HERSCHEL AND THE CONSTRUCTION OF THE HEAVENS

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The history of astronomy knows great telescope builders, great observers, and great theorists; but only William Herschel falls indisputably into all three categories. When he became a professional astronomer in the summer of 1782, he had already demonstrated his skills in the construction of big reflectors: the mounting of his 20ft still left much to be desired, but otherwise his mirrors, his eyepieces, and his mountings combined to give him a head start over any other astronomer in the examination of distant and therefore faint objects. And in little over a year after his arrival near Windsor Castle where he was to be available on occasion to show the heavens to the Royal Family, he completed one of the great telescopes of all time: his 'large' 20ft reflector, with mirrors of 18 inches diameter, and soon to be equipped with a stable ladder-type mounting so that telescope and observing platform could be rotated together by a single workman. This mounting he further improved and refined in the years to come, and meanwhile the telescope was his favourite instrument and in constant use during his twenty-year systematic search of the sky for nebulae. In his extreme old age it was refurbished under his direction by his son John, who resurveyed his father's nebulae and then took the telescope to the Cape of Good Hope to extend the coverage to the southern skies. John's _General Catalogue of Nebulae and Clusters of Stars_ (1864) led to the _New General Catalogue_ that astronomers use today.

Herschel supplemented his pension by constructing telescopes for sale, so that he was indeed a professional telescope maker. For himself he built with help from the King a monster reflector of 40ft focal length, with mirrors 4ft in diameter and weighing up to a ton. The mounting was essentially a scaled-up version of that of the large 20ft; but it proved cumbersome in use, and the modified alloy of the mirrors led to rapid tarnishing: Herschel had exceeded the limits of his technology. He did however complete an excellent reflector for the King of Spain, with 2ft mirrors and focal length of 25ft. Until its destruction by Napoleonic troops, this was the finest telescope of its kind; but, like almost all the telescopes Herschel made for sale, little use was made of it by its owners.

Herschel's skill as an observer had been demonstrated by his discovery of Uranus. This skill was allied to an heroic endurance that astonished visitors to his home. One of them wrote:

> ... I went to bed about one o'clock, and up to that time he had found that night four or five new nebulae. The thermometer in the garden stood at 13° Fahrenheit; but in spite of this, Herschel observes the whole night through, except that he stops every three or four hours and goes into the room for a few moments. For some years Herschel has observed the heavens every hour when the weather is clear, and this always in the open air, because he says that the telescope only performs well when it is at the same temperature as the air. He protects himself against the weather by putting on more clothing (Lubbock, _The Herschel Chronicle_, Cambridge, 1933, 138).

Astronomers not privileged to see him at work would find in _Philosophical Transactions_ the fruits of his labours: two catalogues of double stars, three catalogues of nebulae and clusters adding two-and-a-half thousand to the hundred or so already known, lists of stars in diminishing order of apparent brightness designed to facilitate the detection of variables -- the outcome of observational campaigns of unprecedented persistence.

But they were also the signs of theoretical interests hitherto unknown in astronomy. Herschel was creating a new astronomy, an astronomy that was part of natural history, as the factual underpinning of a speculative cosmology. In the investigation of the solar system (to which most contemporary astronomers were committed), each planet and each satellite had its own name, each comet appeared in a given year and had a recognisable individuality. Herschel, by contrast, counted stars and drew inferences from the numbers alone. And he was a natural historian collecting innumerable specimens, double stars by the hundred, nebulae by the thousand. He described and classified, first according to superficial appearances, later into true species. To teach the life-cycle of a nebulae he paraded before his readers nebulae that he declared were young, middle-aged, and old, after the manner of a botanist pointing out trees at different stages of their growth. Such methods were totally alien to astronomy; they were importations from natural history, a truly new astronomy.

This fact-gathering, so orthodox in other fields of scientific enquiry, might have been painlessly assimilated into astronomy if Herschel had been of the cautious temperament of most great observers. Instead, with a frankness seldom equalled in science, he announced in print his intention to speculate too much rather than too little:

> If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices that soon gave way when better theories were offered. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions, but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that, I could wish not to fall into the latter error (Phil. Trans., lxxv, 1785, 213).

As usual, Herschel was as good as his word. 'A knowledge of the construction of the heavens', he wrote in Philosophical Transactions in 1811 (p. 269), 'has always been the ultimate object of my observations', and from his early days as a professional astronomer to the end of his long life, he published a succession of massive papers on the large-scale structure of the universe.

His contemporaries did not know what to make of it. His stubborn self-confidence verging on arrogance alienated some of them, while even his most loyal supporters were at times baffled and ill-at-ease. For his papers on the physical description of the solar system he received due credit; but these were mere asides from his central concern with the construction of the heavens, and here his contemporaries could not follow him.

They could not follow him because he alone possessed huge cosmological telescopes designed to reach far out into space. When Herschel described a nebula, other astronomers could either believe him or desbelieve him; they could not do what astronomers can normally do, and that is to look for themselves and judge whether what is claimed is in fact true. And even if they believed his descriptions, still the theoretical questions to which these observations were directed had never been part of astronomy, and his methods were often alien importations from natural history. The accepted conventions for the reobservation and critical assessment of published science by fellow professionals, so fundamental in any scientific community, simply did not exist as far as Herschel's life-work was concerned. It was therefore impossible for the novel concepts and methods he created to be assimilated into astronomy in his own lifetime, and they were not so assimilated. He was honoured in his own time as a pioneer telescope builder, a dedicated observer, and the maker of several important discoveries in the solar system. His 'construction' papers, however sceptically received, were published in _Philosophical Transactions_ and so were widely available and part of the permanent and accessible record of the science of his age. Future generations of astronomers would inherit the questions he asked, his methods, even his terminology like 'planetary nebula', 'asteroid', and 'solar apex'; and we today can look back on his achievements conscious of the promise they held for the future -- for Herschel virtually created stellar astronomy and scientific cosmology. But as we now survey briefly his major contributions to the study of 'the construction of the heavens', we must not be deceived into thinking these were appreciated by his contemporaries.

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DOUBLE STARS

As we saw in the preceding article, Herschel's first major task in astronomy was to collect double stars in the hope that some of them would be formed of a nearby star and a second star so distant as to be virtually a fixed point provided by nature for the convenient measurement of the annual parallax of the nearby star. He published catalogues of double stars in Philosophical Transactions in 1782 and 1785, and a third as a token paper in 1821 in the Memoirs of the newly-founded [Royal] Astronomical Society; but he never seriously attempted to detect annual parallax. Instead, like James Bradley with the discovery of the aberration of light, Herschel was to earn an unexpected reward for his efforts.

Newton had declared gravity to be a universal law of nature, but outside the solar system he had no evidence for this. John Michell (Phil. Trans., lvii (1767), 234) had pointed out that double and multiple stars were many times more frequent than one would expect on the hypothesis that all were chance (line-of-sight) configurations, and that most must be true physical associations. Michell's paper was not known to Herschel when he began his search for double stars, and in 1784 (Phil. Trans., lxxiv, 35) Michell published a second paper in which he expressly warned that most of Herschel's doubles would prove to be binary stars and so be useless for the detection of annual parallax. That Michell was right became clear when Herschel's twenty years of sweeping for nebulae came to an end and he had time to re-examine some of his double stars. The components of several, he announced (Phil. Trans., 1803, 339; 1804, 353), had altered position in a way that showed they were in orbit around each other. Of these the best documented was Castor, for in about 1759 James Bradley had remarked to Nevil Maskelyne that the line joining the components was parallel to the line joining Castor to Pollux. This extension of the time-span of Herschel's own observations enabled him to give the double star a period of rotation of 342 years (modern value: 306).

Herschel had little doubt that the force binding the components together was gravity, but observational proof of this would not be available in his lifetime.

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THE MOTION OF THE SOLAR SYSTEM

Until 1718 all the evidence had indicated that the stars were indeed 'fixed' and motionless, for they were without exception in the same positions relative to each other as recorded in Antiquity. In that year Edmond Halley announced (Phil. Trans., xxx (1717-19), 736) that Aldebaran, Sirius and Arcturus had altered position in latitude since the time of Ptolemy. But Bradley's subsequent discovery of aberration revealed a major source of error in modern star catalogues, and it was not until 1760 that proper motions of stars were known in any quantity. In a lecture that year (Opera inedita, 1775, VI: De motu fixarum proprio), Tobias Mayer listed the changes in right ascension and declination of some eighty stars whose modern positions he compared with those recorded half-a-century earlier. Of these changes he considered fifteen or twenty large enough to be due to true proper motions rather than instrumental errors. Mayer also explained the pattern of proper motions that would be generated if the solar system were moving towards some region of the sky: 'all the stars which appear in that region would seem to be gradually separating from each other one by one, and those which are in the opposite part of the sky would seem to be joining up...' (trans. E.G. Forbes, London, 1971, 112). No such pattern, he declared, was present in the data he had given.

It was an uncharacteristic problem for Herschel to tackle, for his telescopes and his observing programmes were irrelevant. Instead, he would be working at his desk on data published by other astronomers, though when he first tackled the problem in the winter of 1782/83 he did not own a copy of Mayer's lecture. Instead, he possessed Maskelyne's Astronomical Observations Made at the Royal Observatory... 1765-1774 (London, 1776), which gave a brief list of proper motions (seven stars in right ascension, and Sirius and Arcturus in declination), and the supplementary vol. iv of the second edition of Lalande's Astronomie (Paris, 1781) which carried a list of the most convincing of Mayer's proper motions.

Very conveniently, Maskelyne's seven motions in R.A. were all consistent in direction with the motion of the solar system towards an 'apex' with R.A. between about 14^h and 19^h. As to

Lalande's stars, some were included because of their large components of motion in declination, although their listed components in R.A. were small and probably spurious. With typical insensitivity Herschel took all these components in R.A. at face value, even the negligible change of 3" in fifty years recorded for Aldebaran. The majority of these components were again consistent in direction with an apex between about 14^h and 19^h. Only one star lay well inside this range or its opposite: Aldebaran; and by taking the direction of Aldebaran's component as established (and refusing to recognise it for the negligible quantity it was), Herschel managed to halve the range of R.A. within which the apex of the solar motion had to lie if the listed exceptions were to be reduced to a minimum. The mathematically more difficult problem of declination he tackled essentially by inspection, and so arrived at a proposed apex near λ Herculis, astonishingly close to the best modern positions. But while it is true that the data available to Herschel suggested a region of sky where the apex might lie, the proximity of his apex to modern positions owes much to his use -- or rather misuse -- of the figures for Aldebaran.

Never one to leave well alone, Herschel returned to the problem in papers published in Philosophical Transactions in 1805 and 1806. This time he tried to derive not only the direction of the solar motion but also its velocity; and for this he needed first to establish the velocities of stars relative to the solar system. Nothing of course could be known of the line-of-sight components. The transverse components had to be derived from the proper motions (that is, angular velocities) multiplied by the distances. To derive distances he had to assume that all stars are intrinsically of the same luminosity, so that distances are related to apparent magnitudes. As in fact stars vary enormously in luminosity, Herschel became trapped in a tangle of argumentation. In particular, he concluded that bright (and supposedly near) stars with no known proper motions and apparently at rest must in fact be keeping pace with the Sun, so that Herschel actually ended up with more motions than he started with -- in contrast to the earlier investigation whose purpose was to show that many observed proper motions were merely optical effects of the solar motion. But good came out of ill, for this led Herschel to discuss the star cluster of which the Sun is part, and which he supposed to be formed of stars moving through space with the Sun.

THE MILKY WAY

In the early years of the second half of the century, three speculators -- Thomas Wright of Durham, Immanuel Kant, and J.H. Lambert -- had each suggested that the Milky Way is the optical effect of our immersion in a layer of stars. We do not know if any hint of this reached Herschel; in any case, he determined to take the further step of charting the outline of the Galaxy, and he realised he could do this if he allowed himself two assumptions. The first was that his telescope (the 'large' 20ft) could penetrate to the borders of the system in every direction -- for otherwise his enterprise was hopeless. The second, and more interesting, was that within the Milky Way system, space is uniformly stocked with stars, so that when we see more stars in a given direction, this is because the system extends further in that direction (rather than because the stars are more clustered).

Armed with these assumptions, Herschel systematically counted the numbers of stars around a great circle of the sky (which was all he could spare time for). He converted these numbers into (relative) distances and published the resulting cross-section of the Galaxy (Phil. Trans., lxxv (1785), 213). It was a dramatic gesture that virtually created the method of stellar statistics. But as time went on Herschel lost confidence in both his assumptions. His 40ft telescope brought many more stars into view, so that the 20ft had not after all penetrated to the borders in every direction: indeed, appearances suggested that even with the 40ft the Milky Way was in some directions 'fathomless'. Further, as his sweeps for nebulae and star clusters continued, so did Herschel's experience of how widespread is the phenomenon of star-clustering, and he admitted (Phil. Trans., 1817, 302) that higher star counts indicated greater clustering as much as greater distance to the border. But if his assumptions were erroneous, his technique of stellar statistics was to become a basic tool in stellar astronomy.

NEBULAE

As we saw in the preceding article, on the very first page of his first Journal, which he wrote in March 1774, Herschel commented that the Orion Nebula was now different in shape from the sketch in Robert Smith's Opticks 'and perhaps from a careful observation of this Spot something might be concluded concerning the Nature of it'. It is astonishing to find this musician for whom in 1774 astronomy was still a recently acquired hobby putting his finger so quickly on the crucial question; for if a nebula had altered shape in only a few decades, then it could not be a huge star system disguised by distance, but must be a small, nearby object and non-stellar in nature. Only the Orion Nebula had been sketched in the seventeenth century, and it was this sketch (by Huygens) that Herschel was comparing with what was now to be seen.

Herschel that March sketched the Orion Nebula himself and again in 1776 and 1778, and satisfied himself that it did indeed change shape. But although his ('small') 20ft was one of the best telescopes in existence for the examination of nebulae, it was not until after his move to the Thames Valley that he began to examine nebulae in quantity. He found on examination that 'most' of the nebulae in a catalogue by Charles Messier 'yielded to the force of my light and power, and were resolved into stars ' (Phil. Trans., lxxiv, 1784, 437). In the autumn of 1783, on the completion of his 'large' 20ft, he set about systematically 'sweeping' the sky for nebulae. Some were clearly 'resolved' into stars by his powerful telescope, but others remained milky in appearance. How to distinguish star systems disguised by distance from 'true' nebulae (if such there were)? Herschel satisfied himself that a star cluster disguised by distance presented the appearance of 'mottled' or uneven nebulosity that he therefore termed 'resolvable', while true nebulae had the even appearance of 'milky nebulosity'.

As a natural historian who was collecting hundreds of nebulae, Herschel found himself compelled to classify them, at first by mere appearances but later, he hoped, into a natural classification.

As he reflected on his specimens, he realised that star clusters suggested the presence of a clustering force (gravitational attraction?), and that a widely scattered cluster might be expected to develop in time into a more condensed cluster. To this extent it would be proper to describe a scattered cluster as youthful and a condensed cluster as, by comparison, aged -- truly novel concepts in astronomy.

Meanwhile, Herschel was beginning to publish both catalogues of nebulae and theoretical papers on 'the construction of the heavens'. On 22 June 1784, just five days after his first 'construction' paper was read to the Royal Society, Herschel came across the Omega Nebula (M17), followed next month by the Dumb-bell Nebula (M27); and in both he found the two nebulosities, milky and resolvable, coexisting side by side. Since the resolvable nebulosity was, he believed, composed of great numbers of distant stars, might not the milky nebulosity be composed of great numbers of very distant stars? -- in which case he had been wrong in the past to equate milky with 'true' nebulosity, for he now realised that all the appearances could be accounted for in terms of star systems alone. All, that is, except for the observed changes in the Orion Nebula, which he proceeded to disregard.

In his second 'construction' paper (Phil. Trans., lxxv, 1785, 213), Herschel shows that many of the nebulae (that is, star clusters) he had observed could have come about through the action of gravitational attraction on an initial, widely scattered distribution of stars. And he explains that some extensive yet milky nebulae must be huge star systems that 'may well outvie our milky-way in grandeur'. Yet what was the ultimate fate of a star system? Were there repulsive forces to ward off gravitational collapse? If collapse took place, what then happened? And what role in the universe was played by the mysterious 'planetary nebulae' that Herschel had discovered and which looked like planets but might be nebulae or might be a previously-unknown kind of heavenly body?

On 13 November 1790, during his regular sweeping, Herschel came across NGC1514, which is in fact a planetary nebula of unusually large apparent diameter with a prominent central star. Herschel admitted that the star must be connected with the encircling nebulosity, and in a dramatic shift of position he accepted that the star was condensing out of what was therefore true nebulosity. Planetary nebulae, and 'nebulous stars' as NGC1514 was classified, now became stages intermediate between the nebulous phase and the stellar phase of the life history of a nebula/star-cluster; and in papers written towards the end of his career (Phil. Trans., 1811, 269; 1814, 248), Herschel selected from his catalogues specimens at every stage in their life-history, claiming thereby to be in effect permitting his readers to witness a nebula develop from infancy as scattered nebulosity to its old age as a tightly-packed globular star cluster.

In other papers Herschel permitted himself wide-ranging speculations on related topics. Where did the nebulosity come from? Perhaps it was light itself, emitted by stars and collected into clouds by the mutual attraction of the light particles. A comet might be a small cloud of nebulosity pulled in by the attraction of the Sun and giving some of its substance to replenish the Sun as it passes through perihelion. Other material in the comet may be consolidated by the heat of the Sun, and after many such passages the head of the comet may become planet-sized. Stars themselves are in reality only planets with a luminous outer atmosphere.

And so Herschel piled speculation on speculation. His contemporaries were often baffled and sometimes openly hostile. He was admitted to be a telescope builder on an heroic scale, an observer of exemplary dedication, and the author of many disoveries within the solar system. But the general reaction to his theories of the construction of the heavens is expressed in the 1820 'Address of the Astronomical Society of London, explanatory of their views and objects':

> Beyond the limits however of our own system, all at present is obscurity. Some vast and general views of the construction of the heavens, and the laws which may regulate the formation and motions of sidereal systems, have, it is true, been struck out; but ... they remain to be supported or refuted by the

slow accumulation of a mass of facts...

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