available. The principal source of his new information is a manuscript journal written by Herschel at some date later than 1793. Let us see how this supplements or modifies the accepted story of his early life.

We learn for the first time some details of his first visit to England, as a boy of eighteen, in the band of the Hanoverian Guards. His sister records only that he brought back with him to Hanover a copy of Locke, on which he had spent all his small pay. The now published journal adds some details of importance. The regiment was quartered in various towns of Kent. At Maidstone :

'I applied myself to learn the English language and soon was enabled to read Locke on Human Understanding. From Maidstone we marched to Coxheath, where the Hanoverian troops were encamped. Here as well as at Maidstone my father, my eldest brother and myself made several valuable acquaintances with families that were fond of music, and which on mine and my brother's return to England proved of great service to us. During our stay in camp we took leave of absence for a short visit to view London.'

After a few months the regiment was ordered back to Hanover, and in the spring of the following year Herschel saw service in the campaign which opened the Seven Years' War. The regiment suffered severely at the battle of Hastenbeck, being involved in the retreat of Cumberland before the forces of Marshal d'Estrées. Unable to endure the hardships of the campaign, Herschel found means to leave the army, under circumstances which have been obscure hitherto, and have given rise to a legend—'too long and too readily believed'—that he deserted from the army.

Herschel's journal gives the following version of the affair :

'About the time of the battle of Hastenbeck (July 26) we were so near the field of action as to be within the reach of gunshot ; when this happened my father advised me to look to my own safety. Accordingly I left the engagement and took the road to Hanover, but when arrived there I found that having no passport I was in danger of being pressed for a soldier ; it was therefore thought proper for me to return to the army. When I had rejoined the regiment I found that nobody had time to look after the musicians ; they did not seem to be wanted. The weather was uncommonly hot and the continual marches were very harassing. At last in September my father's opinion was, that as on account of my youth I had not been sworn in when I was admitted to the guards I might leave the military service ; indeed he had no doubt but that he could obtain my dismission, and this he after some time actually procured (in 1762) from General Spörcken who succeeded General Sommerfeld.'

At this point the editor interrupts Herschel's narrative to point out that the existence of the formal discharge puts an end to the legend that he deserted from the army, and he prints in full the document, whereby 'Friederich Wilhelm 'Herschell, gebürtig von Hannover, 25 Jahre alt, blondt 'von Haaren, Langer Statur . . .' is commended for his services and granted his discharge.

The date of Herschel's arrival in England is not known precisely, but it must have been in November 1757. Of his life there until 1760 nothing has been known hitherto, and his biographers have assumed that it was a hard struggle for a bare living. The journal gives a happier account :

'When we arrived in London we made use of the recommendation of some of the families we had been known to when we were in England before. We were introduced to some private concerts, my brother attended some scholars and I copied music, by which means we contrived to live pretty comfortably in the winter, and in the summer we visited some families in and near Maidstone and Rochester and had a concert at Tunbridge Wells.'

In 1760 William Herschel found himself in difficulties. London was overstocked with musicians, and he felt that he had little chance of success. His brother Jacob obtained a good appointment, and, as usual, relied upon William to supply the means of furnishing him for the post. But very opportunely, the 'Memorandums' tell us, he received an offer to go into Yorkshire, where the Earl of Darlington wanted a good musician to be at the head of a small band for a regiment of Militia of which he was the Colonel.

Six busy years were spent in the North of England, at Richmond, at Sunderland, at Halnaby near Darlington, at Newcastle, Pontefract, and Leeds. Six comfortable and fairly prosperous years they seem to have been, with long stays at the seat of Sir Ralph and Lady Milbank at Halnaby, and with Sir George and Lady Cook at Wheatley, where his mare, 'standing idly in the stable, and being overfed by Sir Bryan's 'grooms, died.' Eighteen symphonies were composed during these years, and he relates, in a letter to Dr. Hutton, that 'during all this time, though it afforded not much leisure for 'study I had not forgot my former plan, but had given all my 'leisure hours to the study of languages.' Italian and Latin were mastered; Greek took up too much of his leisure, and was abandoned; the study of harmony led to mathematics; Dr. Smith's 'Harmonics' led naturally in time to the same writer's famous treatise on Optics; and so we may find a trace of the path which led to astronomy and to telescopes. There is no more than this slight trace. In 1766, February 19, we have this one note: 'Wheatley, Observation of Venus.' Five days afterwards he records an eclipse of the moon, seen at Kirby; and then there is no further mention of astronomy for seven years.

We had hoped to find in these records some indication of the growth of that resolution recorded by Herschel himself, in the well-known letter to the 'Göttingen Magazin der Wissen-'schaften und Literatur' (1783), 'henceforth to devote myself 'wholly to those sciences from the pursuit of which I alone 'looked for all my future happiness and enjoyment.' There is no indication; and it seems possible that the resolve was not in reality adopted until later. But we do find in this account of the years in the North of England the growth of the graceful and accomplished gentleman who held his scientific court in after-years by the King's Court at Windsor.

In 1766 Herschel held for a few weeks the post of organist at Halifax; but he was already engaged to go to Bath, where for six years more he was a successful organist, composer, conductor, and teacher, making an income which rose in a few years to some four hundred pounds per annum, and giving yet no sign of any interest in the heavenly bodies. Then in the journal comes the sudden entry: '1773, April 19. Bought a 'quadrant and Emerson's Trigonometry.' His sister Caroline has recorded the 'uncommon precipitancy which accompanied 'all his actions.' Possibly the purchase of the quadrant was a sudden inspiration; at least from that moment the inspiration never failed. On May 10 he bought a book of astronomy and another of astronomical tables; on May 24 an object glass of ten feet focal length; on June 1 he bought many eyeglasses and made tin tubes; and on June 14 he hired a two-foot reflecting telescope for three months.

Thus, in 1772, Herschel found himself embarked on a voyage of discovery with such instruments as he could construct from the materials then obtainable. Let us glance at the state of telescope-making at the time, that we may the better appreciate the difficulties he had to face. It was an age of stagnation in telescopic discovery. Progress with the refracting telescope had been checked for nearly a century by the extreme difficulty of manipulating the long instruments with a single lens for objective. The achromatic objective had been invented ; but the impossibility of procuring the necessary discs of flint glass had delayed its coming into use, and it is improbable that in 1773 an achromatic objective of more than two inches aperture could have been bought. In the course of the eighteenth century some progress had been made in the construction of reflecting telescopes after the models of Newton and of Gregory ; opticians such as Hearne or Short were able to supply instruments of apertures up to about six inches, though doubtless at a high price. These were useful instruments of their kind ; and we think that if Grant was right in saying that no discovery had ever been made with the reflecting telescope, the fault lay not so much with the opticians as with the observers, who lacked the courage really to try what could be done with them. The faculty of telescopic discovery is in truth one of the rarest of faculties ; for just as no one had possessed it in any marked degree in the twenty or thirty years before the time of Herschel, so not one of those who in after years bought telescopes made by him succeeded in doing anything considerable with them.

Herschel's early attempts at telescope-making were directed to the building of a refractor on the Huyghenian plan, as is evident from the purchase of the object glass of ten feet focal length, and from the account given by his sister of her making tubes of pasteboard, which were too weak and were replaced by tin. A few weeks' experience of the inconvenience of these long unrigid tubes showed that little could be done until the instrument was made more manageable by shortening it. When there was no possibility of getting an achromatic objective, his thoughts, guided by Dr. Smith's celebrated treatise on Optics, turned naturally to the idea of a reflecting telescope. We learn from his sister that he wrote to enquire the price of a reflecting telescope of seven feet, that is to say, with an aperture of about six or seven inches ; but the price asked was so much beyond his means that he determined to make his telescope with his own hands. Those who have always been curious to know how Herschel learned all the tricks of the process of grinding, figuring, and polishing mirrors will read with interest the chapter of Dr. Dreyer's introduction which tells what he has been able to find in the journal and papers.

Herschel's sister has recorded that 'about this time he 'bought of a Quaker resident in Bath, who formerly had made 'attempts at polishing mirrors, all his rubbish of patterns, tools, 'hones, polishers, unfinished mirrors, etc., but all for small 'Gregorians, and none above two or three inches diameter.' The now published journal completes the story thus :

'I was informed that there lived in Bath a person who amused himself with repolishing and making reflecting mirrors. Having found him out he offered to let me have all his tools and some half-finished mirrors, as he did not intend to do any more work of that kind. The 22nd September when I bought his apparatus, it was agreed that he should also show me the manner in which he had proceeded with grinding and polishing his mirrors, and going to work with these tools I found no difficulty to do in a few days all what he could show me, his knowledge indeed being very confined. About the 21st October I had some mirrors cast for a two feet reflector, the mixture of the metal was according to a receipt I had obtained with the tools. It was at the rate of 32 copper, 13 tin, and one of Regulus of Antimony, and I found it to make a very good, sound, white metal. In the beginning of November I had other mirrors cast, among them was one intended for a 5½ feet Gregorian reflector, and as soon as they were ground and figured as well as I could do them, I proceeded to the work of polishing. About the middle of December I got so far as to give a tolerable gloss to some of the metals, and having advanced considerably in this work it became necessary to think of mounting these mirrors.'

We think that in some respects this passage is the most valuable of all that have been extracted from the journal by the editor of these papers. Amateur mechanics know the difficulty of starting on an operation of an entirely new kind, for which every one of the numberless small requirements has to be collected from all kinds of sources. Succeed in getting through the process once, on however small a scale, and progress is relatively easy. Doubtless the Quaker's rubbish of patterns, etc., looked a sorry collection to the eye of Caroline the housekeeper, and the Quaker's knowledge was as small as it seemed to Herschel's recollection in after years ; but they gave him the opportunity of making a start, and astronomy owes much to the chance which produced, at the right moment, a mirror-maker ready to dispose of his humble tools and his humble knowledge.

The process of grinding and polishing mirrors remains to this day very empirical. A convex grinding tool of the desired curvature is worked backwards and forwards over the mirror disc, while emery of successively finer grades is introduced between the tool and the mirror. The particles of emery are held, or partly embedded, in the soft tool, and thus are enabled to cut away the harder mirror in much the same way that a soft disc of tin plate, charged with diamond dust, makes a circular saw to cut slices of the hardest rocks. Herschel worked the mirror on the tool ; in modern practice the tool is worked on the mirror. In either case the nicety of figure given to the mirror depends entirely on the nature and range of the stroke, and the proportion between the sizes of the mirror and the tool. The first aim of the grinder is to produce a true spherical surface ; the excessively small alteration which transforms it into the ultimate paraboloid is achieved by a small change in the character of the stroke. The effect of any alteration cannot be predicted except by a kind of acquired instinct, and cannot be determined except by optical tests; for the difference between a good figure and a bad is much smaller than can be measured by any mechanical means.

For fifteen years Herschel operated his grinding and polishing tools by hand, and hand-working does not allow of any exact knowledge of the various component parts which make the whole stroke. Of systematic methods in testing he had none. He made great numbers of mirrors, keeping a careful record of the work done on each. As they were finished he put them in the telescope tube, and tried them on fine print set up at a distance. The best were reserved for the more delicate test on stars ; the rest went back to the workshop for refiguring. In this purely tentative way he gained by slow degrees a feeling for the kind of stroke that would produce the desired change, a skill entirely personal which could not be communicated to another.

With increase in the size of the mirrors with which he worked it was no longer possible for the strength of a single man to give to the grinding and polishing tools the necessary steady and long-continued motions. It became essential to invent machinery for the purpose. A paper sent to the Royal Society in 1789, but for some reason never published until now, gives an excellent account of the difficulties in hand polishing the second mirror for the great forty-foot telescope, which compelled him to have recourse to machinery.

'The idea of a machine was now again as it were forced upon me ; when I considered, that all the essential part, I had formerly taken in the construction of a speculum, was fairly excluded in the present operation. The enormous weight, of about five and twenty hundred pounds, to be moved upon the polisher, would not permit the use of those delicate touches of the hand, by which I had been accustomed to form small mirrors ; and I found myself reduced to the situation of merely directing the unwieldy manœuvres of a set of men, who when they did their best, could only act like a very imperfect machine.

'As soon as I perceived that I was in fact already working with a machine, there wanted not much to convince me that twenty men made a very bad one ; and that I should find no manner of difficulty in contriving another, that would do the work much more to my satisfaction. This point being brought home to me with such forcible arguments, I caused all my apparatus for polishing with the twenty-men-machine to be pulled to pieces that I might never be tempted to use it again ; and began now to consult the very compleat theory of polishing, which long experience had furnished me with.'

The construction of the polishing machine was accomplished at the end of 1788. Within a few weeks its peculiarities were mastered, and a larger machine was made to fashion the four-foot mirror of the great telescope, which in the following August celebrated its perfection by the discovery of a sixth satellite of the planet Saturn.

It is somewhat remarkable that the editor does not anywhere inform us whether any part of the 'very compleat theory 'of polishing' is to be found in the four volumes of the 'Ex- 'periments on the Construction of Specula,' recording no less than 2160 separate pieces of work performed on specula between the years 1773 and 1818. Perhaps the system was never committed to writing, but remained the secret of the brain which devised it, until in the last years some part of it was communicated to Sir John Herschel to enable him to continue in the Southern Hemisphere the work that his father had done so completely for the Northern.

Dr. Dreyer goes so far as to suggest that Herschel disclosed nothing of his methods because he considered them a lucrative trade secret not to be revealed. It is true that he received considerable sums of money by the sale of his instruments. A twenty-five-foot instrument for the King of Spain brought in £3150; and he gives a list, written from memory, and admittedly incomplete, which totals no less than £14,743. But it seems to us very doubtful if the proceeds of selling telescopes did more than defray the cost of maintaining in activity the workshops which were essential to Herschel for the prosecution of his own work. Undoubtedly an account of his methods would have been very valuable to Lord Rosse and to Lassell, who were forced to re-invent for themselves much of what Herschel might have told them. Yet we must remember that the most difficult part of the process was incommunicable in writing; and those who would complain that the secret was not handed down should perhaps enquire whether any would-be telescope maker applied in vain to Sir John Herschel, who was possessed of all that could be transmitted of his father's art.

The surprising activity of the years between 1773, when Herschel began to make mirrors, and 1789, when he finished the great telescope, makes it far from easy to gain a clear idea of the relation between the various telescopes which he employed and the principal researches he carried through. One fact should be kept clearly in mind: the work actually done with the great telescope was relatively small. This telescope discovered the sixth satellite of Saturn, but there is no record that it was turned on to the two real and the four supposed satellites of Uranus, except for three observations on those bodies in the second paper. Very few observations of nebulæ were made with it, and none of the systematic work on those objects, which remains the most valuable and the least surpassed of all Herschel's work. It is worth while to enquire what explanation there can be of the little use to which the masterpiece was put. Dr. Dreyer deals pretty fully with this question in his introduction, putting forward several interesting suggestions which may partially solve the difficulty.

In the first place, the mirror was made of an alloy much poorer in tin than the metal used for Herschel's other specula.

Several attempts to cast with the usual mixture were unsuccessful, the speculum cracking during the cooling. The addition of more copper resulted in a successful casting, but the success was dearly bought, for the mirror tarnished very quickly, and its polish required frequent renewal.

The great disadvantage of metal specula is that repolishing means refiguring, so that the most delicate and tedious part of the whole process of mirror-making has to be repeated at each repolishing. This in itself must have made the great telescope a very exhausting instrument, especially when we remember that the speculum weighed a ton, and that Herschel had no more effective means of testing its figure than hoisting it into position in the tube and trying it on terrestrial objects or on stars. The method now used, of arranging the polishing machine so that the mirror can be turned up and tested optically at any stage of the process, was at that time quite unknown; the process devised by Foucault came half a century later.

A second reason that may account for Herschel's general disinclination to use the great telescope was its unhandiness. Two men were required to turn it, and it is likely that after it had stood in the weather for a few years it was excessively hard to move. Moreover, the longer the telescope the greater the necessary movement of the observer to keep himself at the eyepiece while the telescope follows the star in the field, the more running up and down long ladders, the greater fatigue. Herschel himself has stated that he made it a rule never to employ the great telescope when it was possible to use a smaller; and it seems to be likely that he was often very dissatisfied with the performance of the large mirror. We may, indeed, say with certainty that the mirror could rarely or never have done justice to itself, for its support was totally inadequate to preserve its figure.

The great telescope was in fact too far in advance of the mechanical possibilities of the time, and it came rather too late in its maker's life. The extraordinary strength and determination which allowed him to observe all night after an exhausting day's polishing cannot have remained unimpaired as he passed middle age. The man who ate with a pencil in his hand and a project in his head was the kind of man to exhaust the hardiest constitution; not even Herschel escaped the fate of those who systematically overtax their strength. His sister tells continually of the strain imposed on him by the visits of parties from the Court, who, like less exalted visitors in more modern times, consider that the office of astronomer is created to entertain them. On one evening in particular, the 14th of October 1806,

'his nerves received a shock of which he never got the better afterwards; for on that day he had hardly dismissed his troop of men when visitors assembled, and from the time it was dark till past midnight he was on the grass-plot surrounded by between fifty and sixty persons, without having had time for putting on proper clothing, or for the least nourishment passing his lips. Among the company I remember were the Duke of Sussex, Prince Galitzin, Lord Darnley, a number of officers, Admiral Boston, and some ladies.'

From this time we read of illness, of his life being despaired of in the following spring, of the necessity for holidays, and of his strength not being equal to the labour required for polishing forty-foot mirrors. Very late in his life he received the official recognition of Knighthood in the Guelphic Order, thirty-four years after he had been made the royal astronomer, and long after most of the learned bodies of the world, with the strange exceptions of the Universities of Oxford and Cambridge, had enrolled his name in their most honourable places. There is little in the now published journals which does more than confirm the account of his later years which his sister Caroline gave in her Memoirs, justly celebrated and widely known. One personal detail we believe is new: the astronomer was naturalised an Englishman in 1793 under the name of William, his baptismal name being Friedrich Wilhelm; which fact we commend to the notice of pedantic cataloguers, who are fond of giving his name as Frederick William.

When we turn from the history of his life to examine the collected papers of this great man, we find no difficulty in selecting those which not only justify but demand republication.

The papers which are of real present value are those dealing with the 'Construction of the Heavens.' The phrase was one of those which displeased this Review in its first volume: and was described as an example of that 'idle 'fondness for inventing names, without any manner of 'occasion . . . and a use of novel and affected idioms.'

The first paper in which this offending phrase occurs contains two ideas which determined the direction of speculation for many years: the probability that all the nebulæ might be resolved into stars with an instrument of sufficient power; and the possibility of explaining the galaxy as an aggregation of stars in the shape of a cloven disc. These ideas survived for long after their author had seen that they must be profoundly modified if not abandoned. The most considerable difficulty in forming a just appreciation of Herschel's work arises from the persistence with which these early speculations have been represented as his mature conclusions. In a summary review of the long series of papers devoted to the construction of the heavens we shall do well to confine our attention so far as possible to tracing the development of these two themes: the nature of the nebulæ and the form of the galaxy of stars.

Messier's catalogue of nebulæ described a great number of these objects as 'nébuleuses sans étoiles,' which under the powerful scrutiny of Herschel's telescope were resolved into stars; and many others showed a kind of mottled appearance, which suggested resolvability. Thus arose the theory that all nebulosity might be resolved into stars were the optical power sufficient for its minute examination. It was a probable view, which turned out to be wrong, and Herschel very soon saw that it was wrong. His successors, less acute, wasted much time and effort in trying to maintain the position, which could not be maintained in its entirety, but from which they were not driven till the invention of the spectroscopic method some fifty years ago. It will be interesting to follow the steps by which Herschel convinced himself that his first position was not tenable. As to the form of the galaxy, its most conspicuous feature is the rift which cleaves it in two through the constellations of Cygnus and Aquila. The general line of the galaxy is so nearly a great circle of the celestial sphere, and so little different in brightness at opposite points, that one can hardly avoid the conclusion first stated by Kepler, that the Sun is not far from the centre of the galaxy. The breadth of the galaxy is not so diverse in its various parts as to lead us away from the obvious idea of a flat stratum of stars, containing the Sun, and in its further parts forming the Milky Way. Some such working hypothesis was inevitable; the merit, such as it is, of enunciating it first seems to belong to Thomas Wright, of Durham, who published in 1750 his 'Original Theory or 'New Hypothesis of the Universe, Founded upon the Laws of 'Nature, and solving by Mathematical Principles the General 'Phænomena of the Visible Creation; and particularly The 'Via Lactea. Comprised in nine familiar letters from the 'Author to his Friend.'

This simple and plausible working hypothesis was adopted from Wright by Kant, whose 'Allgemeine Naturgeschichte und 'Theorie des Himmels,' published in 1755, contains most of the ideas which have been the stock-in-trade of cosmogony, and especially of the vague and grandiose kind of cosmogony, from Kant's day to ours. Kant found an analogy between the nearly plane arrangement of the Solar System and the nearly plane system of the stars by which he would explain the galaxy. He argued for the existence of a central Sun; and he supposed the few nebulæ known at that time were external galaxies. These galaxies in their turn might be units in a system of galaxies and so *ad infinitum*.

Somewhat similar, and equally grandiose, were the theories put forward by Lambert, to whom indeed this kind of theorising is generally ascribed. His views differ from those of Kant in one important respect; he introduces one additional step between the Sun and the Milky Way, by supposing that the Sun and the stars about him form a system of the 'second 'order'; and that many of these systems of the second order, ranged more or less in one plane, form the Milky Way, a system of the third order. Thence he proceeds to systems of higher orders, on the same lines as those of Kant.

It is clear that Herschel need have been in no want of hypotheses on which to try his facts—hypotheses built on speculation with a mere ha'porth of visible capital behind. The working hypothesis which he adopted was the simplest, that of Wright's disc, but with the modification that to account for the bifurcation of the Milky Way he supposed the disc cloven in such a way that the Sun lay in the angle of the cleft. The resolvability of the nebulæ into stars, and the cloven disc theory of the Milky Way, appear in that first paper which we mentioned before undertaking this little excursus on the speculative cosmogony of the time. It will now be our task to trace the steps by which he strengthened his grasp of the facts and loosened his hold on the speculative hypotheses until they fell away discarded.

The famous method of star-gauging, propounded in the first paper of June 1784, is fully described in a second paper which followed the first within a few months. Observationally it consists in nothing more than counting the number of stars visible in the field of a certain eyepiece of low power on the reflector of eighteen inches aperture and twenty feet focal length. Granting that the stars are distributed uniformly in space and ignoring any consideration of the apparent brightness of the stars which are counted, it is possible to estimate the depth to which the view has penetrated from the number of stars that have been seen. Working on this assumption, and taking as his unit of distance the distance of Sirius from the Sun, Herschel calculated that when there were ten stars in the field the 'visual ray' extended to 127 units, and when there were 500 stars it reached to 471 units. By a selection from the first list of 683 gauges, the greater part of which depended on accounts in ten consecutive fields, he constructed the well-known figure of the section across the universe, in which its greatest extension is shown as some five times its least, and the cleft is well marked. Now in view of the importance which has, quite unjustifiably, been attached to this result, as the presumed expression of its author's matured views, it is essential to remark that Herschel from the very first was fully conscious of the insecure character of his premisses. He is careful to preface the enunciation of his problem with the words :

> 'The stars being supposed to be very nearly equally scattered . . . ' ; and later : 'I would not be understood to lay a greater stress on these and the following calculations than the principles on which they are founded will permit ; and if hereafter we shall find reason, from experience and observation, to believe that there are parts of our system where the stars are not scattered in the manner here supposed, we ought then to make proper exceptions.' And finally we read : 'This subject being so new, I look upon what is here given partly as only an example to illustrate the spirit of the method.'

The distinguished author could not have been more careful to guard against misunderstanding of his position in regard to this method of star-gauging ; yet he has in fact been persistently misunderstood. For this it would seem that his son, Sir John, is responsible. He reproduces, in his 'Outlines of Astronomy' the figure of the cloven disc; he describes the method of gauging and discusses the interpretation of the gauges without any indication of the fact that they cannot be applied to the purpose for which they were devised; he describes how he himself has extended the application of the method to the Southern Hemisphere. It is true that he shows in many places that he no longer relies on the original assumption of a nearly uniform distribution of the stars. He admits the existence of discontinuous masses and clouds of stars in the Milky Way. Yet he cannot be acquitted of a singular blindness to the complete change of view which his father expressed quite clearly in his later work. The paper of 1785 gave the results of 683 gauges. Over 400 more were made, but were never published until 1884, when Professor Holden, then Director of the Washburn Observatory at Madison, U.S.A., obtained a copy of them from the Herschel family, and printed them in the second volume of the publications of his observatory.

It is clear enough that Herschel did not publish these results because he saw very soon that they could not be used in the manner that he had intended. He does not make use of the star gauges in his later papers, and it is exceedingly unfortunate that the method of star-gauging and the cloven-disc theory of the Universe which resulted from it should have been so often placed in the front rank of his performances. We find in his papers the most conclusive evidence that they should be treated rather as early indiscretions of which he soon repented, and which in after life he unhesitatingly and unequivocally abjured.

Let us turn to his further investigation of the nebulæ, restricting ourselves of necessity, for the subject is immense, to our purpose of tracing the after history of the original supposition, that with an instrument of sufficient power all the nebulæ could be resolved into stars. Already in the second paper we find that he has distinguished between three different kinds of nebulosity: the resolvable, the coloured but irresolvable, and a tincture of the milky kind; yet it is clear that he still regards these three distinctions of appearance as caused by differences of distance, and he has not yet found reason to abandon the idea that all nebulosity is due to multitudes of stars.

'To the inhabitants of the nebulæ of the present catalogue (he writes in 1786) our sidereal system must appear either as a small nebulous patch ; an extended streak of milky light ; a large resolvable nebula ; a very compressed cluster of minute stars hardly discernible ; or as an immense collection of large scattered stars of different sizes. And either of these appearances will take place with them according as their own situation is more or less remote from ours.'

During these years Herschel's observing activity was at its highest. In 1789 a catalogue of a second thousand nebulæ appeared, and then for a while, on the completion of the great telescope, the sweeps for nebulæ were laid aside. The discovery of Saturn's sixth and seventh satellites, and the long series of measures consequent on this discovery, absorbed him for a year ; in 1791 he returned to the nebulæ with a fresh mind, and immediately there comes a result of capital importance which marks the turning-point of his views. He discovered a star of about the eighth magnitude surrounded with a faintly luminous atmosphere of considerable extent. The paper in which this is announced, 'On nebulous stars, properly so 'called,' exhibits in perfection Herschel's masterly lucidity in the discussion of his material. The new object appears to be of a very instructive nature such as may lead to important inferences. He reviews the steps by which he has passed from the Milky Way, consisting entirely of stars, to clusters in which the stellar points were smaller, yet still clearly distinguishable, and so to nebulous spots in which no trace of a star could be seen. But then the gradations from the former to the latter were by such well-connected steps as left no room for doubt that all these appearances were occasioned by stars.

'When I pursued these researches I was in the situation of a natural philosopher who follows the various species of animals and insects from the height of their perfection down to the lowest ebb of life ; when, arriving at the vegetable kingdom, he can scarcely point out to us the precise boundary where the animal ceases and the plant begins ; and may even go so far as to suspect them not to be essentially different. But recollecting himself, he compares, for instance, one of the human species to a tree, and all doubt upon the subject vanishes before him. . . . A glance like that of the naturalist, who casts his eye from the perfect animal to the perfect vegetable, is wanting to remove the veil from the mind of the astronomer. The object I have mentioned above, is the phænomenon that was wanting for this purpose. View, for instance, the nineteenth cluster of my sixth class, and afterwards cast your eye on this cloudy star, and the result will be no less decisive than that of the naturalist we have alluded to. Our judgment, I may venture to say, will be, that the nebulosity about the star is not of a starry nature.'

The star is involved in a 'shining fluid, of a nature totally 'unknown to us.' Granting the existence of such a fluid, he asks himself if it may not exist apart from stars? Has it been too hastily surmised that all milky nebulosity, of which there is so much in the heavens, is owing to starlight only? Can one not explain the Orion nebula much better by this luminous fluid than by the clustering of stars at a distance? Seventy years afterwards Huggins' spectroscope showed that these milky nebulæ are gaseous.

Within the limits of a review it is not possible to follow out in detail the change that came over Herschel's opinions when he had passed the turning-point of the year 1791. For a time he wrote little on the construction of the heavens. Then in 1802 he published the final catalogue of new nebulæ and clusters; and it is to be noticed that after 1790 he observed very few clusters. The short introduction to this concluding section is important for its indications of gradually changing opinions.

'Though our sun, and all the stars we see, may truly be said to be in the plane of the milky-way, yet I am now convinced, by a long inspection and continued examination of it, that the milky-way itself consists of stars very differently scattered from those which are immediately about us.' Or again: 'The stars of which it is composed are very unequally scattered, and shew evident marks of clustering together into many separate allotments.'

The 'sweeps' were now finished. From the year 1802 Herschel devoted his nebula nights, when the moon was absent, to the critical examination of the more important objects that had passed before him rapidly in the strenuous hours of sweeping. In 1811 he published a paper which is, in our opinion, the most masterly of all his works: 'Astronomical 'Observations relating to the construction of the Heavens, 'arranged for the Purpose of a Critical Examination. . . .'

With the *expertise* of a collector at his cabinet, Herschel selects and arranges his objects, turns them over and criticises them, marks this as a perfect example of its class, and that as doubtful, perhaps spurious. His unrivalled familiarity with the appearance of each object in the collection gives to his *catalogue raisonné* an authority as yet not seriously challenged. During the century that has passed since this paper of 1811 was written the spectroscope has divided the nebulæ into gaseous and apparently non-gaseous categories; photography has given us nebula pictures of wonderful beauty, marvellous intricacy of form. Lord Rosse's discovery of the spiral form of a few nebulæ has been followed by the striking, though not yet fully substantiated, proposition that the greater part of the nebulæ are spirals. Barnard's exquisite pictures of the Milky Way have revealed to us the importance of the star clouds and the vast extent of the milky light in the regions affected with nebulosity. Yet there is no modern work whatever that has any pretensions to rival Herschel's careful classification and discussion in his paper of 1811. The fact is so remarkable that we must venture on a short examination of its cause.

Our theory, which may appear at first sight paradoxical, is this: The trouble is due to the 'General Catalogue of Nebulæ,' published by Sir John Herschel in 1864, and to the more modern 'New General Catalogue of Nebulæ and Clusters of Stars,' published in 1890 by the distinguished astronomer and scholar who has edited the Collected Papers. We shall not be suspected of wishing to minimise the value of the second indispensable work, the most widely used of all the publications of the Royal Astronomical Society. But we believe that this catalogue has had an unforeseen and unfortunate effect. In it every nebula known up to the end of the year 1887 is given a single line, a number, a position, and a shorthand description. Since its publication we have remarked a strong tendency to suppose that when you have allowed a nebula to have an N.G.C. number and a place in the sky, you have done everything for it that may reasonably be expected. First there comes the nebula cartographer who works on the principle: one nebula, one spot on the chart. Later there comes the wielder of modern statistical apparatus, who says in effect: Here is a jolly lot of figures very nicely arranged to save us the trouble of any enquiry into their meaning; let us analyse them into half a dozen orders of spherical harmonics, and we shall discover laws of nature which would never have been visible to the old-fashioned cartographer, and how much less to a man like Herschel who worried himself with the individuality of each object as if it had a character and personality of its own.

There is a real danger that these modern statistical enquirers may, as Artemus Ward puts it, 'throw so much darkness on 'the subject that we shall soon know nothing about the 'early Saxons,' or other subjects of enquiry. A pioneer of modern statistics has proved that all the skulls of a certain Egyptian collection belonged to a single race of people; the anatomist replies pathetically: I cannot understand your mathematics, but if you look at the skulls you can *see* that they belong to two distinct races. The mathematician constructs an expression to represent the relief of the Earth's surface; the geomorphologist is respectfully doubtful about the propriety of squeezing new mountain ranges and old ocean beds into a single formula.

In all such enquiries there is need for a personal critical faculty which is beyond the realm of statistics. The scenery and construction of the heavens are as characteristic, as locally complex and significant, as the scenery and the condition of diverse regions of the Earth. For a century past astronomy has had specialists who have developed their own special branches of enquiry to an extraordinary degree, but the man has not yet appeared who can write a natural history of the sky as Herschel did for his time.

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