of millimetres. Retaining the name 'heliometer' he constructed a first-rate precision instrument for the Königsberg Observatory, able to measure far larger distances than the filar micrometer, up to several degrees. It was not ready until 1829, three years after his death. In using it for the first determination of a stellar parallax, Bessel subjected it to a thorough theoretical and practical examination, so that it acquired a birthright in astronomy and was used, up to the end of the century, in a number of important researches on stars and planets, sun and moon, where moderate distances had to be measured.

Then, however, it met with a competitor in this field of astronomical practice. From the middle of the nineteenth century photography was brought increasingly to bear upon the heavenly bodies. The first photographs of the sun and the moon showed, in short exposures, an abundance of detail that would have demanded hours and months of observing in direct drawing and mapping. Since the existing objectives, achromatized for visual rays, did not give sufficiently sharp images in the photographically active blue and violet light in consequence of the secondary spectrum, Lewis M. Rutherfurd, at New York in 1864, for the first time constructed a photographic objective of 29 cm. aperture. It provided him in the next year with the first usable photographs of the starry heavens. Every progress in photographic methods found its application in astronomy, as in 1871, when the wet collodion process was replaced by the far more sensitive dry silver-bromide-gelatin plates, that photographed faint and even telescopic stars in a short exposure. It soon appeared that the nice, perfectly round, stellar images on the plate, though rather large, were more easily and accurately measurable than the vibrating, often irregularly jumping stars in the telescope; the exposure had averaged the irregularities. Thus a new astronomical practice appeared: to use the telescope only for taking photographs and then to measure them at leisure by day.

Besides the gain in accuracy, the photographic method presented other advantages. First, all the stars of a field were pictured in one exposure, so the few clear and favourable nights might be exploited to the full. Their measuring, moreover, could be postponed until the stars were needed. Secondly, there was the increase in size and visibility of the images that could be achieved by simply lengthening the exposure time, so that the effect of larger apertures on the brightness of the stellar images was enhanced. Thirdly, in using lenses with great angular aperture—i.e. a short focal distance relative to the aperture, a great surface brightness was produced. Extended faint, nearly invisible nebulous objects, which by no optical means could be made brighter, now by long exposures revealed for the first time the beauty of their intricate structure.

Astronomy profited from the increasing perfection of optical techniques developed on behalf of practical photography since its discovery in 1839. Laborious theoretical computations, as made by Seidel and Petzval, combined with the practical inventiveness of constructors like Chevalier, Steinheil, Brashear and Zeiss, gradually created a number of increasingly more perfect types of optical systems, consisting of different combinations of lenses. It was the struggle with the problem of how, for any point of an extended object, all emerging rays of all colours could be united into one point, thus forming a sharp image situated in one flat field. The demands for great brightness, an extended field, and faultless depicting could not all be met at the same time; according to the purpose, one or another demand dominated; thus a wealth of different types has been invented and constructed, first of simple triplets, gradually improving to systems of 6 or 8 lenses. Portrait objectives of large angular aperture for the use of amateur photographers are found all over the world in thousands of cameras. With the same angular aperture of 1 : 4 or 5, in larger dimensions, they have been made especially for observatories, providing a new type of instrument that offered new aspects of the celestial objects.

If the positions of the stars are to be determined accurately by measuring them on a photographic plate, there is the difficulty that an ordinary achromatic objective consisting of two lenses depicts the object in a somewhat distorted way. This defect can be avoided by using a well-computed system of more lenses. Before this was fully realized the brothers Paul and Prosper Henry in Paris succeeded, with self-made objectives, in photographing stars down to the fourteenth magnitude. Full of enthusiasm for this result, an International Conference convoked in Paris in 1889 resolved, all too hastily, to construct by international co-operation an atlas of the entire sky down to the fourteenth magnitude and a catalogue of exactly measured star places down to the twelfth magnitude; this was the so-called *Carte du ciel* enterprise. Since the plates were 2° square only, the error of distortion was rather insignificant; but the number of plates needed now was so large that after tens of years the end was not yet in sight. Schlesinger demonstrated in 1928 that on larger plates of 5° square, provided that they are taken with the right optical systems, the star positions can be determined with the same precision as with meridian circles. Indeed, the Astronomische Gesellschaft had already resolved that its AG catalogue, started in the seventies, should be repeated not by meridian circle observations but by means of photographic plates.

With all these new practical appliances, the striving for more light, i.e. for larger apertures of the instruments, persisted. Lenses of more than 40 inches diameter were unusable; here the limit had been reached. To gather more light a return was made to the reflector at the end of the nineteenth century. A mirror not traversed by the light rays but reflecting them at the surface can be supported at the back to prevent it from bending by its weight. By giving it a parabolic shape, the rays are collected exactly in the focus. To be sure, the field presenting perfect images is small; but when single stars are investigated, as in spectral work, this does not matter. Moreover, it is possible to get sharp images over a larger field by interposing an appropriate correcting lens between the mirror and the photographic plate as was devised by Ross.

Since the beginning of the twentieth century, reflecting telescopes of increasing size have been built; instruments of 40 inch aperture became common, and soon apertures of 60, 70 and 80 inches came to surpass them. Foremost among them is the 100 inch Hooker telescope at the Mount Wilson Observatory, devised and ground by Ritchey and put into use in 1919. It won fame for its size, for solving the multitude of construction problems and still more for the amount and high quality of the research performed with it. The experience acquired in this construction induced the plan of an all-surpassing enterprise, the construction of a 200 inch telescope. The story of its gradual realization, the overcoming of difficulties and setbacks before the telescope was erected on Mount Palomar, reads like an epic of modern technique.

Modern development of astronomical instruments would not have been possible without the growth of techniques in nineteenth-century industry, which revolutionized the entire aspect of society. The same precision with which the skilled artisans of a century ago produced instruments of high perfection was now an essential part in the construction of the increasingly colossal machinery of modern giant telescopes. As in other industrial machines, great size is here united with precision in detail. In the vital parts, to secure the exact shape of the glass surface of lens and mirror, control to single thousandths of a millimetre is demanded, for which modern techniques affords its aid. The fact that heavy masses can be moved by a finger is no longer regarded as remarkable. The glass and metal masses of hundreds of tons are moved by electric motors. The astronomer, hardly visible in his cabin near the focus is, so to speak, the small brain of the huge steel organism and directs all the motions of the mammoth machine by the mere pressing of buttons. Technical precision in electrical control of gigantic instruments is the material basis of modern astronomy.

CHAPTER 33

DISTANCES AND DIMENSIONS

A KNOWLEDGE of distances and dimensions is the basic necessity for a good concept of the universe. At the end of the eighteenth century a rather rough idea of the sun's distance and of the dimensions of the stellar system had been acquired, expressed by the parallax of the sun. The dimensions of the stellar system, however, expressed in the yearly parallax of the stars, were entirely unknown.

The increased precision of nineteenth-century technical implements imposed as a first task what might be called a 'geodetic' space survey. Here man was faced with the same kind of problem as when, confined to the few square feet at the top of a hill or tower, he has to determine the distance of distant towers or mountains. Parallaxes had to be measured. Reliable distances and the structure of the outer world could not be ascertained until it was possible to measure parallaxes to hundredths of seconds.

The nineteenth century gave expression to its higher standards in a more elaborate treatment of the best and most important data of observation bequeathed by the preceding century. Bessel gave a new reduction of Bradley's observations of star places. Johann Franz Encke (1791–1865), astronomer at Gotha, undertook a new reduction of the transit of Venus in order to derive the solar parallax. In 1788 Duke Ernest II had founded an observatory on the Seeberg near his residence of Gotha; at this small court science stood in high favour, just as did literature in the neighbouring court at Weimar. For a short period Gotha was a centre of rising science in Germany; in 1796 an astronomical congress assembled here, and a first review of astronomy, called *Monatliche Correspondenz*, was published by F. X. von Zach, the director. Encke worked here after 1813 and took up the reduction of the Venus transits, before he was called to Berlin in 1825 to modernize the observatory there.

In this reduction, published in 1822 and 1824, he applied a new principle to the mathematical treatment of natural phenomena, chiefly developed by Gauss in the preceding years. In former centuries the astronomer selected from among his observations those that seemed the best; this made him liable to bias or inclined to select such data as showed a possibly unreal agreement. It could not seem unreasonable that what agreed in the end was deemed the best. We remember how with Tycho Brahe the cycle of differences of right ascension around the sky resulted in a sum total differing from 360° by only a number of seconds, whereas every separate difference was uncertain by more than half a minute. In the seventeenth century scientists like Huygens and Picard realized that the average of a number of equivalent measurements would be better than one of a couple selected from them, and in the eighteenth century this averaging came more and more into use, all the more so since the concept of chance or probability of errors as a quantitative character had gradually become clearer. The notion of 'laws of chance', already applied by Huygens ('plays of luck'), by Jan de Witt, and by Halley ('mortality tables'), acquired its pure theoretical form a century later in the theory of errors developed by Laplace and Legendre and in Gauss's quadratic-exponential law of errors. It afforded to the computers a method of dealing with a series of observed data according to rules which excluded any arbitrariness.

Thereby a new attitude was brought into being, typical of the nineteenth-century scientist towards his material: it was no longer a mass of data from which he selected what he wanted, but it was the protocol of an examination of nature, a document of facts to which he had to defer. His method of working he found prescribed in the formulas of computation derived in 1804 by Gauss as the 'method of least squares'. By the condition that the sum total of the squares of the remaining errors shall be a minimum, the 'most probable' value of the unknown quantity is found. By making this condition for each of the unknowns of the problem, the most probable values for all of them can be solved directly from the equations. This new method of computing was enthusiastically greeted by the astronomers, who were always struggling with the problem of how to derive the best result from an abundance of data. The 'best' was now defined as the 'most probable'; all hesitation and doubt gave way to solid certainty. Moreover, the deviations themselves that remained in the observed data, called their 'errors', allowed the derivation of a 'mean error' and a 'probable error' as objective indicators of the remaining uncertainty in the results.

This method, afterwards used in all astronomical research, was applied by Encke to the observations of the transits of Venus. These observations gave the moments of ingress and egress. If the relative position of Venus's and the sun's centre in two co-ordinates is known for the middle time and also the velocity of relative displacement, the moments of ingress and egress can be computed as observed from the centre of the earth and, by adding the influence of the relative parallax, for the observing station. The differences between the observed and the computed moments were due not only to the errors of observation but, in addition, to the error in each of the assumed co-ordinates (the velocity was known with sufficient exactness) and to the error in the assumed parallax. Each observation at any place on earth gave a relation between these three unknowns; thus they could be solved by treating all these relations as one body of data. Moreover, Encke could expect a substantially better result than the former computers, because now for a number of stations far to the east or the west, like Hudson Bay, Tahiti, Orenburg, Peking and others, the essentially relevant longitude differences with Europe had been better determined. Thus he got a result for the solar parallax which, after an additional correction in 1835, was given in the form 8.57116″ ±0.0371″.

This form shows how astronomy in these early days of the new method of computation revelled in the delight of being able to compute exact results. According to the probable error after the ±, there is an equal chance that the error of the result is larger, or that it is smaller than 0.037″. It is clear, then, that the third and fourth decimals in the result are not only valueless but even senseless, since the second decimal is already uncertain. Modern scientists would have written more soberly 8.57″ ±0.04″. It will appear that thereby the real uncertainty was still underrated.

Instead of the many different values derived from various combinations of data, we now had one established value derived from their totality; this value has been accepted and used for half a century. With the known radius of the earth, 6,377 km., this solar parallax fixed the distance of the sun—more exactly, half the major axis of the earth's orbit—at 153.3 million km. (95 million miles). It is the astronomical unit of length in which are expressed all the distances and dimensions in the solar system; the sun's radius now is found to be 714,000 km. The sun's mass relative to that of the earth is also connected with it; according to Newton's formula, the third power of the ratio of the orbits of the earth and the moon, i.e. of the ratio of the parallaxes of the moon and the sun, divided by the square of the ratio of their periods of revolution, gives the ratio of the masses, for which 356,000 is found.

These are numbers only, of more or less figures. To understand what they meant for man, who now realized the size of the universe, we have to look into the popular literature that spread scientific interest and knowledge among broad masses of the people in the middle of the century. Here we can read that if a big gun on the sun fired a shot at us we would not perceive it until 25 years later we were struck down by the ball. In a widely read booklet, *An Imaginary Journey through the Universe*, a German writer, Adolph Bernstein, in an amusing way dealt first with the enormous diameter of the sun and then with its volume, computed to be 3,500 billion (3.5×10^{15}) cubic miles (German 'geographical' miles of 7.5 km. or 4.5 statute miles). Then in a chapter entitled 'All Respect for a Cubic Mile', an unsuccessful attempt was made to fill a box of one cubic mile with all the towns and all the people on earth, and equally to count the number of these boxes, which would take millions of years. Against this delight in large figures, F. Kaiser, the restorer of astronomy at Leiden, and a successful popular writer, remarked that the greatness of the universe did not consist in its size but in its order as established by the rule of universal law.

In the first half of the new century the problem of the fixed stars was also taken up. The astronomers of the eighteenth century had failed repeatedly in their attempts to find a yearly parallax of a star. Fraunhofer, however, had now provided such excellent instruments that better results could be expected. Stars presumed to be near us had to be tried first, so that a large parallax might be expected. In his discussion of Bradley's observations, Bessel had met with a fifth-magnitude star in the Swan (61 Cygni) that had an extremely large proper motion of 5.2″ yearly—surely an indication of proximity. In the years 1837–40 he repeatedly measured with his heliometer its distance from two small stars at 8′ and 12′ distance. Since his measurements were very accurate (with the mean error of an evening result only 0.14″), the parallactic circle it yearly described appeared with great clarity. The parallax was found in 1838 to be 0.31″ and in 1840 to be 0.348″. Hence the star was at a distance of 590,000 astronomical units.

At the same time Struve at Dorpat attacked the problem by means of the filar micrometer attached to the 24 cm. Dorpat refractor. He chose the star Vega (γ Lyrae) because of its brilliance, its considerable proper motion (0.35″ yearly) and its situation near the pole of the ecliptic, so that its yearly orbit was an unforeshortened circle. His observations in 1835–38 afforded a parallax of 0.26″. At the same time, Henderson, the director of the Cape Observatory, and his successor Maclear, observed the first-magnitude southern star α Centauri. On many grounds this star was suspected to be very near; it had a large yearly proper motion of 3.7″, and it was a binary describing in a short period a large orbit, which might be supposed to appear large only because of its proximity. The observations were made by means of an ordinary mural circle, thus being far less accurate; but they afforded (1839–40) a large parallax of 0.91″. Later, more exact measurements have reduced this value to 0.76″, still the largest of all stellar parallaxes; so α Centauri remained our nearest neighbour in the world of stars, at a distance of 270,000 astronomical units.

Thus the old problem posed by Copernicus was solved, and the distance of three nearby stars was determined. 'I congratulate you and myself that we have lived . . . to see the great and hitherto impassable barrier to our excursions into the sidereal universe, that barrier against which we have chafed so long and so vainly . . . almost simultaneously overlapped at three different points. It is the greatest and most glorious triumph which practical astronomy has ever witnessed.'[193] Thus spoke John Herschel, as President, to the Royal Astronomical Society when he explained why it had awarded its gold medal to Bessel. And he added that, considering the many false announcements of stellar parallaxes in former years, there might be a certain hesitation as to the results of Struve and Henderson. Bessel's results, however, showing clearly the regular increase and decrease in the measured distances in a yearly period, could not leave the least doubt as to the reality of the parallax found.

It is easily understood that these examples incited the astronomers to follow the same path and to determine parallaxes of other, preferably rapidly moving, stars. Now, however, disappointments supervened. The results of different experienced observers for the same star, though derived from carefully made series of measurements, showed far larger differences than were expected after the agreement within each series. Such was the case, for example, with a star of the seventh magnitude in the Bear, called 1830 of Groombridge's catalogue, which had a proper motion of 7.07″, greater than Bessel's star; its parallax, measured with the Königsberg heliometer, was found to be 0.182″, but with the filar micrometer of the Pulkovo Observatory, to be 0.034″, a fifth of the former figure. For 61 Cygni, Bessel's final result of several years of continued measuring was 0.35″; but Otto Struve at Pulkovo found 0.51″. Obviously, there were large systematic errors present in all these measurements, the origin of which could be only imperfectly guessed: yearly differences in temperature, differences in aspect through atmospheric dispersion, personal errors in pointing stars of different brightness, the comparison stars being usually much fainter than the object star. Thus the determinations of parallax became the severest test of infinite and minute carefulness in the arrangement of measurements and the elimination of sources of error. The heliometer remained the favourite instrument, and it was chiefly David Gill (1843–1914), first in Scotland and afterwards in Cape Town, who, from 1870 on showed how with well-devised handling this instrument could give reliable results. He and his assistant Elkin measured the parallaxes of a number of southern stars, and this work was continued by Elkin at the Yale Observatory at New Haven, Connecticut, in measuring a number of reliable parallaxes of northern stars.

The results obtained for the first dozens of parallaxes allowed some general conclusion to be drawn. Average values had already been derived in the forties by Peters at Pulkovo; for the average parallax of a second-magnitude star he found the small amount of 0.017″, and for fainter stars it must be still smaller. But it became increasingly clear how little such an average meant. Among the brightest stars measured by Elkin, some large parallaxes occur: 0.76″ for α Centauri, 0.38″ for Sirius, 0.32″ for Procyon, 0.24″ for Altair; but also very small values, like 0.028″ for Betelgeuse and 0.008″ for Rigel. The latter, of the same apparent brightness as α Centauri, with the parallax taken as stated, must be about a hundred times farther away, hence 10,000 times more luminous. Conversely, large parallaxes were found for very faint stars that could be singled out from the mass of faint stars as near stars only by their rapid proper motion, such as Lalande No. 21185 of the seventh magnitude, with 0.40″, and Kapteyn's star of the eighth magnitude with 0.32″ parallax. The latter, being at the same distance as Procyon, must have only $\frac{1}{1000}$ of its light power. So there is such a large diversity of luminosity among the stars that one can be millions of times brighter than another.

While the knowledge of distances in the world of the nearest stars thus gradually progressed, the determination of their fundamental unit, expressed by the solar parallax, had entered into a dramatic epoch. The quiet assurance, that through Encke's result we knew it to $\frac{1}{200}$ of its amount, was shaken about the middle of the century by a series of blows. Among the perturbations of the moon's course caused by the sun, there is a term of 125″, called the 'parallactic equation' (already mentioned in Chapter 30), which depends on the ratio of the solar and lunar parallaxes. From this ratio in 1857 and 1863, Hansen, in his theory of the moon, derived a value of 8.92″ for the solar parallax. At the same time, Leverrier found a solar parallax of 8.95″ from the mass of the earth, derived by means of its perturbing action upon Venus and Mars.

Another criticism came from the side of physics. Formerly the velocity of light had always been derived from astronomical data, the constant of aberration fixing the ratio of the velocity of light and of the earth, combined with the velocity of the earth. The former, 20.44″, according to a new determination by Otto Struve and the latter, 30.56 km./sec. from Encke's solar parallax, determined it at 308,000 km./sec. However, methods were now devised to measure the velocity of light by direct physical experiments, first (in 1849) by Fizeau with a toothed wheel, then (in 1862) more accurately by Foucault with a rotating mirror. These afforded a far smaller value, 298,000 km./sec. The

aberration constant could not be in error so much as 0.1″ or $\frac{1}{200}$ of its amount; so Foucault immediately pointed out that the adopted solar parallax must be wrong and must be increased to 8.8″.

Distrust now arose concerning the Venus transits, and attention was being directed towards the old method of directly measuring the parallax of Mars in favourable opposition. Better results than in former centuries might now be expected, since the accuracy of observation had so greatly increased. At the perihelion opposition of 1862 a number of corresponding measurements of the declination of Mars were made at northern and southern observatories; a preliminary discussion afforded 8.96″ and 8.93″, and it was believed that the true value would be near 8.90″. Moreover, a new discussion of the Venus transit of 1769 was undertaken in 1864 by Powalky. Because he had at his disposal more and better longitudes than Encke and interpreted some observations in a different way, he got a different result, viz. 8.83″. So Encke's result had to be dropped entirely and the world of astronomy could without bias face the question of where between 8.80″ and 8.90″ the true solar parallax was to be found.

Astronomers began to prepare for the next transits of Venus which would take place on December 8, 1874 and December 6, 1882. They did not restrict themselves to Halley's method of observing solely the moments of ingress and egress, which was proposed at a time when there was little accuracy in direct measurements. The full benefit of the occasion could be hoped for only by measuring the position of Venus on the solar disc during the entire transit. Expeditions were sent to all parts of the world. Ten German expeditions were distributed over the continents, all provided with similar heliometers, considered the best instruments for this purpose. Moreover, photographic telescopes were used especially by English and American expeditions, so that the position of Venus upon the sun could be found by measuring the plates later. An enormous amount of work was expended on the reduction; the results of the German expeditions were published twenty years later in five bulky volumes, and the work of the English and Americans was hardly less. The results were rather disappointing: the English contact results were 8.76″ (Airy), 8.88″ (Stone), 8.81″ (Tupman); from the American photographs Todd derived 8.88″; from the German heliometer measurements of 1874, Auwers found 8.88″ ±0.04″. Not only did the values from different sources diverge but the mean errors added showed a lack of inner agreement too. It can be understood, indeed, that in measuring positions of an object against the luminous background of the sun, with the light rays traversing the heated and vibrating air layers, we are in a worse condition than when using the same instrument at night in observing the stars.

When in 1877 Mars again had a perihelion opposition, David Gill took his heliometer from Scotland to Ascension Island, near the equator, to derive the parallax by measuring the position of Mars between the stars, in the east in the evening, in the west in the morning. Whereas collaboration of northern and southern observers was needed for the north–south effect of the parallax, the east–west effect could be measured by one observer, so that personal differences had far less effect. The stars could be made to coincide with the centre of the planet's disc very accurately, and the result, 8.78″, was considered to be very reliable.

Earlier the idea had already been expressed—first by Galle in 1872—that greater accuracy and especially greater freedom from systematic errors were to be expected if, instead of Mars, a starlike small planet was used. Objects must then be chosen whose perihelion came nearest to the sun and the earth. These minimum distances to the earth for the chosen planets Iris, Victoria, and Sappho are 0.83 or 0.84 astronomical units, which are certainly large compared with 0.37, the distance of Mars, so that the parallaxes to be measured were far smaller. It was expected, however, that what was lost in this respect would be gained by the absence of systematic errors. Comparison stars of equal magnitude were carefully selected and determined; a broad programme of meridian and heliometer measurements of the positions of the planets relative to the stars was drawn up. The observing campaigns in 1888 and 1889, in which a number of northern and southern observatories took part, came up to expectation; the result of a careful investigation and discussion of errors by Gill was 8.802″.

The confidence that we were now on the right track and that the solar parallax was near 8.80″ could not be shaken by the deviating values from the Venus transits, where so many points of distrust were present. It was strengthened by the physical result; the velocity of light was determined by Michelson and Newcomb, using Foucault's method, to be 299,860 ±60 km., certain to $\frac{1}{5000}$ of its value; combined with an aberration constant of 20.47″ ±0.02″, it afforded a solar parallax of 8.80″, with an uncertainty of no more than 0.01″.

Better was still to come. In 1898 Gustav Witt at Berlin discovered a small planet, No. 433, later called Eros, the orbit of which is not situated between Mars and Jupiter but, in its perihelion, comes within the orbit of Mars, near the earth. Only in such a near approach was it possible to discover the tiny body; computation showed that in the opposition 1900–1901 it would approach the earth to 0.27 astronomical unit, and in the opposition 1930–31 still nearer, to 0.17. By a piece of good luck, the astronomers were presented here with a celestial body more likely than any other to procure for them the coveted solar parallax by a value of the highest precision. The opposition of 1900–1901, in a complete discussion of all the data by Hinks, gave 8.807″ ±0.003″ from the photographic and 8.806 ±0.004″ from the visual measurements.

The twentieth century added new results of similar high quality. Stars in the ecliptic show yearly periodic variations in radial velocity because the earth is alternately approaching them and moving away from them. Hough in 1912, by means of spectrographic measurements, determined the orbital velocity of the earth, from which a solar parallax of 8.802″ was found. From the strong perturbations of Eros caused by the earth, Noteboom in 1921 derived the mass of the earth, which provided a parallax of 8.799″. In 1924 Spencer Jones at Cape Town derived for the parallactic inequality of the moon a value of 125.20″, from which a solar parallax of 8.805″ followed. Then De Sitter at Leiden made an exhaustive discussion of all the mutually related astronomical constants, from which the solar parallax came out at 8.803″ ±0.001″. This does not mean that the third decimal was now secured. Spencer Jones in 1929 made a new discussion of a still more complete mass of star occultations by the moon, which resulted in a parallactic inequality of 125.02″ ±0.033″ and a solar parallax of 8.796″ ±0.002″.

For the even more auspicious opposition of Eros in 1930–31 a great campaign of meridian and micrometer observations and photographic plates was started, in which nearly forty northern and southern observatories took part with their best equipment. The reduction took ten years, and in 1942, during the war, Spencer Jones, then at Greenwich, published the result. It was 8.790″ ±0.001″. Considering the extent of the collaboration, the large amount of observational data, the perfection of the methods used, the watchful elimination of sources of error, the careful discussion, we may say that another determination of the same or of a higher quality is not to be expected in the near future.

This does not mean that now all is well and that astronomers in this respect are free from worry. This latest best value again deviates more from the former best values than could be expected from their mean errors. There must still be some hidden sources of systematic error. Eros is not a well-formed globe; it has been suspected of being either an irregular, elongated, or a double body; to have a result free from such irregularities, photographs at northern and southern stations should have been simultaneous. So the indication 0.001″ should not be taken too literally. Yet how far was our knowledge advanced!

The history of the determination of the solar parallax is one of the most striking examples of what has been called the 'struggle for the next decimal'. One decimal more means enormous progress, the diminution of the possible error to one-tenth of its former amount. At the end of the seventeenth century it was known that the solar parallax was about 9″, with some seconds' uncertainty, say $\frac{1}{3}$ of its amount. After the Venus expeditions of the eighteenth century its value was held to be between 8.5″ and 9″, with an uncertainty of some tenths, $\frac{1}{30}$ of its amount. Toward the end of the nineteenth century, after some hesitation, its value could be said to be near 8.80″, with some hundredths of a second uncertainty only, i.e. $\frac{1}{300}$ of its amount. And now we may with some confidence add a third decimal, with the certainty that it is accurate to some thousandths of a second, $\frac{1}{3000}$ of its amount.

From the solar parallax and the equatorial radius of the earth (6,378 km.) the astronomical unit is now found to be 149.7 million km., 92 million miles, $\frac{1}{37}$ less than Encke's value. It is the unit not only for the distances but also for the dimensions of the celestial bodies. To find them, their apparent diameters must be accurately measured. This became possible in the nineteenth century by means of instruments from Munich. Since the diffraction phenomena of light rays passing substantial wires prevent the use of filar micrometers, the heliometer is the approved instrument for measuring diameters, by bringing the two images of a luminous disc into contact. So Bessel was the first to derive usable results; after him, Johnson and Main at Oxford, Kaiser at Leiden (with a more simple double-image micrometer, carefully handled), and Hartwig at Bamberg worked along the same lines. The semidiameter of Mercury was found to be 2,380 km. (by Kaiser), of Venus and Mars 6,372 and 3,370 km. (by Hartwig); so with the earth they form the inner group of smaller planets. For Jupiter and Saturn, Kaiser found an equatorial radius of 70,550 and 59,310 km., 11 and 9 times larger than the earth; the polar diameter is considerably smaller, the flattening being $\frac{1}{17\cdot1}$ for Jupiter, $\frac{1}{9\cdot2}$ for Saturn. The semidiameter of the sun, for which a special mode of measurement was needed, amounts to 696,400 km., 109.2 times larger than the earth.

Parallel to these developments went the connected problem of the stellar distances. Towards the end of the nineteenth century, when photography was used more and more in astronomy, especially for the determination of positions, the first attempts were also made to use it for stellar parallaxes, but in the beginning with poor results. It appeared that systematic errors vitiated the results more than they did in the case of visual measurements. Diverse causes could soon be traced in the photographic or observational processes. The object star for which the parallaxes were desired was usually much brighter than the comparison stars, which mostly appeared on the plate as faint little discs. If, for some reason, part of the light fell at a spot beside its right central place, its impression for the faint star was too weak to leave any trace, but the image of the bright star was somewhat disturbed or even displaced. Such a deviation might be caused by refraction when during the

exposure the field came nearer to the horizon. The observer in 'following' with the solidly connected visual telescope kept the star exactly at the cross-wire; but, because of atmospheric dispersion, the photographic image gradually deviated from the visual image. This explanation of the systematic errors pointed to the means of avoiding them: by removing the difference in brightness and taking the plates when the altitude of the stars did not change, i.e. in the meridian. The example was set by Frank Schlesinger, who in 1903 began to take parallax plates with the 40 inch refractor of the Yerkes observatory, continuing it, from 1905 on, with a photographic refractor at the Allegheny Observatory. The brightness was equalized by having a screen with adjustable free sectors rapidly rotating before the place where the image of the bright object was formed, so that it was illuminated for a few moments only. The sectors were opened to such a width that this image corresponded to the average size of the comparison stars. Since, moreover, the plates were taken symmetrically about the meridian, between one hour before and one hour after the meridian passage, and the long focus of the instrument gave a large scale, the results at once proved to be highly accurate and reliable. The accidental errors of his parallaxes were no more than 0.01″, and they decreased in the course of the work.

This example was soon followed by other observatories. A regular co-operation on stellar parallaxes ensued with six other observatories, Greenwich, Cape Town, and four American, all of which had long-focus refractors or other superior instruments. All were working with the same standard of accuracy; when the results of different observatories for the same star were compared, nothing was found of the former large differences. The differences that were now at most some few hundredths of a second, testified to the new high standard of accuracy. The number of reliable parallaxes increased in the first decades of the twentieth century from tens to hundreds and at last to thousands; Schlesinger's *General Catalogue* of 1924 contained 1,870 stars; and this number was steadily increasing. All stars easily visible to the naked eye and a number of telescopic stars with large proper motions now have their parallaxes measured. The knowledge which earlier had been limited to a few stars has now grown out into a survey of the thousand stars forming our nearest universe. Their distances can now be computed; the unit generally used is the 'parsec', the distance for which the parallax is one second, whereas in popular books the light-year, nearly $\frac{1}{3}$ parsec, is often found. Of the stars surrounding us at not too great a distance, the relative situation can now be represented in a spatial model—of course, not at greater distances, because they are too uncertain. If a parallax is determined at 0.01″ with an uncertainty of 0.01″ also, it means that it is probably between 0 and 0.02″, so that its distance probably is beyond 50 parsecs—how much, we do not know. Negative values for parallaxes also occur; it means either that, because of accidental errors, a small positive parallax has been depressed below zero or that the parallax of the object star is smaller than the small parallaxes of the comparison stars. Though such negative parallaxes have no meaning for an individual distance, they should not be omitted in statistical researches where averages are formed.

The fundamental problem of the parallaxes and distances of the fixed stars has thus been solved. Originally aiming at a demonstration of the truth of the heliocentric system, it was also inspired by the desire to acquire knowledge of distances in the world of stars. When a few dozen stellar parallaxes had been determined, they could already be used for a statistical treatment of the nearest surrounding stars. With the thousands of parallaxes now at our disposal, a deeper insight into the structure and character of the sidereal world may be expected.

CHAPTER 34

CELESTIAL MECHANICS

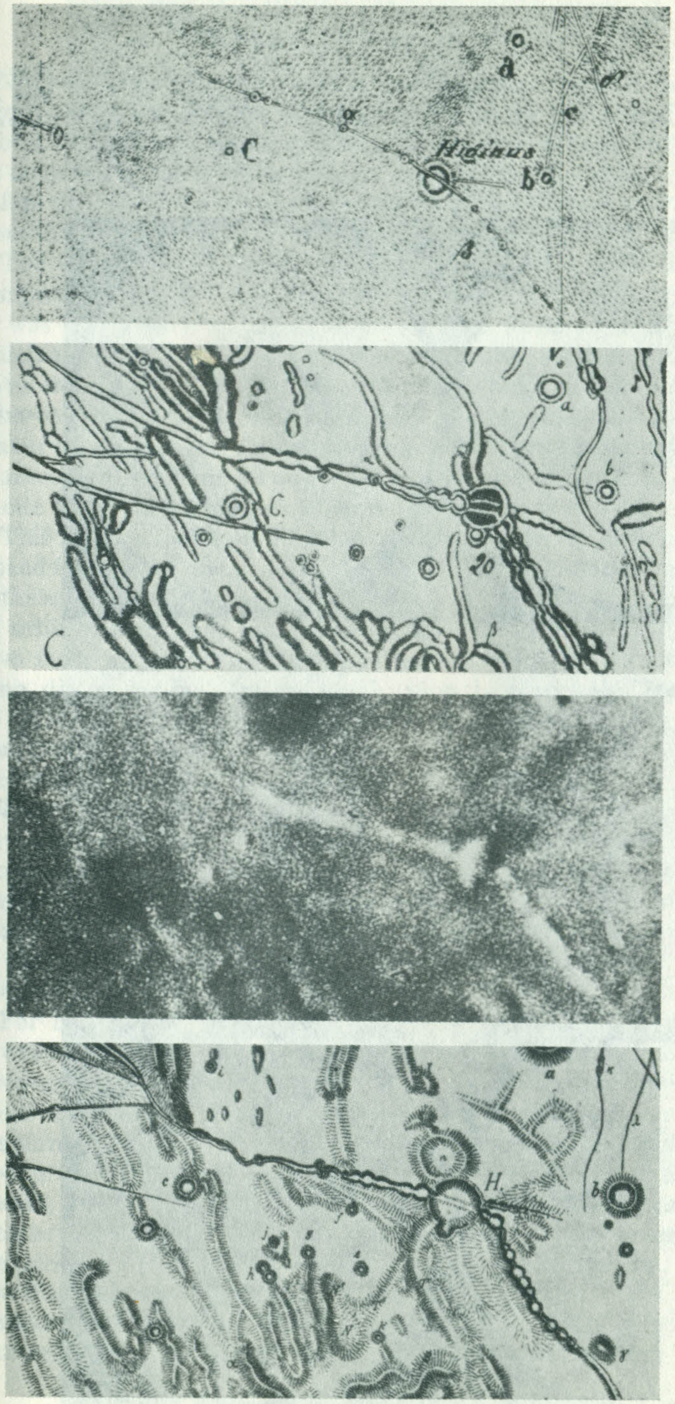
With the title of his work, *Mécanique Céleste* ('Celestial Mechanics'), Laplace had set forth a programme of theoretical astronomy. Mechanics is the science of forces and motions; through Newton's law of gravitation the forces were known by their dependence on the positions; then came the task, through integration of the differential equations expressing this dependence, of computing the general characteristics of the orbits of the bodies. The special dimensions of the orbits and the constants determining them had to be derived from the observations.

Astronomy now became the science of patient computation. Besides the practice of untiring observation and measurement, indefatigable computation now took its place as equal partner in practical astronomy. It consisted of two kinds of work: first, the theoretical solving of the mathematical problem, resulting in a body of formulas, which taxed the mind and ingenuity of the most brilliant mathematicians; and then the practical elaboration of this theory in numerical calculation, laid down in endless rows and tables of figures. Astronomy at this time was the only science of nature in which exact practical computation was an important activity; computing was the daily task, so that in astronomy techniques and methods of computing were devised and improved through painstaking practice. Besides the astronomer who observed the celestial bodies in the telescope was a man of equal merit, the astronomer at his desk, who with his pen followed the celestial bodies in their course; they were, of course, often embodied in one person.

Soon there was a lot of work to be done first in the field of the simple two-body problem of motion under the attraction of the sun alone. Comets appeared as faint, small nebulae, but were nevertheless assiduously traced by diligent comet-hunters; their orbits across the planetary system had to be computed. In the eighteenth century they also had been computed, but with difficulty. After the first primitive indications by Newton various mathematicians had occupied themselves with the problem, and Laplace had given formulas for the computation of a parabola through successive approximations; but the procedure was cumbersome and unsatisfactory. The practice of the astronomers who had to deal with them led to the solution of the problem before the turn of the century. In 1797 Wilhelm Olbers (1758–1840) published his treatise *Ueber die leichteste und bequemste Methode die Bahn eines Cometen aus einigen Beobachtungen zu berechnen* ('On the Easiest and Simplest Method to Compute the Orbit of a Comet from several Observations'). Olbers, a physician at Bremen with a busy practice was at the same time a practical astronomer who at night observed the stars and the comets and computed their orbits. Though an amateur, he was highly esteemed among the astronomers. He had devised for himself a method of computing orbits and practised this for many years, not suspecting, in the simplicity of the times, that it was anything special, until his friends persuaded him that it was worth printing. During the entire nineteenth century his method was used by successive generations of astronomers, young and old; some of them succeeded in devising technical improvements in minor points only.

But there was more to it than the comets and their parabolic orbits. In the structure of the planetary system the gap between Mars and Jupiter had often caught attention. Kepler had employed it to insert his tetrahedron. In the eighteenth-century notions about the origin of the planetary system such a gap did not fit. This showed most clearly when the succession of distances was reduced to the mathematical form of a series, the so-called 'law of Titius' or of Bode—Titius had published it in an inconspicuous footnote of a translated book, and Bode had dug it up and made it public in 1772. It renders the size of the orbits of the planets from Mercury to Uranus by the numbers 4; $4+3=7$; $4+2\times3=10$; $4+4\times3=16$; etc., up to $4+64\times3=196$; but $4+8\times3=28$ had to be omitted: there was no planet known at that distance.

So attention was directed to it, and the opinion was expressed that it should be tracked. But it was found by chance. On the first day of the new century, January 1, 1801, Piazzi at Palermo found among the stars he was observing a faint one of the seventh magnitude that moved. He first called it a starlike comet, without nebulosity; but soon it was realized that it must be a new planet, with a period of revolution of about four years; he afterwards named it 'Ceres', the tutelary deity of Sicily. The discovery was made known by letters to foreign astronomers; but communication in those days was slow because Italy was full of warring armies, and before others could observe it, the planet had been overtaken by the sun. Piazzi had made a number of observations, but he withheld them in order to publish the orbit computed from them at the same time. He worried over this task without much success, because no regular method existed of computing an orbit from an arc as small as a few degrees. When the planet was sought at the computed place, after the period of invisibility, it was not there; it was lost and could not be



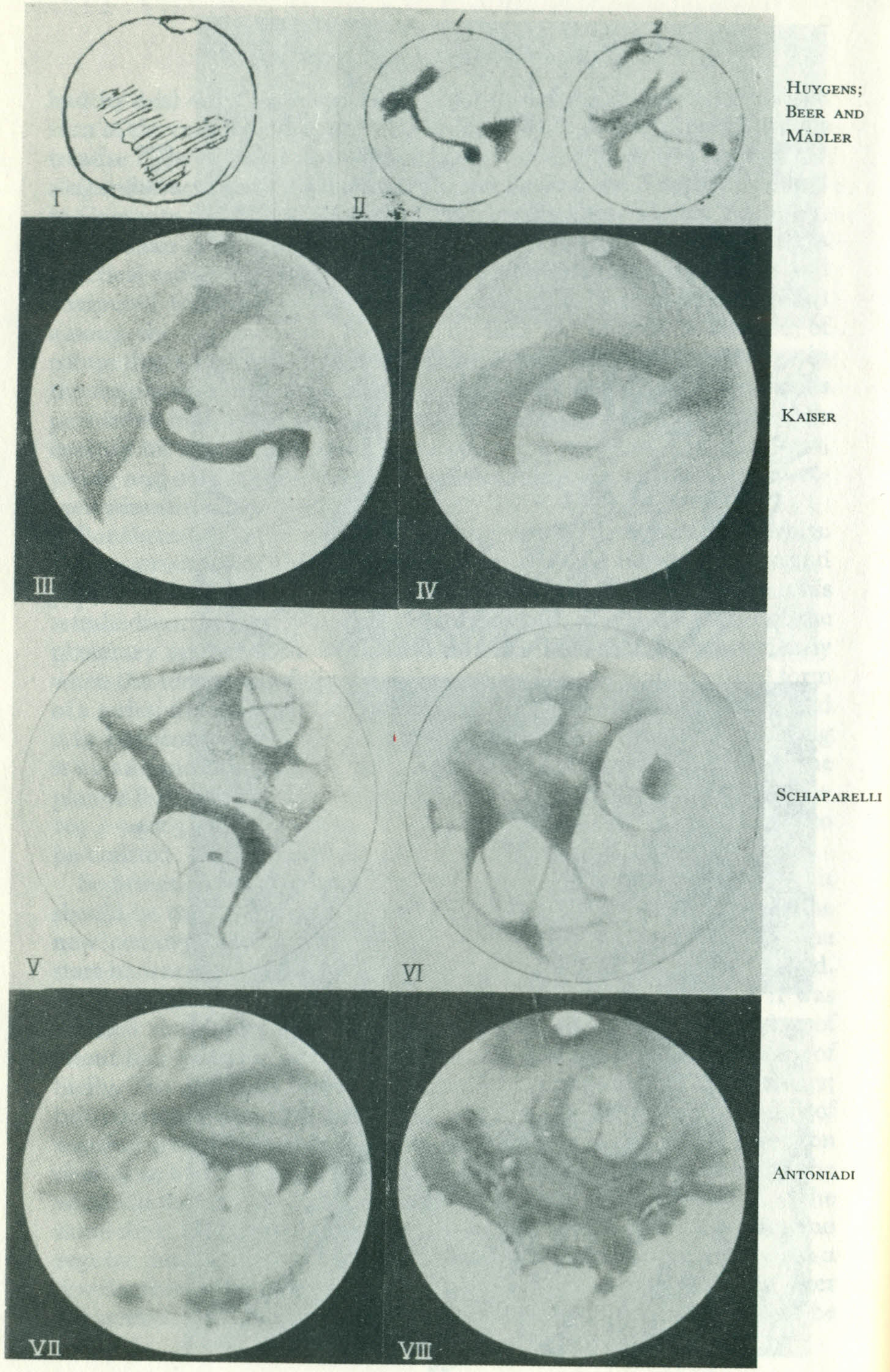
BEER AND MÄDLER

JUL SCHMIDT

PARIS

PH. FAUTH

11. The Hyginus Crater and its surroundings (pp. 372–4)



12. Drawings of Mars (p. 376)

recovered. Then the young mathematical genius, Carl Friedrich Gauss (1777–1865), who happened to be working on the problem of planetary orbits, turned his attention to the lost planet. From Piazzi's observations he determined the orbit by his new method, and the planet was found before the end of the year at the place predicted by him, quite different from the former computations.

The success of a skilfully devised theory gave Gauss's method of computing orbits its prominence during the entire nineteenth century. It was expounded at length in his *Theoria motus corporum caelestium*, which appeared in 1809, in form and method the most perfect textbook on motion in unperturbed orbits. Whereas Olber's method supposed the orbit to be a parabola, determined by five elements, Gauss's method makes no supposition about the form and character of the orbit; the character of the conic section expressed by the eccentricity comes out as one of the six computed elements.

This method soon found further application. In the region where he looked for Ceres, Olbers, in April, 1802, discovered another moving star of the seventh magnitude, a second small planet, called 'Pallas', having an orbit of nearly the same size but strongly inclined to the ecliptic. Two more planets were added in the next years—in 1804 the planet Juno, discovered by Harding; and in 1807 the planet Vesta, discovered by Olbers. Gauss's method in each case, after a short period of observation, produced the elements; by means of these elements the new planets were secured and their course computed in advance.

Thus the gap was filled, but in quite an unexpected way: by four tiny planets instead of by one large one. They were more than a thousand times fainter than Mars or Jupiter, looked exactly like stars, and represented another type of celestial body. They were called 'planetoids' or 'asteroids' or simply 'minor planets'. They have orbits of nearly equal size, 2.3–2.8 times the earth's orbit, and periods of revolution between 3.6 and 4.6 years.

For nearly forty years their number remained four. Astronomers, however, were convinced that there must be more. Therefore, to discover them in an easier way, the Berlin Academy organized the construction of star maps covering the zodiacal zone and containing all the stars down to the ninth magnitude. This was made possible by the co-operation of about twenty astronomers, each taking a field of 15° square, inserting the stars of the catalogues according to their co-ordinates and subsequently the bulk of the other stars by eye-estimates at the telescope. Thus furthered, in 1845 there ensued a regular stream of new discoveries which increased steadily. In 1852 the number of minor planets had risen to 20; in 1870 it reached 110. Of course, they were increasingly smaller bodies, and their brightness on discovery decreased to the ninth magnitude, then lower and lower, to the tenth, eleventh, and twelfth magnitudes. The Berlin star maps were now no longer of any avail; the chief discoverers, such as C. H. F. Peters at Clinton and Palisa at Vienna, had to construct new maps to include the fainter stars, and these maps were published. Then Palisa simplified the work by using photographs taken with long exposures by Max Wolf at Heidelberg, with a wide-angle objective, and published reproductions of them. Thus he was able himself to add nearly one hundred new asteroids to the list.

A new and still more efficient photographic method was introduced about 1891 by Max Wolf. When he photographed a large star field with an exposure of several hours, the camera exactly following the stars, the motions of the minor planets during this time produced small streaks, which, merely by visual examination, could at once be distinguished among the thousands of pointlike stellar images. These trails when measured afterwards, gave good positions of the planets for the central moment of exposure. In this way Wolf rapidly increased the number of minor planets; in 1900 it had risen to 450, and he himself discovered (up to 1927) a good 500. Other observatories joined him with similar or better instruments. Through a variation of the method, by having the telescope follow the estimated course of the planets so that they produced small points while the stars drew streaks, still fainter objects were hunted up. In this way they increased still more rapidly; in 1938 their number had reached 1,500. Whereas the first four discovered were substantial bodies of 500 or 800 km. in diameter, the last hundreds tracked had diameters of only 50, 40, or 30 km.

This unexpected increase confronted observers and computers, as well as theoretical astronomy at large, with increasing difficulties. From the very first, the computation of orbits meant a lot of work. The computation of an elliptical orbit from three observations—roughly some days after the discovery, but more precisely from observations a month later—could be performed rapidly enough by Gauss's method. But in order to predict the positions in later years with sufficient certainty, a large number of observations, extending over the entire period of visibility, was first needed, and then the computation of the most probable elements from all these observations had to follow. Moreover, the work was never finished, because every succeeding year brought a new opposition, with new observations. If however this work were neglected, the predicted result would be more and more in error, so that the planets could not be found again among all the little stars, and if seen by chance later could not be identified; general confusion would result. Now and again it was said that we should disregard all the small fry and drop them; but where is the limit? It was the same here as

with the entire technical development of the nineteenth century; people were dragged along in endless labour which allowed no slackening. In the early years, about the middle of the century, the enthusiasm and perseverance of young scientists and the charm of astronomical computing sufficed to satisfy the needs. When the numbers increased alarmingly, when, moreover, the ever smaller bodies lost their salient individuality although names were assigned to them, this supply of work began to fall short. Now the work was concentrated more and more in computing offices, where official duty and routine, organization and mechanized computing methods, combined to cope with the ever increasing flood, though with a decimal less in accuracy. The Berlin Computing Office afterwards transferred to Dahlem and christened 'Copernicus Institute', among other duties took over the major part of this crowd of celestial Lilliputians.

If only the regular orbit computation had been the only task! All these little bodies, however, were subjected to the attraction not only of the sun but also of the major planets. So their orbits were continually changed through perturbations. These perturbations are even larger than those of the older planets, first because the minor planets come nearer than any other to Jupiter, the great perturber of the solar system, and also because their orbits often have great eccentricities and inclinations. Yet the perturbations must be computed. If we should neglect them, the computed orbit within a few years would be far too incorrect; the errors of the computed positions would be so large as to render the entire computation useless. Nor could we think of developing the perturbations, once and for all, in a general algebraic way, as Laplace had done for the seven major planets. Whereas in the latter case, where the high order terms rapidly decrease, the theory of one planet may already demand exertion over dozens of years, the number of terms for a minor planet would be endless. Hansen in later years, in 1856, indicated an approximate method and applied it to some of the first discovered asteroids. However, all the astronomers who first encountered the problem—Gauss, Encke, Olbers, Bessel—agreed that there was only one practical way to take account of the perturbations. This was the same method used since Clairaut for the comets: to follow the planet in its course continually, from place to place, from week to week or month to month, computing for every moment the perturbing forces, motions and displacements, and to see where they next brought it. It is a never-ending work, each year requiring as much time as did the preceding one. Since, however, there was no other way, this method of 'special perturbations' has been built up, mainly by Encke, into a handy, fixed and simple routine scheme that has been used by all good computers of the first dozens of planetoids, to give a solid basis to the derivation of orbits. Finally, however, when the number grew into hundreds, even the greatest ardour and patience could not master the work. The computing offices had now to devise gross methods for rapid and approximate computation of perturbation terms for a number of similar orbits. In the judicious balancing of the opposing demands of feasible work and attainable accuracy, no less ingenuity was needed here than formerly for the mathematical problems themselves. Yet astronomers always have to face the question of whether it pays to derive orbits for all those small and smaller lumps of rock of some tens of kilometres straying through space.

The reward of all these labours took the form of new discoveries, new insight and new interesting problems. Kirkwood, in 1857, when the number of minor planets was hardly sufficient, and again in 1866, pointed out that the periods of revolution were not regularly distributed about their average of 4.7 years. There were gaps at 5·93 and 3·95 years, exactly ½ and ⅓ of Jupiter's period. When the number of planetoids increased, these gaps stood out still more clearly, and less definite gaps also appeared at ⅖ and ⅗ of Jupiter's period. It must be an effect of Jupiter's attraction, which obviously does not tolerate these simple commensurabilities and drives the small body away from this situation. As an unexpected new phenomenon, it confronted celestial mechanics with new problems.

In 1906 four new planetoids were discovered shortly after one another —some more were added later on—with periods of revolution of 12 years, exactly identical with Jupiter's. They travelled in a similar orbit at a distance of five astronomical units from the sun, keeping a distance of nearly 60° in longitude from Jupiter, some preceding, some following; in this way they formed an equilateral triangle with Jupiter and the sun. This had already been anticipated by celestial mechanics; Lagrange had theoretically designated these triangular points as points of equilibrium, where small objects under the combined attraction of the sun and Jupiter are relatively at rest. Further analysis showed that small bodies can oscillate about these points in so-called 'librations', always accompanying Jupiter at nearly 60° distance. These remarkable fellow-travellers were named Achilles, Hector, Patroklus, Nestor and other heroes of the Trojan War; the entire group was called the 'Trojan planets'.

Remarkable in other ways and more important practically are such minor planets as in their perihelion pass to within the orbit of Mars and come near the earth. One of them, Eros, we have already met as an excellent aid in determining the solar parallax. Its oscillations of brightness, not even entirely regular, indicated that it was not a perfect globe but a more or less irregular block, rotating irregularly. Surely,

large numbers of such misshapen boulders are swarming through space; but they are visible to us only when they pass the earth at a short distance. In 1932 Delporte at Brussels discovered an object on his plates that could be followed in its rapid motion for some days and that allowed an uncertain orbit to be derived; it had approached the earth to a distance of 0.11 astronomical units, i.e. 44 times the distance to the moon. Shortly afterwards Reinmuth at Heidelberg found on his plate a trail produced by a planetoid passing the earth at 0.06 distance. The extreme in close approaches was an object which in 1937, on October 30th, dashed past the earth at a distance of 0.004, i.e. 1½ times the distance to the moon. Compared to the ordinary minor planets, such bodies are of an even lesser order, measured by single miles, and only visible at such very short distances. The astronomer is faced with the question, down to what size do these boulders have to be treated as planets for which orbits are computed? For mankind they pose the more important question of what will happen if they pass us at far shorter distances or even hit the earth. Their mass is so small that such collisions cannot perceptibly disturb the motion of the earth or moon. However, if they happened to strike a continent, the transformation of arrested energy of motion into heat, making the body explode into gas at thousands of degrees of temperature, would cause catastrophic devastation.

Such collisions, though on a far smaller scale, have in fact occurred. Ancient reports sometimes spoke of fiery stones that fell from the heavens; but in the eighteenth century they were relegated to the realms of fables. When meteorites fell in the French province of Gascogne in 1790, the Paris Academy refused to accept a protocol on this phenomenon, in order not to encourage a 'superstition unworthy of these enlightened times'. The German physicist Chladni, however, demonstrated in a well-documented study that a number of real cases had occurred; and shortly thereafter new instances confirmed his view, one meteorite even setting fire to a farm. Small meteorites are now collected and preserved in museums; bigger ones have been found in far countries, often worshipped as legendary objects. A great crater in Arizona is ascribed to the impact of a gigantic meteorite in prehistoric times. On July 30, 1908, a meteorite struck and devastated a region in Siberia fortunately uninhabited. Though optical atmospheric phenomena were widely perceived all over the earth, their cause remained unknown until, ten years later, a scientific expedition brought the occurrence to light.

From this digression by way of planets to meteorites, we now return to the orbits. Gauss's method had originated from the problem of the planetary orbits. It was applicable of course to any ellipse, even to the parabola, where the computation would produce an eccentricity of one; so it was also used for comets. In the first half of the nineteenth century comets repeatedly turned up, small and faint nebulosities, for some of which, as became immediately apparent, a parabola did not fit. Here, by means of Gauss's method, strongly elongated ellipses of small dimensions and short periods of revolution could be deduced. The smallest orbit was shown by a comet discovered by Pons at Marseilles in 1818, which, however, contrary to custom, has always been named Encke's comet after the computer who during the following years took care of it by computing its course. Its period was 3.3 years only, and it turned out that at former appearances—in 1786, 1795 and 1806—it had been observed but not recognized as a periodical appearance. After Halley, Encke was the second man to predict the return of a comet, and it has since been computed, predicted and observed at every return. Its distance from the sun varies between 0.34 and 4.08 astronomical units, so that it does not reach as far as Jupiter's orbit. To compute the perturbations, there was no way but to follow the comet continuously along its orbit by means of a careful computation of special perturbations. As a reward, this procured an accurate derivation of the mass of Mercury, because in 1835 the comet passed it at close quarters. A second result was the curious phenomenon that the orbit gradually became smaller and the period shorter. Encke attributed this to a resisting medium filling the space between the planets; such a resistance can reveal itself only in such extensive and tenuous objects as comets. Other comets, however, did not show it; and later computers, continuing Encke's work on this comet (first Backlund and then Von Asten), were inclined to ascribe the resistance to separate encounters, in special parts of its orbit, with other comets crossing its track.

Shortly after the first short-period comet, another was discovered in 1826 by Biela; it had been perceived in 1772 and in 1805, and had a period of 6¾ years; we shall return to it later. Gradually, more comets with elliptical orbits were discovered. Their number, however, remained restricted: a dozen with periods of about 5 or 6 years, some few with 13 and 33 years. Halley's comet returned twice, in 1835 and in 1910, and was of course carefully observed and computed. A small comet discovered by Olbers in 1815 was found to have a similar period of 72 years and duly returned in 1887. That cometary periods occur in such groups seemed to be connected with the planets; elliptical orbits of 6, of 13, of 33, of 75 years have their aphelia at the distance of Jupiter, Saturn, Uranus, Neptune. That a comet could acquire a short five-year period through the attraction of Jupiter had already been stated by Laplace; if in its parabolic orbit it comes very near to Jupiter, the strong attraction of this powerful planet can throw it into an entirely different track and keep it captive within the solar system. It was supposed for the other groups that a close encounter here with Saturn or with Uranus or Neptune had produced the orbits of 13, 33 and 75 years. Such orbits will not always be final; if it is not in the meantime displaced by other perturbations, the comet must return to its meeting place with the planet and can suffer a new large change in orbit. Such has been the case with a comet observed in 1770, for which Lexell at St Petersburg derived an elliptical orbit of 5.6 years. It has never been seen again. According to Lexell's computation, confirmed by Burckhardt, the comet had acquired this orbit in 1767, shortly before its appearance, through a near approach to Jupiter; after two revolutions it came back to the same spot in 1779 when Jupiter was there again. And it was thrown into a rounder ellipse that forever kept it out of our sight. Other analogous cases occurred later on.

The essential domain of celestial mechanics did not lie in all these computations of orbits; the perturbations of the major planets and the moon were its most important object. After Laplace had brought the theoretical work of the eighteenth century to a close, the task of deepening the foundations of theory and of bringing the developments to a higher stage of accuracy was left to the nineteenth century. The former task was taken up by such ingenious mathematicians as Jacobi (about 1830) in Königsberg, and Henri Poincaré (about 1880) in France. The exact computation of the perturbations of the planets was the object of many studies on the part of able theorists in the first half of the nineteenth century, such as Cauchy, Bessel and Hansen. The most thorough and complete treatment of these perturbations was the work of Leverrier.

Urbain Jean Leverrier (1811–77) gained fame mainly by his theoretical discovery of the planet Neptune, solely from the perturbations it produced in the motion of Uranus. That the motion of Uranus presented irregularities not accounted for by the attraction of the other planets was first perceived by Alexis Bouvard, a farmer's boy from the Alps, who had come to Paris to study science and who, owing to his talent for computation, became an invaluable aide to Laplace. He computed tables for the major planets, but his tables of Uranus, derived from regular observations in the 40 years after its discovery, could not represent the earlier scattered data when Uranus had been observed as a star. In publishing his tables in 1821, Laplace spoke of 'some extraneous and unknown influence which has acted upon the planet'. When in the following years Uranus again began to deviate more and more from the tables, the opinion became widespread among astronomers that there must be an unknown planet disturbing the motion of Uranus. In the late 'thirties Bessel made one of his pupils, F. W. Flemming, attempt a computation of this unknown planet from Uranus's deviations; but Flemming died when the work had just started. In 1842–43 J. C. Adams (1819–92), a gifted student of mathematics at Cambridge, began to tackle the problem; in September, 1845, he was able to communicate to Airy, the Astronomer Royal, and to Challis, the director of Cambridge Observatory, his results on the orbit and position of the guilty planet. Because both astronomers lacked confidence in the research, and as a consequence of his own modesty, the result was not published, and no attempt was made to discover the planet. In the meantime, urged by Arago, Leverrier set to work on the problem. First he made a thorough revision of the theory of Uranus and published it in November 1845. In June, 1846, he added his results on the orbit of the supposed unknown planet and its position in the sky. When Airy and Challis saw his result, which agreed closely with that of Adams, Challis began to make a search in July and August by registering at different days all the stars in a field about the assigned place, to see whether one among them changed its position. Diverted by other work, he then failed to reduce and compare his observations; otherwise he would most certainly have discovered the planet, for it was among the registered stars. In the meantime, Leverrier, impatient that no observer gave heed to his results, in a letter to Galle, astronomer at the Berlin Observatory, asked him to examine with the large refractor the stars about the place indicated, to see whether one showed a disc. A short time before, the Berlin Academy map of this very region had been received at the Berlin Observatory. On receipt of Leverrier's letter, on September 23rd, the map was at once compared with the sky, and the planet was immediately found as a foreign star of the eighth magnitude not present on the map. It received the name of Neptune.

This course of events made a deep impression on the world of scientists, but no less on the world of educated laymen. From all countries honours were showered upon Leverrier, and the discovery at a desk of a body never seen was the ruling topic for a long time. It was in this mid-century that science came to dominate the world concepts of the middle class in western Europe, and in a spiritual struggle gradually superseded the traditional biblical ideas. A number of popular books on science, by spreading knowledge, furthered the *Aufklärung* ('enlightenment'); welcomed enthusiastically among intellectuals and laymen, they served as an aid in the fight against antiquated political and social ideas and institutions. In such an environment this unexpected demonstration of the power of science and the certainty of its predictions came like a brilliant ray of light to strengthen the fight against darkness. Surely the astronomers were right who

pointed out that any of the hundreds of computed perturbations used in the planetary tables, whose exactness was confirmed by subsequent observation, was as strong a demonstration, silently repeated every day, of the truth of science. Yet the brilliance of this discovery, happening in the intellectual atmosphere of the years that led to the stormy 1848, made it an important event in the history of science.

To the astronomers it at once brought new problems and new worries. The difficulty in the research had been, first, that it was of course an inverse problem—to derive not the effect from the cause but the cause from the effect; the derivation of the perturbing body from the perturbations was quite a new adventure. The second difficulty was the uncertainty of the data, owing to the short period of regular observation of Uranus. So as not to make the solution practically impossible by the larger number of unknown elements of the unknown orbit, both Adams and Leverrier had assumed, for the mean distance to the sun, 38 astronomical units, according to Titius's law; the corresponding period was 217 years. Both found a large eccentricity of about 0.10, which, for about 1820, made its distance to the sun 34 astronomical units. They could represent in this way the older observations of Uranus before its discovery, that of Flamsteed in 1690 excepted. After Neptune had been discovered and had been observed for some months, Adams (and also Walker in Washington) found that its orbit was much different; it was smaller, with a mean distance to the sun of 30 units and a period of 164 years, and with a very small eccentricity. The real Neptune, therefore, had occupied quite different places in space and had exerted forces upon Uranus widely different from those of the theoretically computed planet. Hence the Neptune discovered by Galle, said the American computers Peirce and Walker, was another body than the planet computed, and its discovery near the designated place had been pure luck. The European astronomers, Leverrier himself in the van, passionately tried to refute these criticisms and doubts. Peirce and Walker in their further work have contributed most to dispel them, first by showing that the real Neptune, through the perturbations it produced, could entirely represent the observations of Uranus, that of Flamsteed included. Moreover, they pointed out that the problem facing Adams and Leverrier, like so many inverse mathematical problems, allowed of several different solutions, all satisfying the data. By assuming the mean distance to be 38, they had hit on one of the solutions, whereas the real Neptune represents another. Figure 31, in which the arrows represent the perturbing forces working upon Uranus, shows that they mattered only in the period between 1790 and 1850 about the conjunction of the two planets. In these years the real and the computed Neptune, because of the large eccentricity of the latter, stood nearly in the same position relative to Uranus; the remaining small difference in distance could be accounted for by a somewhat larger mass of the computed planet. The large differences in position occurred at times when the perturbing forces were very small.

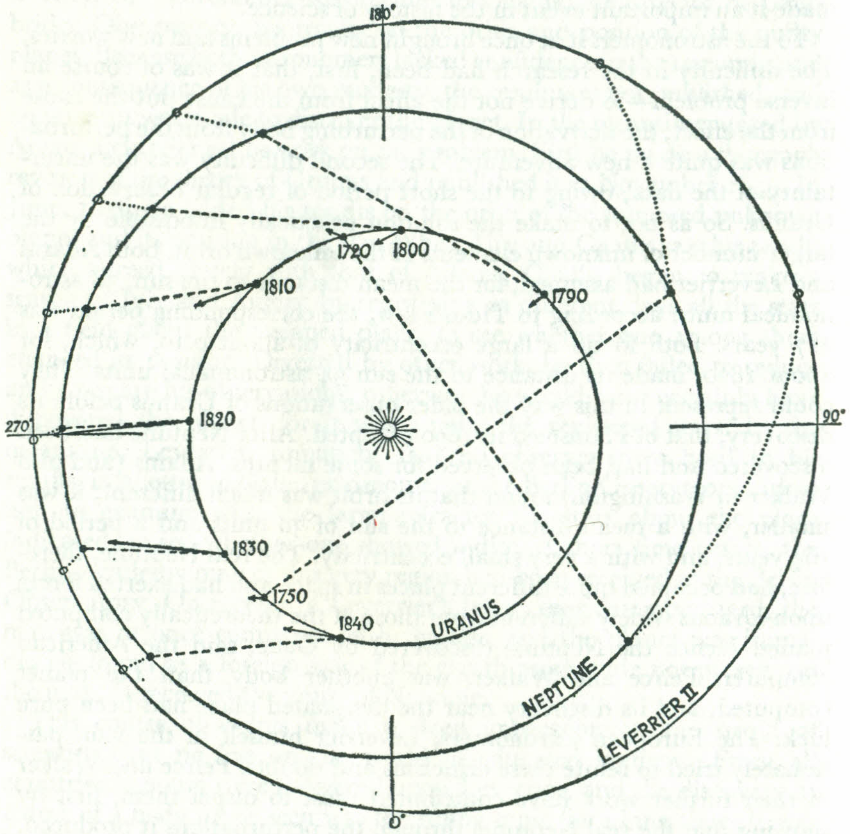


Fig. 31. Perturbing forces of Neptune on Uranus

These researches by the American scientists met with a chilly reception in Europe, and they were mostly ignored as injurious to the honour of the discoverers. The Leiden astronomer Kaiser, in a popular book on planetary discoveries, expressed his surprise and disapproval over such unscientific conduct. He said that evidently the European astronomers had seen the discovery of Neptune chiefly as a means to impress the lay world with the perfection of their science, which in reality, as a human

product, it never can attain, and from prejudice rejected everything that could throw doubt upon the agreement between prediction and realization. 'In North America they did not proclaim the miraculous character of the discovery, but they worked all the harder to make it subservient to the benefit of science.'[194] What Kaiser here observed and criticized can be understood when we consider that in the United States there was no need for such a fight for social progress against antiquated social systems and powers as in Europe where, on the contrary, militant natural science was an important factor of a new culture.

Leverrier, since 1853 director of the Paris Observatory, further devoted himself to the perturbations of the planets. Before the Neptune interlude he had already given an accurate theory for Mercury; now he took up all the other major planets. Using new mathematical methods, he could extend the approximations to a far higher order of terms than his predecessors had done. In this way he achieved (a difficult approximation on account of the multitude and smallness of the terms) an accuracy that had formerly been unattainable but which was needed to keep pace with the greatly increased accuracy of the observations. Whereas at the beginning of the nineteenth century astronomers had been content with an agreement of 10″ or 20″, now the uncertainty decreased to a single second or even less.

As a result of his investigations which, between 1855 and 1877, filled many volumes of the annals of the Paris Observatory, he could state a nearly-perfect agreement between theory and observation. There remained a few discrepancies. The longitude of Mercury's perihelion according to theory must increase 527″ each century; the observations, however, of Mercury's transits before the sun since 1631 with great accuracy afforded 565″, i.e. 38″ more. Moreover, the nodes of Venus's orbit regressed more slowly, and the perihelion of Mars advanced farther (by 24″) than theory demanded. The latter deviations were explained when it appeared that the earth's mass, assumed after Encke's solar parallax, was too small by $\frac{1}{10}$. The large deviation of Mercury, however, could not be removed by any acceptable change in the masses. Different explanations were suggested—an undiscovered planet inside the orbit of Mercury; diffused attracting matter about the sun; a small variation in Newton's law, consisting in an increase in the exponent 2 by $\frac{1}{6000000}$. None of them was satisfactory.

When Simon Newcomb (1835–1909) became chief of the computing office for the *American Nautical Almanac*, he had already spent a great deal of time in preparing exact solar and planetary tables for the ephemerides. An able theorist, he succeeded, by using symbolic methods of analysis, in conveniently arranging, organizing and taking into account the entire field of higher terms, thus improving Leverrier's work. With no less practical ability, he succeeded in collecting and reducing uniformly all the old and new series of often unpublished observations made in Greenwich, Paris, Washington and elsewhere, which had been only partly used by Leverrier. He condensed them into conveniently arranged, easily examined tables and used them for the derivation of the best possible elements. This combination of theoretical and practical skill resulted in a definite test of the theory.

It appeared that, for the four innermost earthlike planets, the secular variations of the orbital elements showed entire agreement between the theoretically computed and the practical values, the differences remaining mostly below the probable errors of the latter, though with some few exceptions. For the recession (per century) of the nodes of Venus, the observed and the computed values were 1,783″ and 1,793″, a difference of 10″ ±3″; for the advance of Mars's perihelion, they were 1,603″ and 1,595″, a difference of 8″ ±4″; for the advance of Mercury's perihelion, they were 575″ and 534″, a difference of 41″ ±2.1″. The first two cases mean differences in the positions of the planets of less than 2″; for Mercury the deviations in position may reach 8″, more than can be admitted for a result of many accurate observations. This, then, remained the only weak spot in the solid structure erected on Newton's law of gravitation. It would have been an idle attempt to remove this contradiction by artificially invented explanations. We had to wait, as is so often the case in such situations, until new points of view should arise from entirely different quarters. They came from the side of physics, in 1905, through Einstein's principle of relativity.

The theory of relativity is based upon the fact that no absolute motions but only relative motions can be observed. In 1914 the principle of the general theory of relativity was postulated by Einstein: that the true form of the laws of nature cannot depend upon the state of motion assumed for the observer. Newton's law of attraction did not conform to this principle, quite understandably, since Newton had proceeded from the notions of absolute space and absolute time. To satisfy the principle of relativity, Newton's simple formula had to be modified, and the consequence was a movement of the perihelia of the orbits. The difference, depending on the square of the ratio of the planet's velocity and the velocity of light, was so small that only for Mercury, the most rapidly moving planet, was the effect perceptible; it consisted in an advance of the perihelion by 43″ per century. The only serious discrepancy that had remained between theory and observation found its explanation here in a natural way, through a refinement of theory, without any arbitrary assumption. Should someone have doubts about the certainty of the relativity principle, there was another consequence: light rays passing large world bodies were subjected to its attraction and might be deviating from their straight course, just as would a particle passing with the velocity of light. This deviation, amounting to 1.75″ for a ray grazing the sun's surface, was established by photographs of the field of stars surrounding the sun during the total eclipse in 1918, taken by two English expeditions; later expeditions confirmed this first result. It gave certainty to the theory of relativity as a whole and so to its explanation of the motion of Mercury.

Though the theory of the planets, especially of the motion of the earth reflected in the apparent solar motion, had practical consequences for the whole of astronomy, it was in the theory of the moon that the vital importance of celestial mechanics lay, because of its importance to practical life and trade. The result of the eighteenth century's work was that Laplace, in his formulas and tables, could represent the course of the moon to less than half a minute of arc. This was far more than the errors of observation; so theory had to be developed further. Laplace had included terms with the third power of eccentricity and inclination. A prize problem was set by the Paris Academy in 1820, asking for the construction of tables of the moon, entirely founded upon theory. In his answer Plana gave a new form of theory, whereas Damoiseau, following Laplace's method, extended it to terms of the seventh order and reached an accuracy to a small number of seconds—yet insufficient for modern needs.

It was owing to the complication, multitude and mutual dependence of the terms that the elaboration of the theory was so very difficult. The ablest mathematicians tried to overcome the difficulty by starting developments according to new theoretical designs. In 1838 Peter Andreas Hansen (1795–1874) at Gotha, lacking good instruments for observation, began to develop theoretical formulas for the motion of the moon. This was the basis of a research that was to occupy him for twenty years. His aim was, by his theory, to represent, with no greater deviations than a second of arc, the complete series of observations of the moon made at Greenwich since 1750. The resulting tables of the moon were published in 1857 by the British Admiralty and were used for more than half a century as the basis of the *Nautical Almanac*. In 1862–64 he added to it an exposition of the theoretical basis and all the data used. Along an entirely different though equally general way Charles Delaunay in Paris at the same time developed a lunar theory and published his results in 1860 and 1867. So very different were their methods that, later on, only by an extensive special research, was Newcomb able to compare their results for each of the terms. Then such a wholesale agreement came to light that theory could be said to have given an unequivocal answer to the question of the moon's motion.

Yet there were some weak points. The rapid advance of the longitude of the moon's perigee of about 40° per year, which had embarrassed Clairaut in the eighteenth century, had remained a difficulty. Through a laborious computation of eight terms, Delaunay had succeeded in approximating its value to $\frac{1}{8000}$; since this margin amounts to 18″ yearly, the uncertainty is far too great to be tolerated. Then George W. Hill, computer at the American 'Nautical Almanac Office' (described by S. Newcomb as 'perhaps the greatest living master in the highest and most difficult field of astronomy, winning world-wide recognition for his country in the science, and receiving the salary of a department clerk')[195] in 1877, by a stroke of genius, solved the problem through an entirely new principle of treatment. Whereas the common method proceeds from the general orbit of the two-body problem, the ellipse with its chance eccentricity, to which the solar perturbative action is added, he proceeded from the orbit in the three-body problem in its most simple form, without chance characters—a circle transformed by Tycho's variation—to which the chance eccentricity is then added. This fitted in so well with the inner essential character of the problem that the successive terms of the development decreased with a factor $\frac{1}{30000}$, so that a short computation of some few terms sufficed to produce the desired quantity to $\frac{1}{100000000000}$ of its amount. Was it now in exact agreement with observation? The question cannot be put in this form. In addition to solar attraction, the moon's perigee is advanced by the flattening of the earth, in a way that depends on the density distribution within the earth. If the computed solar effect is subtracted from the observed value, there remains the flattening effect. The resulting flattening is the mechanical flattening (i.e., the difference between the moments of inertia), not the geometrical flattening of the outer form. It can be used as a datum to derive the distribution of mass inside the earth. Hill's theory has been used by Ernest W. Brown as the basis of his lunar tables, which since 1922 have replaced Hansen's tables in the nautical almanacs.

A second point was the secular acceleration of the moon. Laplace had found that the ancient eclipses and the theoretical computation of the sun's perturbing effect agreed in establishing its value at 10″ per century. By theory Hansen had found 11.47″, and from observations 12.18″; not a great difference. In 1853, however, by more extensive computations including higher terms, Adams found the theoretical value considerably smaller, 5.70″. At first he was contradicted from all sides, chiefly because the former value fitted in so well with the results of observation; but gradually his conclusion was confirmed by Delaunay,

by Plana, by the eminent mathematician Cayley, and was finally also recognized by Hansen. This involves the presence of another factor accounting for the other half of the observed acceleration.

This other factor was found in tidal friction. The high-tide wave is dragged along by the earth's rotation; the mutual attraction of the wave crest and the moon retards the rotation of the earth and pulls the moon forward, increasing its angular momentum. This results in an expanding of its orbit and a slowing down of its orbital motion; expressed in days of increased length, the month is apparently shortened and the motion accelerated. It was tacitly assumed that the tidal friction of the lunar tides was able to explain half the observed acceleration. In 1909, however, Chamberlin, geologist at Chicago, deduced that the friction of the oceanic waters against sea bottom and shores was too small to produce such an effect. So this way out seemed to be cut off, until in 1919 Geoffrey Taylor showed that it is especially in shallow inland seas and bays with strong tidal currents that the energy of motion of the earth and moon is lost through tidal friction. According to a rough computation, the Irish Sea could produce $\frac{1}{56}$ of the necessary loss; the Bering Sea later on was found to be responsible for more than half the amount needed, whereas the oceans contributed little. It was of course not possible to compute the exact amount due to all the seas on earth; so the value adopted by Hansen must be considered as an empirically and not a theoretically determined quantity.

In the Greenwich longitudes of the moon there remained, after all other perturbations had been inserted, a slow fluctuation amounting to some tens of seconds. Hansen supposed it to originate from a perturbation by Venus, with a period of 240 years amounting to 21″. Delaunay, by theoretical computation, could find for this term an imperceptible amount only, below 1″. Yet Hansen, because the Greenwich observations clearly indicated it, introduced it into his tables, confident that theory by means of further researches would be able to explain it. So it was now an empirical term, based upon the rather short time of one century; the earlier observations—less reliable, it is true—did not confirm it. What was worse: in the years since 1860, when Hansen's tables had been introduced as a basis for the almanacs, the moon began to deviate increasingly from the tables. In the sixties it was retarded some seconds; in 1880 it was already 10″ behind; and in every following decade the retardation increased. Then Newcomb in 1878 took the matter in hand. He had already begun to collect, as a check on the meridian observations, all the ancient observations of eclipses and star occultations since the invention of the telescope; these he copied from the manuscript data in the archives of the observatories, while he was on a journey across Europe in 1871. The entire collection of observed data could be represented by introducing an empirical term of 17″ with a period of 273 years—practically confirming Hansen. An explanation, however, was still lacking; so it was not to be expected that in the coming years the differences between the tables and the observed positions would cease. Tisserand in the third volume of his standard textbook, *Traité de mécanique céleste*, in 1894 closed his exposition of the lunar theories with the words: 'The theory of the moon finds itself arrested by the difficulty we have just developed; in Clairaut's time also, theory seemed to be unable to explain the movement of the perigee. It will also vanquish the new obstacle now presented; but a great discovery has still to be made.'[196]

The discovery was indeed made and the problem solved, but in an entirely unexpected way. The cause of the deviations did not lie in the field of theory, nor in the moon's motion, nor in the forces of attraction, but in irregularities in the earth's rotation. Newcomb for a moment had considered it, but in his last work on the moon, in 1903, he had dropped the idea as improbable and not sufficiently warranted. Such variations in the period of rotation of the earth must reflect themselves in corresponding apparent irregularities in all rapidly moving heavenly bodies. In 1914 Brown established that in the motion of the sun, Mercury and Venus the same oscillations occurred as with the moon, at the same time but to a lesser amount. It was confirmed by Spencer Jones's detailed studies at Greenwich in 1926 and 1939. When Willem de Sitter (1872–1934) at Leiden investigated the motion of Jupiter's satellites and worked out a complete theory of all the perturbations in this sub-system, he found in 1927 the same irregularities in their motion. So the conclusion was clear; the moon was not guilty; it was the earth. The empirical term by Hansen and Newcomb could now be dropped; in its place came more or less sudden leaps in the earth's time of rotation. About 1667 it was lengthened by 0.0011 second; about 1758 it was shortened by 0.0006; about 1784 by 0.0017, and in 1864 by 0.0027. This caused the alarming deviations from Hansen's tables. Then followed a lengthening of 0.0017 in 1876, of 0.0034 in 1897, and a shortening of 0.003 in 1917.

It is a remarkable fact, though not surprising, that the simple conditions of world structure given by primitive experience and assumed throughout the centuries to be the results of cosmological or mechanical principles could not be maintained against modern refined research. Such was the case with the invariable position of the rotation axis within the earth, as well as now with the invariable period of the earth's rotation. Their close approximation was an important aid in establishing a simple and harmonious astronomical world concept. The great precision of the nineteenth-century measurements showed the deviating—

though very little deviating—reality. Theoretical mechanics, in the hands of Euler, had demonstrated that only as a highly exceptional case could the rotation axis keep an invariable position in a rotating body. In 1885 S. C. Chandler, in a series of latitude determinations at Cambridge, Mass., found progressive changes of 0.4″ in half a year, which could not be ascribed to any error, so that 'the only alternative seemed to be an inference that the latitude had actually changed. This seemed at the time too bold an inference to place upon record . . .'[197] The same thing had already occurred in a series of very accurate observations at Washington in 1862–67; the observers could find no error, but did not venture to explain the periodic deviations as real variations in latitude. In 1888 Fr. Küstner published the results of accurate measurements he had made in Berlin in 1884–85, in order to derive the constant of aberration; he explained the differences found as real variations in latitude. To test the explanation, a second observer was sent to Honolulu at the other side of the North Pole; so it was ascertained that the pole had moved. It described a small orbit of some tens of feet around the end of the axis of maximum inertia. Euler had computed that such an orbit in a rigid earth is completed in 305 days; the search for variations in latitude in this period had always prevented the discovery of the real variations. The real period appeared now to be about 430 days, besides an additional yearly oscillation. Newcomb soon explained the difference by the consideration that the ocean waters, obeying centrifugal force, continually try to adapt their figure to the momentary axis of rotation and in this way slow down the displacement of the axis. The yearly displacement of water masses between arctic and antarctic regions, by melting and freezing, as a cause of the yearly oscillation of the poles, points to other displacements of masses within the earth as a cause of polar variations in general. Now that the period of rotation has turned out to be inconstant, we have to appeal to the same field of events: geological changes within the earth. Since the total moment of momentum is constant, an acceleration of the rotation means a shrinking, its retardation means an expansion, in the earth's mass. It must be added that redistributions of mass to account for the observed leaps demand, from a geological point of view, the transportation of enormous quantities of matter.

For the moon tables in the almanacs the situation now looked precarious. There is no possibility of predicting or computing beforehand, with indefinitely increasing accuracy, the course of the moon measured by the common days of the earth's rotation. Unpredictable changes in the earth's rotation, dependent on unknown geophysical events, cause irregularities in our counting of time which appear in the observed places of the moon. That great ideal of perfect tables of the moon, which navigation needed, on which for several centuries so many of the ablest theorists and observers had spent their best efforts and which seemed within reach—was it to remain unattainable?

Navigation no longer needs the moon. For the centuries-old problem of the longitude at sea a complete solution has meanwhile been found in quite a different way, by wireless time signals. In 1886 the physicist Heinrich Hertz at Bonn made his first experiments with electrical oscillations propagated as waves. In 1895 Marconi introduced wireless telegraphy by means of these radio waves. In 1913 international collaboration was established to send out time signals every exact hour, Greenwich time; they can be observed by ships at any point of the ocean. Once every hour the precise standard time is given, which, compared with the local time, gives the longitude. The problem of the longitude at sea was an episode in the history of astronomy, but highly important for the progress of science now closed. It greatly stimulated celestial mechanics as an important branch in the general theoretical knowledge of mankind. In its stead we are now faced with phenomena of our own earth, and this may be a starting point for new geophysical study.

CHAPTER 35

PLURALITY OF WORLDS

A silent force in astronomical research, among scientists and especially among wide circles of interested laymen, was the desire to acquire knowledge about other worlds as an abode of other men. What for authors of antiquity was mere playful fancy, had grown, since Copernicus and Bruno, to be a serious though hesitant opinion that possibly on the moon and the other planets conscious beings might live, gifted with intelligence and reason. Now the study of the planets, of their surface and their conditions, acquired a background of more profound interest.

As long as telescopes were imperfect, this study had to be primitive. At the end of the eighteenth century a diligent amateur, Amtmann Schroeter of Lilienthal near Bremen, using his reflecting telescope, made hundreds of drawings of lunar mountains and of markings on the planets. Compared with the standard of the period that followed, they were rather coarse. This higher standard was set by the refractors from Munich; with their sharp images and easy handling, they opened a new way for this study.

That the moon should be inhabited entirely suited the rationalist mode of thinking of the eighteenth century. In one of the first of the papers he presented to the Royal Society, William Herschel had expressed his conviction that 'lunarians' existed; but on Maskelyne's advice he cancelled this sentence as insufficiently founded. Afterwards the question was raised by Gruithuisen at Munich, who said in 1816 that he had seen clouds on the moon and had recognized fortifications and other human buildings in some lunar formations. Such fancies, however, were soon cut short. In 1834 Bessel established that from occultations of stars the moon's diameter was found to be not perceptibly smaller than from direct measurements. This means that the light rays from the star grazing along the moon's edge were not deviated by atmospheric refraction. The refraction in the horizon, which for the earth's atmosphere amounts to about 2,000″, can be no more for the moon than a few seconds, so that its atmosphere must at least be $\frac{1}{2000}$th of the density of the earth's. This would be insufficient for the respiration of man or animal.