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ASTRONOMY

By ARTHUR R. HINKS

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CHAPTER I

ASTRONOMERS AND OBSERVATORIES

In a strictly limited work upon a quite unlimited subject we are compelled to exercise a strict economy. However tempting it may be to enquire what knowledge of astronomy existed among the ancient people of the world, how it comes about that their buildings are built astronomically, or why it was that their considerable skill in the science was so nearly lost to future generations—we must refuse to be drawn into the charming but devious paths which lead through these parts of our subject. The Chaldæan shepherd had a far better knowledge of the stars and their movements than the modern educated dweller in a glaringly-lit town, with a cloudy and smoky sky; he had a traditional lore of the stars which enabled him to regulate his husbandry and to find his way; and his religion, his legends, and perhaps some of his early history can be traced in the names which he gave to the groups of stars which were familiar to him.

The very natural association of religious ideas with the heavenly bodies led to the building of temples with a definite astronomical significance, which we can trace in the Temple of the Sun at Luxor, in Stonehenge, and in many other buildings of antiquity. Let us be careful, however, not to go too far in finding an astronomical meaning for every line and for every angle in such a building. At the present time there is a decided tendency to go further than strict prudence would permit; to ascribe to the builders of old a knowledge and an accuracy which the facts do not warrant; and to pretend by measurement and calculation to arrive at the date of the building with all the authority which should attach to the solution of an astronomical problem. We shall do well to receive all such results with caution; they are interesting archæology, but their astronomy is questionable.

It must appear curious at first sight to any reader of the history of astronomy, that while many of the principal facts, the rotundity of the earth, the gradual change in the position of the pole among the stars, were known to the famous astronomers of antiquity, who were Greeks, yet the influence of the Greek philosopher Aristotle was for many centuries sufficient to stifle any spirit of enquiry into the truths of astronomy. That their teaching was contrary to Aristotle was enough to condemn Copernicus or Galileo. Why was this particular Greek of authority so much greater than Hipparchus or Ptolemy? The answer is not hard. These Greek astronomers lived not in Greece, but at Alexandria, whither learning migrated as the famous Greece of history decayed. Their science was hardly known to the Romans, but such of it as survived was preserved by the Arabs and came to Europe by the Moslem invasion of Spain, tainted no doubt in public opinion with the discredit attaching among the Christians to all things Moslem. Thus it affected at first but slightly the revival of interest in learning and science which took place in Europe in the fifteenth and sixteenth centuries. The first astronomers of Europe had to work against, not with the support of, whatever remained in repute of the ancient learning of Greece. At every turn they were stopped with the objection that Aristotle said so and so.

Now what Aristotle said was founded upon the vaguest kind of foundation. Planets must move in circles because the circle is the only "perfect" figure. Seven is a perfect number, and therefore if you have found seven of a thing you need not waste time looking for an eighth. This was the kind of argument that passed for scientific in the middle ages; this was the kind of prejudice which the early European astronomers had to rid their own minds of first, and then to banish from the minds of others. It could be done in only one way, by insisting on first observing what actually happened and then constructing arguments to fit the facts. Strange as it may appear, this very sensible line of conduct was quite foreign to the ideas of the time, and was received with very bad grace by those in authority.

Foremost among the men who insisted upon observation and experiment was the Danish nobleman Tycho Brahe, who was above all things an experimenter. On the little island of Hven, lying off Elsinore in the Sound between Denmark and Sweden, Tycho built a magnificent observatory, and fitted it with splendid instruments all of his own design and make. He founded in fact the modern science of observational astronomy. In the Royal Library at Copenhagen one may see the great journals of observations which Tycho and his assistants made for some twenty years in that island—catalogues of stars, places of the planets and of the Moon, places of comets, and all the regular routine of a modern observatory. There are two things to be remembered about Tycho's work; it was immensely more accurate observation than had ever been made before, and it was done before the telescope was invented.

One is so accustomed to think of an astronomer as placed at the eye end of a telescope, that it seems extraordinary that much could be done without it. True, Tycho was limited to the stars and the planets which the unaided eye can show, but there was much to be done with these which had never yet been done. Instruments mounted on pillars, furnished with circles or with portions of circles divided into degrees and parts of degrees, provided with carefully made sights, were capable of considerable accuracy in the hands of a man with a genius for handling instruments. At least they were capable of providing the mass of observations upon which Kepler founded his laws of planetary motion, and upon which in the end Newton rested his demonstration of the principle of universal gravitation.

But we are going too fast. Tycho Brahe was an admirable astronomer, justly considered the greatest that ever lived; but he was not quite so admirable as a neighbour. It seems that a haughty spirit and a high-handed manner of disregarding his public duties were responsible at least in part for the catastrophe which overtook him. His revenues were withdrawn; he was forced to leave Denmark; and so he came to Prague, and Kepler became possessed of his observations, with the great consequences to astronomy which we have briefly mentioned. "Had Tycho remained in his island," said a famous historian of astronomy, "perhaps we should still have been ignorant of the true system of the universe."

But a few years after his death in 1604 the telescope was invented in Holland. There are several claimants to the invention, and their respective merits are somewhat obscure. Nor is it quite certain that posterity has been right in giving to Galileo the undivided credit of first turning the telescope to the skies. His famous discovery of the four satellites of Jupiter led to his immediate recognition of the truth they demonstrated, that the earth and the planets move round the sun, as Copernicus taught. The persecution which he suffered, for teaching his opinions, threw a romance about his life which obscured the fact that the injustice of others made him unjust himself. A German astronomer, Simon Marius, had himself discovered the satellites of Jupiter within a few hours of and probably before Galileo's discovery. He had made many observations of them and constructed a first rough table of their motions. Galileo roundly accused him of stealing his observations and his credit, and the world, believing Galileo, went on calling Marius an impudent pretender until two or three years ago. So difficult it is to be just in assigning credit to rival discoverers.

The first discoveries of the telescope opened a new heaven to men's eyes. There were mountains in the moon, spots upon the supposed unblemished surface of the sun; the planets became worlds like the earth, and the Milky Way was resolved into innumerable stars. Astronomers of all countries set themselves to improve the telescope, and up to a certain point their progress was excellent; but at that point they stopped. For technical reasons, into which we can hardly enter, those who aspired to make their telescopes more powerful were forced to make them inordinately long; and such instruments, though they were fairly satisfactory for viewing the planets, were very little good on stars. Hence we find in the 150 years after the invention of the telescope that little progress was made in exploring among the stars and nebulæ.

In other directions, however, immense advances had been made. Newton at Cambridge had made his immortal discovery of the principle of universal gravitation; Halley in London had spurred him on to complete his work and publish it. On the continent of Europe mathematicians had opened up regions of mathematical analysis, in which Newton's principle was the foundation, though his methods were superseded by others more tractable in ordinary hands. It is a remarkable fact that no one since Newton has succeeded in doing much by his methods. Everywhere the telescope had been applied to the instrument of measurement, with great gain in accuracy. Bradley had discovered the important principles of the aberration of light and the nutation of the axis of the earth—principles which at once reduced to order all the questions connected with the accurate determination of star places in the sky. The foundations of the astronomy which concerns itself with minute nicety of observation, with profound depths of calculation, had been well and truly laid. Upon that foundation generation after generation of mathematicians and of observers have built; with hardly any pause the edifice of precise astronomy has arisen, stone by stone, till to-day it forms the immense structure into which it is hard to enter without the master key of some mathematics and a great deal of technical knowledge. All this side of astronomy lies outside the range of a small book like the present. We shall make some acquaintance with its results in the chapters which follow; with its methods and with its technique we cannot concern ourselves further.

Upon the other side of astronomy lies the realm of exploration and discovery, whose frontiers receive a great extension with every considerable advance in instrument building or new method of observation. Such an extension was made by Sir William Herschel. The reflecting telescope which Newton had invented made at first slow progress in size or in use. It remained an interesting example of optics rather than an instrument of any practical importance, until Herschel found himself at last in the position to carry out his cherished scheme. What it was that implanted in Herschel's brain the resolution to explore the sky is of course unknown. All that we do know is that during years of hard work as a teacher and conductor of music he steadily kept in sight the purpose, that he would make greater telescopes than any which had yet been made, and that he would thoroughly search the heavens. It was natural that his thoughts should turn to the reflecting telescope, whose construction, though tedious and delicate, is yet relatively simple and within the power of a skilled amateur—as has often been proved since Herschel's time. Of the way in which Herschel made his telescopes very little is known, or at least very little has been published. The observations which he made with them are the foundation and to a considerable extent the structure of what we know to-day about the nebulæ and star clusters. In the chapter dealing with the nebulæ we shall see how the survey of the sky carried from pole to pole by Sir William and his son Sir John remains to this day complete and unsurpassed.

The first half of the nineteenth century was again a period of steady rather than of startling progress. Bessel in Germany, Airy in England, systematized and organized observatory work so that the methods of to-day bear the indelible stamp of their talent. Adams in England and Leverrier in France discovered Neptune by calculation—a feat which produced an impression more profound than any other event in the history of science. In Ireland Lord Rosse made in his own workshops the gigantic six-foot mirror which remains up to the present day unsurpassed in sheer size. Then just after the middle of the century came the invention of spectrum analysis by Kirchoff, and its application to the stars and nebulæ by Huggins.

This discovery marks an epoch as distinctive as, perhaps more distinctive than, the discovery of the telescope, for it gave to astronomers a new sense. The telescope is but an eye allowing man to see farther and better than he could see before. The spectroscope enables him to achieve what might have seemed perfectly hopeless, to determine what the stars are made of by analysing the light which they send us; and to determine the speed with which they are approaching or receding in the line of sight.

With this new sense at its command astronomy made at once a brilliant advance into regions which had been closed to all examination. It was immediately clear that the stars are suns like our sun, while many of the nebulæ are but clouds and drifts of shining gas. It was at once possible to prove that the substances, common or rare, which are found in the crust of the earth are present as glowing vapours in the distant stars, and that from end to end of space matter is essentially the same. Some light rays there are, indeed, in the nebulæ or in the stars which have not yet been run to earth. The brilliant discovery of helium by Ramsay gives us reason for hoping that a knowledge of the stars may in time show us how to look for and to find other substances which are all the while terrestrial, though we have not yet recognized them.

The application of photography to astronomy followed hard upon the introduction of light analysis. But for all except the brightest objects the necessary exposures were so long that a wet collodion plate could not deal with them; the plate dried during the exposure and lost its sensitiveness. Real facility in astronomical photography came with the introduction of the dry gelatine plate.

We may perhaps spend a few moments in considering exactly how the photographic plate helps the astronomer, for there is much misconception on the subject. The old-fashioned astronomer was pictured in imagination with his eye glued to the telescope—the adhesive was always used—waiting and watching to discover something new. The modern astronomer is supposed to glue a photographic plate in the place of his eye, and to sleep in confidence that the plate on development in the morning will show him everything that he might have observed in the night. But there is not much truth in this picture. Respectable photographs cannot be taken without attention. The astronomer is differently occupied nowadays, but his lot is not made much easier; to watch a faint guiding star on the cross wires of the guide telescope, during an exposure of many hours, is a most exhausting business.

The real advantages of the photographic plate lie in two of its properties: its power of storing up impressions until they become developable; and its power of recording simultaneously a number of things which could be observed only one by one. To these we should add its power of taking in a very much larger field of view than can be seen at one time in the telescope. The first and the last are responsible for the great success with which the star camera portrays star clouds and very extensive nebulæ, which cannot produce their proper effect when they are seen piecemeal. The second and the last have revolutionized many branches of precise astronomy by transferring the work of measurement from the micrometer on the telescope to the measuring machine in which the negative is placed. This has great advantages; among them are two which can be appreciated at once. It economizes time, because one can take advantage of an hour of clear and steady sky to take a photograph which will provide work for a week of cloudy weather. And secondly—a more technical reason—it greatly facilitates the troublesome corrections which have to be applied to the measures, because they can be calculated and applied all at once, instead of one by one.

The requirements of photography have made new and very interesting demands upon the skill of the optician and the mechanic. The optician is called upon to design lenses which shall give the greatest concentration of light, for photographing faint objects; and the best possible definition over a large field of view, for photographing extensive objects like the star clouds of the Milky Way. And what passes for good definition in a terrestrial camera turns out to be very bad definition when it is a question of photographing a star far from the centre of the field. One wants the minute image, hardly visible on the plate without a glass, to be quite round and quite free from wings and from surrounding glare. In spite of long calculation and trials, it is not as yet possible to obtain a satisfactory picture more than about ten degrees across.

The mechanic is required to make long telescopes without flexure, and to make driving clockwork that shall cause the telescope to follow the daily motion of the stars without allowing them to wander by ever so little upon the photographic plate. One such device never fails to fascinate the visitor to an observatory—the electrically controlled clockwork devised by Sir Howard Grubb, of Dublin, and fitted to many modern British instruments. A pendulum in a corner of the telescope house has nothing to do but to send a current every second to the clock. This current detects whether the clock has gone fast or slow, and if the error amounts to one-fortieth of a second of time, it is automatically corrected. The reader will have no difficulty in believing that an apparatus of this delicacy does not always work smoothly. Like all delicate machines, it wants "tuning up" from time to time; and these modern refinements and complications in astronomical instruments put something of a strain upon the astronomer who has to add the technique of photography, of electrical testing, of clock making, and of silvering mirrors to his more old-fashioned professional attainments.

With every increase of size in astronomical instruments the question is asked, Has the limit of successful construction now been reached? Is it possible or profitable to go farther? Such questions cannot receive any positive answer, but it may be worth while to enquire for a moment what prospect there may be of providing the astronomer with greatly increased telescopic power.

The greatest object glass which has been successful is that mounted at the Yerkes Observatory of the University of Chicago. Its diameter is 40 inches; its focal length is 62 feet; and it requires for its installation a dome only two or three feet smaller than the inner dome of St. Paul's in London. It has been in existence some sixteen years, and it remains the largest lens at work. What has put a stop to the series of larger and larger glasses which was made during the last thirty years of last century? There are several reasons.

The first of course is the expense. The cost of mounting one of these big refracting telescopes is immense. The telescope itself increases in cost at a rate out of all proportion to its aperture; the building in which it is housed, the revolving dome and the rising floor, soon come to cost as much as the telescope. Hence there has arrived a time when the increased cost of installation is out of proportion to the gain in light; and while it is impossible to say that the Yerkes telescope marks the limit to construction in that particular style, one can at any rate say that there are no signs at present of any one trying to build a larger

The second reason against attempting to build larger refractors is the extraordinary difficulty in getting large discs of optical glass. The optician could handle a bigger disc than the glass founder can supply. About the business of fashioning these discs there has always been something of a mystery, concerning a secret strictly guarded, in the possession of only two or three persons at one time. Unless a firm was in possession of the secret, it was hopeless to think of making big discs, so the story went; and the man who knew it was paid a princely salary on condition he never went outside the gates—with the inevitable result. Whether or no there is any truth in the story as it is told, it is certain that the manufacture of large discs of optical glass has always been difficult; that not more than three or four firms in all have succeeded; and that at the present time there is only one—a French firm—which has any success.

The difficulty lies, of course, in getting the disc of glass uniform throughout; a patch of density slightly greater than the rest will ruin it, unless the excess can be removed by re-annealing—a process which occasionally succeeds in a remarkable way, which is very curious when one considers that the glass is not, in its last stages, ever raised to anything like melting point. We have spoken, up to the present, of the ordinary crown glass and flint glass. With the more complex kinds of glass success has been still less. It seems to be impossible to produce discs of the celebrated Jena glasses larger than about twelve inches in diameter, and this puts a severe restriction on the optician, who could do ever so much more if he had a larger range of kinds of glass with which to work.

A third reason why no more big refractors have been built is that enterprise has been turned into other channels—the construction of big mirrors for reflecting telescopes, and horizontal or vertical telescopes for solar work. The great centre of activity in such work is at present the instrument shop at Pasadena belonging to the Solar Observatory of Mount Wilson, California. With financial resources which seem illimitable, administered by a director of boundless energy and an engineer of consummate skill, the Mount Wilson Observatory has created an equipment which is extraordinarily fine. The reflector of 60 inches aperture is for the moment the last word in instrument construction; that it will not be so for long is clear from the fact that a 100-inch mirror is already under way, finally to beat the record of size which Lord Rosse's reflector has held for some sixty years. Into the 60-inch telescope the constructor has put every refinement and device that he can think of as possibly useful; the result is a formidable complication somewhat discouraging to the astronomer of strictly limited means, as most astronomers are.

These magnificent instruments can do things which no other telescopes can do, and it is an immense advance that their constructors have made. There is naturally a danger that the owners of smaller telescopes will be depressed, and will perhaps under-estimate the possibilities that they have within their reach if they have courage. There is, as a matter of fact, still plenty of excellent work for the smaller instruments to do—both for the modest observatory and for the still more modest amateur. In various places as we proceed we shall consider what work there is within reach of the amateur observer. For the moment let us return to the public observatory, and glance at its equipment, We shall suppose that it is a University Observatory, well but not extravagantly equipped, and prepared for teaching as well as for research.

First there will be a meridian circle—a telescope of moderate aperture, say 6 inches—mounted upon very solid piers so that it will turn from north to south, through the pole and the zenith, pointing always to the meridian of the place. In the focus of this telescope there is a set of wires, equally spaced about a centre wire which forms the visible instrumental meridian. And the telescope is provided with a divided circle which serves to measure how far it is turned from the zenith, north or south. With this instrument a double observation is made—the time at which a star crosses the meridian, and the distance from the zenith at which it passes. The purpose of the instrument is first to find the time, the correction to the clock from observations of known stars; and then to observe the places of stars which are not so well known, for making star catalogues; or to observe the places of the moon and the planets, for improving the theory of their motions. All this kind of work is called astronomy of position, and belongs to the department of meridian observat on.

Secondly, there will be the department of the equatorial, a telescope mounted and driven by clockwork so that the effects of the rotation of the earth are annulled. In the equatorial the star can be kept in the field of view for as long as may be desired; it does not rush across the field of view and vanish, as it does in the meridian circle. With the equatorial one studies and makes drawings of the planets, measures the relative positions of the components of double stars, or searches for new comets and nebulæ.

Then, more likely than not, there will be another equatorial telescope, with its lens corrected for the photographic instead of for the visual rays of light. Such a telescope is in reality a camera, though different in proportions from the ordinary landscape camera of commerce. The lens will be perhaps 12 inches in diameter; the tube about ten times this diameter in length. There will be no bellows, because the camera is in this case used at a fixed focus, the focus for infinity, and the small range of focusing required to bring the plate into exact adjustment can be provided in other ways; and bellows would be very much out of place in an instrument which requires to be rigid in a high degree. Lastly comes the carrier for the photographic plate, which is perhaps six inches square, not nearly so large as the objective. It is this curious proportion between the lens and the plate which makes the photographic telescope look unlike a camera. One is so much accustomed to seeing an inch of objective covering a photographic plate $8\frac{1}{2} \times 6\frac{1}{2}$ inches, that it seems at first sight absurd to require 12 inches of objective to cover six inches of plate. But we must remember that we want a large objective because we are going to photograph very faint objects; and we use a small plate because we are content only with the centre of the field, where the definition is of the very best.

The photographs taken with this instrument will be intended for accurate measurement. Hence a measuring machine equipped with a microscope, with accurate divided scales, and with micrometer screws, will form part of the equipment of the photographic telescope.

With such an outfit the observatory will be able to undertake a share in the work of charting and cataloguing the stars by photography, which is now being carried out by an international co-operation of observatories.

The photographs taken with this instrument, being intended for accurate measurement, will be on a large scale, with the stars widely separated. They are excellent for their purpose, but they make very poor pictures. The magnificent, densely crowded pictures of the Milky Way which make a splendid show on the screen at a lecture, or in a work on astronomy which can afford illustrations, are made with an instrument differently proportioned, with a focal length only four and a half or five times the aperture, and with an objective of four lenses arranged in pairs, very much like the ordinary high-class camera lens. Then, with an exposure of several hours and a relatively much larger plate, one can concentrate many square degrees of sky into one picture, which looks magnificent, and is very useful for study of the broad features of the sky, but is not good for measurements of precision.

Our observatory will also have a department devoted to astrophysics, that is to say, to the study of stars and nebulæ with the spectroscope. One of the equatorials will be equipped with the apparatus of slit, prism, and camera which photographs the result of the light analysis, the spectrum with its bright lines coming from glowing gas, or its dark lines telling of absorption in the stellar atmospheres. When the spectrum is obtained it requires interpretation; the lines in the spectrum, for example, may correspond in position exactly with the lines which are produced by the vapour of iron, but some may be abnormally strong and others almost invisible. It is then the task of the astrophysicist to discover what this difference means. He turns the observatory into a physical laboratory, equipped with dynamos, electric arcs, furnaces, pressure pumps, and vacuum pumps, and he tries to reproduce upon earth the conditions, indicated by the abnormal spectrum, which are present in the stars.

Again, it may be that a department of our observatory is devoted to solar physics, to the study of those details in our own particular star which cannot be observed in the others because they are so far away. The solar physicist will have quite a different equipment. He has plenty of light available, so that his instruments need not be very great in aperture, and may be very long, to give him pictures upon a large scale. So he will probably have a very long fixed horizontal telescope and spectroscope, into which the sunlight will be reflected by a slowly rotating mirror, the coelostat. Or if he is quite up to date he will decide that a horizontal telescope is too much affected by the radiation from the heated ground, and he will substitute a vertical telescope, with the coelostat and objective mounted on a high tower, and the spectroscope at the bottom of a deep well below. But he will not be able to do this unless he is very rich.

Lastly, our observatory will be equipped with a library and with a staff of calculators, called computers. The library will contain the publications of every other observatory—all of them presented to it; the journals of the astronomical and kindred societies; and the four or five technical journals published by subscription. The bulk of astronomical literature is becoming enormous. There are some hundred and fifty observatories that publish observations more or less regularly;

the journals amount in the total to several thousand pages annually, and it is quite impossible for the astronomer to read everything that appears. Hence there has arisen in astronomy, as in other sciences, the need for carefully prepared and arranged abstracts of each year's work, and several volumes are now devoted annually to this purpose. From this mass of raw material the evidence on any point is gradually extracted and worked up into a case; the case is examined and tried by discussion and controversy, at meetings of the astronomical societies or in the pages of the journals; and in the end years of observation, hundreds of pages of publication, months of calculation and discussion, produce the brief conclusion, for example, that the gaseous nebulæ are concentrated upon the Milky Way.

But we must not overlook the computer, who plays an indispensable part in the observatory. Hardly a measure which is made at the telescope is of any use until it has passed through a process of calculation and correction more or less prolonged. The continued variation in the state of adjustment of the instruments, the refraction of the air, and the changes day by day and year by year in the position of the celestial equator have all to be reckoned with by calculation. Three places, for example, of the same star on different days will all be different, and each one must be reduced to the place which will ultimately appear in the star catalogue. A great deal of this work is quite simple arithmetic, proceeding according to instruction on well known lines. Under proper direction it is done by boys and young ladies, who in the different observatories of the world play a part in scientific work which is often little realized by those who hear only of the final conclusions. Their labour is often greatly lightened by the calculating machine, which performs long multiplications and divisions at a speed almost incredible to the uninitiated.

We have referred already to the astronomer's dependence upon the engineer, the optician, and the glass maker. We must not omit from our list the photographic plate maker. The question is often asked at an observatory, whether the astronomer uses specially sensitive plates. The answer is that it is not possible to obtain better emulsions than those which are prepared by the leading makers of photographic plates. Not that they are perfect; they are far from being perfect, and it is particularly desirable that some means should be found of getting rid of the coarseness of grain at present universal in rapid plates. For the time being, however, it seems that not much progress is being made to this desirable end. In one way only do astronomical plates differ from those in ordinary use. They are coated upon thin plate glass, which is nearly flat, instead of upon the ordinary glass, which is generally twisted and billowy.

In this necessarily rapid sketch of the evolution of the modern observatory, we have been compelled to leave large gaps which could not be filled in the space at our disposal. We have now to deal in successive chapters with some of the leading results of modern astronomy. We shall try as ar as possible to present the subject free from the complications which beset it in actual practice.

In concluding a chapter on Astronomers and Telescopes, we may remark that the application of photography has made a difference which is hardly yet realized. Since photographs can be reproduced and put into the hands of all, it follows that any student of astronomy is the potential possessor of the greatest telescopes in the world. By this we mean that astronomical work of real importance—not merely intellectual recreation, but study which may make valuable contributions to knowledge—can be done upon published photographs. The output of pictures is enormous, and many of them remain mere pictures for lack of students to treat them seriously. If you would be an astronomer, think twice before buying a telescope. A measuring machine, to measure photographs taken by other people, is a far better investment.

CHAPTER II

THE SUN AND MOON

When we put the Sun and the Moon together in one chapter of our study of astronomy, we do not attempt to be logical. The sun, the overpowering source of light and heat and energy which makes life upon the earth and all our activities possible, and the moon, the dead lifeless companion of the earth, whose most obvious use is to act as a pale reflector of sunlight to mitigate the darkness of our nights, are at opposite ends of the series in which we may arrange the components of our solar system. Nevertheless, we will study them together, and we shall find in so doing that a disregard of logic is sometimes as advantageous in the study of astronomy as it is in the conduct of life.

The study of the sun is essentially modern, because it depends very little on the formal laws of geometry, and requires above everything some elementary ideas upon the properties of matter, which are of quite modern growth. Little more than a hundred years ago, for example, it was possible for scientific men to discuss quite gravely, without ridicule, whether or not the sun might be inhabited; not on his surface, indeed, which was undeniably too hot, but perhaps somewhere in the darker layers which we seem to see below the brilliant surface, when we look down into the depths of a sunspot. To-day, such an idea has only to be mentioned to appear ridiculous. We can no longer be content with the plain fact that the surface of the sun is pouring out immense supplies of heat and light; we ask ourselves at once, whence come those supplies; we realize that they must be brought up to the surface from within; and we see that it must be even hotter inside than it is on the surface. There is no more a question of cool, habitable depths below. Nor is it probable that the sun is anywhere solid, in the ordinary sense of the term.

With a diameter one hundred times that of the earth, the average density of the sun is no more than a quarter the average density of the earth, and not more than one-half the density of the earth's rocky surface layers. One can account for this combination of great size and little density only by supposing that the sun is is in a state of what must be called gas, since it is neither solid nor liquid, but which is gas in a state which we cannot reproduce experimentally, a kind of thick treacly fluid, denser than water, yet with the elastic properties of a gas. Between the interior and the surface there must be a continual rush of hot currents bringing up from inside the heat which is poured out at the surface. Beyond that bare fact we still know comparatively little of what goes on inside the sun.

The first question that must be examined is this: Whence comes the heat? It is brought up by currents from below; but how is the supply below maintained? The sun is not burning, in the sense in which a coal fire is burning and producing heat by combustion in the oxygen of the air. Its heat is more like the heat of a white-hot poker, which radiates away, and the poker becomes cold, not having the means of renewing its heat as the sun has. The way in which a gaseous body may radiate heat, and yet remain hot or even become hotter, was explained by Helmholtz in a simple manner.

The sun maintains his heat because he is contracting.

When a stone is dropped upon a pavement, it acquires a store of energy during the fall which is suddenly dissipated when the motion is stopped. It is not lost, however. No energy can be lost. It can only be transformed from one kind to another; and in the case of the stone suddenly stopped, the energy which it had acquired in falling is converted into heat, which is radiated away. But the stoppage need not be sudden. The same amount of heat would be produced if the stoppage were ever so gradual. Suppose then that the sun, under the influence of its own strong gravitation, contracted in diameter a thousand feet. Every particle at the surface would have fallen a thousand feet and have been stopped; all the matter inside would have fallen in proportion, and the amount of heat generated would have been enormous. It is not hard to calculate that a shrinkage of a thousand feet would supply all the heat which the sun radiates in five years. At this rate the sun would decrease in diameter about 40 miles in a thousand years; and since his diameter is 800,000 miles, it is quite clear that this principle of Helmholtz is able to explain how the sun's heat might be maintained for a hundred thousand years without any appreciable decrease in his apparent size. During hundreds of thousands, or millions of years, the sun might be pouring out his enormous supply of heat, year by year, and might all the while be actually growing hotter. He will continually grow hotter so long as his material obeys the laws of gases.

Gravitation, therefore, is a cause sufficient to maintain the sun's heat for ages. Yet it has been the opinion of the most eminent mathematicians that we cannot in this way provide for the maintenance of the earth as a planet deriving its heat from the sun, for a period extending into the past more than twenty million years; while geologists have demanded a hundred million for the evolution of the earth into its present condition. How far this difficulty was ever serious it is hard to say, for it cannot be denied that both estimates rested upon assumptions a little difficult to verify. Happily it would seem that there is no longer any need to be concerned at the difference between mathematicians and geologists.

Within the past few years all our notions of the constitution of matter have been revolutionized. We have become familiar with radium and with all the radio-active substances. From these we have learned that atoms of matter contain stores of energy which are ordinarily locked up, but which in certain bodies can gradually escape, and produce heat and many kinds of electrical action. These radio-active bodies exist in the crust of the earth in most minute quantities—as is proved by the enormous price of radium—yet it has been calculated by Strutt that the earth would be far hotter than it is, were the interior as rich in radium as are the surface layers, and were the radium able to produce heat in the interior as it can at the surface. Moreover, it by no means follows that the energy of the atom, which is ordinarily imprisoned in terrestrial matter, remains imprisoned under all conditions—under such conditions, for example, as prevail in the interior of the sun. Hence it would seem that by the discovery of the energy within the atom, all our difficulties are inverted. It was a question how to account for a sufficient supply of heat; in future the physicists may be embarrassed by the problem, how to explain that the sun's supply of heat is no greater than it is.

There is another source, not yet considered, from which the sun might derive heat—by the fall of material upon his surface from what we are accustomed to regard as empty space. This differs from the first way we considered, in that it involves a continual increase in the mass of the sun, which would shorten the year—for dynamical reasons quite simple, into which we need not enter for the moment. The question then is, how much matter could the sun absorb, without causing a difference in the length of the year which we could detect. An interesting calculation which gives the answer has been published lately by Newall. He shows that four thousand million tons of matter might fall on the sun every second, without diminishing the length of the year by more than a thousandth of a second per year. "It sounds," he adds, "a large quantity. But if we regard our earth as the quarry from which the material had to be supplied, we should have a source that would last for fifty thousand years." The combined effects of shrinkage, radio-activity, and fall of matter from without are amply sufficient to maintain the sun's heat for a time to which we can set no limit.

It is an interesting question, why does the sun, though it is but a sphere of gas, have a visible and fairly definite surface? The telescope shows that the surface is granular, formed of small bright cloudlets in rapid motion, changing in pattern from minute to minute, so that on photographs taken ten minutes apart it is impossible to identify the individual cloudlets, so rapidly have they moved. In this shining surface, the photosphere, are the rents or vortices from which the cloudlets have been sucked away—the sunspots which were discovered immediately the first telescope was turned upon the sun, and which are still most fascinating objects for study in small telescopes. In spite of the prolonged and minute attention which they have received for many years, and in spite of the refined and ingenious modern processes which are now employed in their examination, their nature remains very much of a mystery. They are evidently holes in the photosphere from which the bright cloudlets have been temporarily removed. One may watch the edges of a spot as it were melting away and falling in; one may see, in the spectro-heliograph pictures which we shall presently describe, how the masses of hydrogen and calcium vapours which lie above and around them, are whirled in vortices. But whether the sunspot represents an uprush of heated matter or a downrush of cooled gas from without remains still uncertain.

Whatever may be the actual physical structure of sunspots, it is evident that they are an index to an activity which is not merely superficial, but affects the whole structure and functions of the sun. For many years the ever varying number of spots had been observed and recorded without any suspicion that they were governed by a set of complicated and far-reaching laws. But about the middle of the nineteenth century it was remarked by Schwabe that the number of spots increased and diminished in a period which is roughly eleven years; and very soon afterwards it was found that other phenomena, with no apparent connection, obeyed the same law. Displays of the aurora are frequent in high latitudes, and most frequent when the number of spots is great. Occasionally the aurora is seen in temperate latitudes, and a remarkable display is generally associated with an outbreak of unusually large spots upon the sun. Again, the magnetic compass is subject to a small daily swing backwards and forwards, which is greater at some times than at others; the amount of the daily swing is closely associated with the number of sunspots, and upon some days when sunspots are unusually active we have what is called a magnetic storm, when the compass needle is violently disturbed and telegraphic communication is interrupted by currents of electricity in the earth.

These widespread effects can hardly be ascribed directly to the spots upon the sun's surface. Rather should we consider that all three, sunspots, auroræ, and magnetic disturbances, are manifestations of some deep-lying cause in the sun, of whose nature we are at present ignorant. The whole phenomenon is indeed extremely complicated. At the beginning of each new period the spots break out in middle latitudes upon the sun. They break out rather suddenly, and in a year or two the number and area of the spots becomes a maximum. Then they gradually decline, and, as they do so, they approach more and more nearly to the sun's equator. Their period of eleven years is far from regular; at different latitudes they cross the sun's disc in different times, making it appear that the sun does not rotate as a whole; at one time a great sunspot will be associated with auroræ and magnetic storms; another equally great will not; and sometimes one will have the disturbances upon earth without a visible disturbance on the sun, in which case it is of course permissible to suppose that the spot is on the far side of the sun.

Such complex facts necessarily require long continuous observation before their laws can be elucidated. For many years there has been an organization at work in the observatories of Greenwich, South Kensington, Dehra Dun, and Mauritius, by which the sun has been photographed every day, and the number and area of the spots has been recorded. More recently new methods of investigation have been developed. The Carnegie Institution of Washington maintains a magnificent solar observatory upon Mount Wilson, in California; the Government of India has established an observatory of the first class 7,000 feet above sea at Kodaikanal, in Madras. South Kensington is the English station specially equipped for this work; and there are hopes that before long a fourth solar observatory, in Australia, may complete the chain and provide for the almost continuous study of all the phenomena of the sun's surface. There appears to be some reason to think that in course of time it may be possible to discover the connection, which probably exists, between the activity of the sun and the larger features of terrestrial weather, more particularly the conditions which produce drought and famine by failure of the Indian monsoon.

Above the photosphere of the sun lie the regions which become visible to the eye only when, at a total eclipse, the moon intervenes exactly between the sun and the earth. So nearly of the same apparent diameter are the sun and moon that it depends upon the position of the moon in her not quite circular orbit, whether she shall appear at the moment of eclipse slightly larger or slightly smaller than the sun. If she is smaller the eclipse is annular, that is to say, a narrow ring of the sun is visible all round the moon, and the phenomenon is of no particular importance. But if the moon happens to be in a part of her orbit nearer the earth, and the sun looks a little smaller than the moon, then as the last segment of the sun's photosphere is covered by the moon, there appears a narrow belt of bright crimson with prominences rising from it, and the most beautiful halo of pale white light, which is the sun's corona.

It is great good fortune that we can see the corona at all. Were the moon ten per cent less in diameter eclipses would always be annular, and we should be unaware to this day of the very existence of the sun's most beautiful member. As it is, the moon is just large enough, when circumstances are favourable, to cast a narrow shadow, perhaps seventy or eighty miles across, on the earth.

It is the object of eclipse observers to get within this shadow, and with a battery of instruments to make the most of the precious few minutes while the shadow is sweeping across them; the nearer they are to the central line, the longer does the shadow take to cross them; but in no case can the duration of total eclipse exceed about seven minutes, and the average is more nearly three or four. The path of an eclipse passes indiscriminately over land and sea, habitable country and desert; so that eclipse observers find plenty of variety in their journeys, and travel to out-of-the-way places. Within the last fifteen years astronomical expeditions have gone to the North Cape, Novaya Zemlya, and Japan in 1896; to India and Mauritius in 1898; Florida, Spain, and Algiers in 1900; Sumatra in 1901; Labrador, Spain, and Tunis in 1905; Flint Island in the South Pacific in 1908; Tasmania in 1910; and at the time of writing several expeditions are on their way back from Vavau, near Tonga, again in the South Pacific. In 1912 they will have to go to Rio, and in 1922, after a lapse of nearly 200 years the shadow track of a total eclipse will cross the British Isles.

There are few quests of a more sporting nature than the pursuit of the solar corona. An eclipse expedition involves months of preparation at home; the construction and testing of special instruments; the building of portable huts and the provision of all kinds of stores both scientific and domestic; then the long voyage and sometimes the perilous landing in the surf of a coral island; the anxious weeks of building the observing huts and setting up the instruments; the long practice and drills, so that each member of the party may do exactly the right thing at the right instant in operating a battery of cameras and spectroscopes during the precious seconds of totality. Then if the sky has been clear comes the anxious, trying work of developing photographs in a temporary darkroom, with the air much too dusty or water much too hot and dirty to make photography easy. And finally, after a safe return home, there are more months to be spent in measuring and discussing the photographs obtained, and in publishing the new conclusions which result from their study.

The writer's personal experience has been confined to one eclipse, which was completely cloudy; the kind of eclipse which scoffers say is the most satisfactory, because there is no responsibility for any failure, and nothing to do afterwards. A cloudy eclipse does show one part of the phenomenon particularly well—the shadow which comes sweeping across the country at twenty miles a minute. From the hill above Vadso on the Varanger Fiord in 1896, one had a long view up the fiord to the west. The party had left their ship at midnight in the rain; the eclipse was to be at five in the morning, and in those high latitudes in August the sun scarcely set. Breaks in the cloud at intervals gave rise to hopes which were as quickly dashed. For a few moments during the partial phase of the eclipse the sun came out; then the clouds settled down and took on a gloomy purple as the light failed and totality approached. Then the shadow, plainly visible upon the sea, came tearing down the fiord from the west, and in the semi-darkness, with no trace visible of the spectacle which was passing behind the clouds, the party, as is always ordered, performed the solemn farce of exposing the plates and operating all the instruments as if everything were well.

Such an experience is by no means unusual. In the same eclipse the parties in Japan had fog. In Spain, in Sumatra, in Labrador, in Tasmania, and lately at Vavau, the observations have been more or less completely spoiled by cloud. An astronomer plays for high stakes when he devotes the best part of a year and a good deal of money, not always his own, to an enterprise whose success depends absolutely on clear sky at the critical moment.

When he wins, the stakes are worth having. He no longer occupies himself with the prominences, for he has found a way of observing them without an eclipse. But in spite of many attempts, no one has yet succeeded in observing the corona without the friendly aid of the moon, to cut off the sun's overpowering light. All that we know of the corona has been gathered in the couple of hours or less obtained by adding up the scattered minutes of totality, during the sixty years or so in which eclipses have been seriously observed. The main facts about the corona are relatively simple. Its light is largely light reflected from the sun, as if by small particles of dust; but there is also present a gas which emits a green line belonging to no known element. Its structure depends upon the sunspots. When the spots are many the corona is condensed, with short rays somewhat evenly distributed. When the spots are few the corona has long curving extensions like wings in the plane of the sun's equator, with a brushlike structure of short rays proceeding from the poles. The form of the corona is, then, one of the inter-related effects of that prime cause, whatever it may be, which is responsible for the sunspot, the aurora, and the magnetic storm. How the effect is produced remains nearly as much a mystery as it was when the corona was first photographed.

We have, however, obtained an idea of the manner in which the streamers of the corona are probably formed. Within the last few years it has been realized that light can exercise a very effective pressure upon particles which are within quite narrow limits of the right size, about a third of the wave length of the light. The flood of light poured out by the sun must be capable of sorting out the dust particles in surrounding space, and of sending flying those which happen to be of the right size. Very probably the corona is formed in great part of dust streams driven outwards by this pressure of light; but the mechanism by which precise shape is given to the streamers, and the reason why this shape depends upon the sunspot period, remain quite obscure.

Apart from these rather complicated questions the corona has an interest all its own as the most splendid feature of a rare and intensely impressive event. So long as the slightest segment of sun remains, no corona is to be seen; nothing is ever seen of it in the narrowest annular eclipse. So long as even a single bead of bright light on the moon's llmb marks where the sun is still shining through the notch of a lunar valley, the corona remains invisible; as the moment of complete totality arrives it flashes out clear and pale on a deep blue or purple sky;

and it vanishes again at the first instant of returning sunlight.

Leaving aside the many other very technical investigations which are made during an eclipse, let us glance for a moment at the prominences, which before the eclipse of 1868 were visible only during totality, but since then have been visible on any day to any one who can follow the path which Lockyer and Janssen indicated. The prominences are eruptions of glowing vapours, principally hydrogen and calcium, which are thrown up from the sun's surface, and appear during eclipses as the red tongues of flame round the dark edge of the moon. Since they are gas, they emit rays of a few definite wave lengths only; the spectroscope is able to spread the general white light of the sun into a band as long and consequently as diluted as one may please; but it can do nothing to dilute the pure bright images of the prominences in the lines characteristic, let us say, of hydrogen. Hence with a good spectroscope the prominences may be seen and photographed at any time, without waiting for an eclipse, and in fact the record of prominences is included in the daily routine of a solar observatory.

This brings us back to the new methods of invest'gating how the vapours of hydrogen, calcium, iron, and other metals are disposed in the atmosphere of the sun. The inst ument employed is the spectro-heliograph, developed by Hale at Chicago and by Deslandres at Meudon. We can scarcely, without presupposing some knowledge of instrument construction, explain the principle of this excellent invention. Let it suffice to say that by means of two slits moving in unison it is possible to separate out the light coming from the vapour of a given element in a given layer of the solar atmosphere, and to produce, for example, a picture of the sun in hydrogen light from the upper layers or in calcium light from the denser lower layers just above the photosphere. The spectro-heliograph has opened out a wholly new line of possibilities, which must have in the future a profound influence on our knowledge of what goes on in the solar atmosphere. But for the time being the profusion of results, outrunning the powers of interpretation in many cases, makes it impossible to sum up in a few words the clear gain which has resulted.

In considering these new methods of solar research we must not overlook the fact that the results achieved may have a double significance and value. We study the sun because we are interested in all knowledge of the great power-house, so to speak, of the solar system. But we study him also because the sun is in a sense a gigantic laboratory, where processes can be observed at temperatures, at pressures, and on a scale which cannot be imitated in terrestrial laboratories. In the sun may be found the answer to questions which can hardly be investigated otherwise, questions of the highest importance in physics and chemistry. Are all the chemical elements permanent, however high the temperature? Or are some of them capable of resolution into simpler elements? How shall we account for the fact that some elements—hydrogen for example—give two spectra completely different, one of which can be produced easily enough in the laboratory, while the other cannot be produced at all, and is indeed recognized as belonging to hydrogen only by some curious arithmetical relations between the lines in one spectrum and the lines in another? All questions such as these may be solved some day by experiment in the solar atmosphere, conducted at a distance of more than ninety million miles from our solar observatories upon earth.

Moreover, there is yet another side to the question. The sun is a star; and the only star which we can study in detail at close quarters. A thorough understanding of what is going on in the sun's atmosphere will lead us gradually, no doubt, to an interpretation of the many problems presented by stars of varying types.

As we pass from the sun to the moon let us glance for a moment at the tides, in the production of which the moon and sun play similar but somewhat unequal parts—the proximity of the moon considerably more than compensating for the much greater mass of the sun. Every one knows in a general way that the tides are due to the moon—though it is on record that one observer became doubtful because he had sometimes noticed that there is a tide when there is not any moon! But the subject is complex and difficult; and the well-known figure, of high water under the moon, where the moon drags the water away from the earth, and another high water on the opposite side, "where the moon drags the earth away from the water," is responsible for not a little misconception.

The crest of the principal tide wave produced by the moon is not necessarily under the moon, but its position depends upon a number of conditions. Perhaps the best way to think of the tides is to imagine in the first place a free ocean, of uniform depth, unobstructed by land. Then the actions of the sun and moon, in their various configurations, combined with the rotation of the earth, would cause a number of tidal waves of various periods and different heights, and the effect at any moment is due to the combination of these waves. Their tendency to run round the earth is profoundly modified by the configurations of the land, which check the waves, and deflect them, and concentrate them in certain places, so that in different parts of the world the phenomena of the tides are extremely various.

Around the coasts of the British Isles the tides are somewhat exceptional, in that the two tides each day are approximately of the same height. In consequence the tides are not very difficult to predict with fair accuracy by the use of rather rough and ready methods. In other parts of the world—India, for example—the two successive tides are very unequal, and they vary apparently very irregularly. Far more scientific procedure is then required, to predict the tides with the accuracy demanded by navigators in the Eastern seas. First, the tides, for a series of years, must be observed at various ports. Then the observed tidal curve must be analysed into its separate components by the method known as harmonic analysis. When that is done, the tides for future years can be predicted by the beautiful machine, the property of the Government of India, which used to stand in the Science Museum at South Kensington, and is now in the National Physical Laboratory at Bushy House. Every year a series of pulley wheels in this machine are set to execute the oscillations prescribed for them by the results of the analysis. A cord passes over the whole series and carries a pen at the end. The machine is set in motion, and the pen, actuated by the combined motion of all the pulleys, draws in a few hours the predicted curves for the year, one after the other, for the principal Indian ports.

For its practical importance the study of the tides ranks with the most indispensable applications of scientific enquiry. Success in predicting the apparently haphazard, the really complex but orderly tides of a port such as Aden, is justly regarded as one of the triumphs of the theory of gravitation. But the most fascinating chapter in the theory of tides is the chapter in which Sir George Darwin has traced the past and predicted the future history of the moon. The friction of the tides retards the rotation of the earth, so that the day is becoming longer by very slow degrees. By a process of reaction, which we cannot attempt to follow here in detail, the moon is by equal necessity driven away very slowly from the earth, so that the month becomes longer also. Carrying the process back, Sir George Darwin has pictured for us a time when the earth and the moon were almost in contact, and the day and the month were equal, probably between three and five hours long. With the two bodies so close together the tidal forces were much more powerful and efficacious then than they are now; the moon was driven away, and the month was lengthened; the day was lengthened also, but not so fast; by slow degrees we arrived at the present condition, with a lunar month 27.3 days. Looking forward, we are told that the day will grow longer more rapidly than the month; that in the far distant future the day and the month will be again equal, at about fifty-five of our present days; after which the moon will begin to approach gradually nearer and nearer to the earth until, if the heavens survive, her ultimate fate is to return to the earth from which perhaps she was formed in the beginnings of our system.

The most familiar result of these intricate tidal effects is that the moon turns the same face constantly towards the earth — not absolutely, for her varying motion in an elliptical path allows us to see occasionally a few degrees one side or the other beyond the usual limit. But, speaking roughly, we see always the same face of the moon, and are condemned to remain in ignorance of what may be on the hidden side. Let us, however, say at once that there is no probability that the side we cannot see is anywise different from the side which we can see. The speculations which have given the far side of the moon an atmosphere, water, vegetation, and inhabitants, have no justification whatever. When, after the lapse of millions of years, the effects of tidal friction allow future astronomers to examine at last the side which is hidden from us, we may feel sure that they will be disappointed if they expect to see anything more than the volcanic wilderness which the moon's near face presents to us. No trace of atmosphere survives upon the moon; a star passing behind the moon moves steadily up to the moon's limb without dimming or refraction, and at the limb, especially if it be the dark limb, the star goes out with a suddenness which never fails to surprise the observer. No trace of cloud, no trace of the action of water in any form, can be descried now upon the moon's surface. No twilight mitigates the hardness of the black shadows cast in the intense sunlight, and it is not perfectly certain that any change has ever been observed in the exquisite detail which even a small telescope will show.

In the opinion of one who has spent many Saturday evenings in showing celestial objects to members of a University, the moon is the one object in the sky which may be shown to anybody without fear of disappointment. The exaggerated ideas which are current as to the power of a big telescope; the expectations that any casual observer should see as much as is depicted in the finest astronomical drawings or photographs, make it a matter of some uncertainty whether any other object will please or disappoint. The moon alone among telescopic objects looks immeasurably better in the telescope than any published representation; her beautiful silvery colour, the exquisite sharpness of the shadows on plain or crater floor, the brilliance of the mountain peaks as they catch the first rays of the rising or the last of the setting sun, cannot be reproduced upon paper.

The many excellent published photographs of the moon give, however, a perfect idea of the moon's features, if not of her beauty. Those features are in great part volcanic—or so we are prone to believe. The volcanic features are indeed upon a scale which is immense compared with the size of the moon, and they differ in some important respects from the generality of volcanic craters upon the earth. The perfect development of the central cone, the clean cut which one crater will make into the wall of a neighbour, and the immensely long, bright, straight streaks which radiate from some of the principal craters, are features to which the earth's volcanic regions afford no very perfect counterpart. In considering how far such differences are significant we must remember the lesser power of gravity upon the moon, the absence of atmospheric denudation, perhaps also the absence of the steam which plays such an important part in volcanic action upon earth. Interesting and suggestive as are the theories which would explain lunar craters as due to the splash of meteorites, or the bursting of bubbles, it does not appear that there is much justification for refusing to believe our eyes, when they tell us so plainly that the moon is volcanic but that the volcanoes have long been dead and cold.

For the remarkable difference between the earth and the moon, or, so far as we can tell, between the moon and other satellites, no reasonable explanation has been given. The problem is a curious one when we regard the moon as a satellite of our planet earth. It is no less curious if we regard the earth and moon as twin planets moving together about the sun. One analogy may perhaps be mentioned here, though we must be careful not to press it far. Of late years we have become familiar with the fact that quite a large proportion of the stars are twin stars, and that the two members of the pair are often surprisingly different in their physical state. Is it perhaps possible that when one body separates into two—

as we suspect has happened with the earth-moon system, and in the binary stars—there is a considerable sorting out of materials, so that the two bodies which are produced differ from one another very much in composition and in character? It may be so, though we cannot pretend to understand the mechanism of the process. And unfortunately nothing is happening now upon the moon's surface, to give us any clue as to the processes which have gone on in the past.

To revert for a moment to our consideration of the sun: we were able to regard him as a laboratory in which we could study, though at a distance, the performance of experiments in physics upon a vast scale. The moon we must liken rather to a closed museum case in which the past is so to speak crystallized, hard, brilliant, and exquisite, but unchanging, and lacking, therefore, the fascination that belongs to continual change and reconstruction.

CHAPTER III

THE PLANETS AND THEIR SATELLITES

FOR many people there is just one question in astronomy: Is there life upon Mars? The possibility that astronomers may find in some other planet traces of intelligent life has exercised a fascination that inspires the novelist, the dramatist, and above all the journalist. So the world has become quite familiar with the idea that something or other has been found upon Mars which seems to prove the presence of intelligent inhabitants. For Mars, at any rate, the question is considered almost settled: why else did a generous benefactor leave to the Paris Academy of Sciences a magnificent prize, to be given to the man who first succeeds in establishing communication with a planet *other than Mars*.

A rapid survey of the planets will show us why it is always Mars that is chosen for the abode of life. Mercury is very near the sun; Mercury, if he has an atmosphere, which seems doubtful, must be very hot; and if there is no atmosphere can hardly be reckoned habitable. Moreover, Mercury is small and never gets very far away from the sun, and so is difficult to observe; and it is not certain that any definite markings have ever been seen upon his surface. Now, for any proof of habitation, definite markings are indispensable. Mere habitability as an abstract quality has little attraction for those whose interest lies in a search for evidence that life actually exists. And if we ask ourselves what kind of evidence would prove, or at any rate strongly suggest actual habitation, the answer seems to be that we must see something that looks unnatural, but can be interpreted with some reason as artificial. We must look, in fact, for evidences of intelligent engineering upon a gigantic scale.

From this point of view Mercury is hopeless; and the case is no better with Venus, though Venus is nearly the same size as the earth, is not impossibly near the sun, and has a large cloud-laden atmosphere that might very well make life as we know it tolerable there. Unfortunately it is just this atmosphere that makes it impossible for us ever to see down to the surface of the planet. None but the most vague and indefinite markings are visible upon the disc of Venus: so vague indeed are they, that it is not yet quite certain what is the period of rotation of the planet, though curiously enough the doubt lies between 24 hours and 227 days. So long a day as 227 of ours might well seem to make the planet undesirable, even though other conditions were not very dissimilar to those upon the earth. But in any case we are debarred from a sight of the surface, and from any possibility of detecting what may lie upon it. Hence, in our search for inhabitants, Venus must be passed by.

Proceeding outwards beyond the orbit of the earth we come to Mars, and then to the zone of the minor planets. These small fragments of worlds have an interest all their own, but it lies remote from possibilities of habitation. Making up in number what they lack in size, the minor planets are at once an interest and burden—objects of that sporting instinct which prefers always to discover some body unimportant but new, rather than to work soberly at the known bodies which are from their great number an incubus to those whose duty it is to keep track of them, and to predict their places so that they may be observed. The time has long gone by for gratitude at the discovery of a new minor planet. Very early in their history they broke down the rule that every planet must have a symbol of its own—or, at any rate, they reduced the symbol to the simple form of a number enclosed in a circle; and the day is not far distant when those numbers will run into four figures, and the circles will have to be elongated to hold them. Next, the increase in their numbers made it very difficult to maintain the rule that every minor planet should have a female name drawn from classical mythology. The selection of the name Victoria led to a heated argument with staunch republicans, till a learned editor pointed out that Victoria was a daughter of Pallas, adding the delightful note: "Pallas, a giant—not the goddess, who is believed to have left no children." Later, the classical dictionaries became exhausted; then came the names of sweethearts and girl babies; afterwards Alleghennia, Pittsburghia, and Chicago; and finally, the naming became so burdensome that discoverers neglected it altogether, and the German commission who look after such matters made the rash threat that if a discoverer failed to name his discovery within a given time, they would do it for him.

There are many fascinating problems connected with the minor planets, but they lie on the mathematical side; for these planets are all so small that only three or four have measurable discs. The biggest are only a few hundred miles in diameter, and if we look for places where we may hope to find intelligent life, they are negligible. Their mathematical interest may be indicated very briefly. First, there was the question whether they were the result of the explosion of a large planet; that seems to have been settled definitely in the negative. Then came the question, why are they arranged in zones, with considerable gaps between the zones, and the answer to that is, that the gaps have been formed by the disturbing action of Jupiter and Saturn. Finally, of late years it has been found that some of these small bodies are so arranged that they provide beautiful examples of certain celebrated theorems in the problem of the motion of three bodies under their mutua attractions; but it would take us too far to cast even a glimpse into this highly specialized field of enquiry.

One of them, the planet Eros, we must mention for its great importance as an aid in determining the distance of the sun, as we shall see in Chapter VI. But Eros is an exceptional body, with an orbit considerably inclined to the ecliptic, of decidedly elliptical form, interlacing the orbit of Mars, and on occasion approaching the earth within fifteen million miles. Perhaps we should not think of Eros as a minor planet at all, but as a small planet, the only one of its class at present known, which, like the new satellites of Jupiter and Saturn, seems to have been reserved for the purpose of upsetting preconceived notions at the beginning of the twentieth century.

Passing on now to Jupiter, the largest of our sun's family, we perceive at once that as we cross the zone of the minor planets, we cross into a region where the planets are no longer terrestrial bodies, in the sense that they are something like our own Earth, but are much larger, much lighter, almost certainly much hotter, and seem to belong altogether to a more primitive age, such as that which the smaller planets doubtless passed through long ago. The surface of the planet is obscured by strongly marked belts of cloud, flecked with bright and dark patches which change from month to month; and from year to year the belts change also, a single belt becoming double, or a pair coalescing to form one. At first sight it may be thought that this appearance is very much like what the earth must present from without, with its equatorial belt of clouds and its trade wind zones. But the cloud belts upon the earth, and consequently its appearance from without, vary with the seasons and repeat themselves pretty exactly year after year; while upon Jupiter there is little appearance of season, and the changes do not appear to repeat themselves after the lapse of Jupiter's year. We are forced to conclude that Jupiter is in a condition very different from that of planets which we may regard as habitable. If Jupiter has a solid surface, which is not altogether probable, it must be sunk so deep in the cloud envelope that the rays of the sun can never penetrate there, and we cannot regard it as fitted at present for life anything like our own.

As an abode of life the planet Saturn seems to be in worse case than Jupiter. The mean density of the visible globe of Saturn is less than that of water, from which we may, of course, conclude with certainty that we see nothing of any solid nucleus, which, if it exists, must lie far within the cloud envelope and can hardly be reached by the rays of the sun. It is idle, therefore, to speculate how the rings which encircle the planet, and the nine or ten satellites which accompany it, may ook to the inhabitants on the planet's surface. These splendid appurtenances are thrown away upon an uninhabitable world.

Proceeding still farther outwards we come to Uranus, and then to Neptune, of which, as planets, there is little more to say than that they are large, each about 30,000 miles in diameter; that they are light in substance like Jupiter and Saturn; and that they show no detail upon their tiny discs. Neptune in a large telescope is of a fine green colour, which makes him readily distinguishable from the surrounding stars, and the spectroscope shows that both he and Uranus have large and dense atmospheres, that absorb some part of the sunlight falling upon them, before it is reflected back to the earth. More than that we cannot say of these planets on the boundary of our solar system.

The result of our review is, then, that no planet excepting Mars affords us any opportunity of scrutinizing its surface so closely that we may hope to find there any evidences of intelligent life. We may consider it very fortunate, therefore, that Mars—whose position next outside the earth's orbit allows us to study his surface to greatest advantage—is the one planet whose real surface seems to be open to our examination.

At first sight Mars is by no means unlike what we may believe the earth would look like if we could study her from Venus. Mars is smaller, indeed, than the earth, with a diameter not much more than half as great; but still Mars is amply large enough to be a comfortably habitable globe. And though the force of gravitation would be less there than upon the earth, and a man suddenly transport d there would feel that his size and his strength were out of proportion to his weight, nevertheless we need not imagine that he would require any immense structural alteration to be adapted to the new conditions. Mars has an atmosphere—not, indeed, so dense as that of the earth, nor so laden with clouds and moisture—but still an atmosphere which can carry some amount of cloud, and which is not altogether too thin, we may believe, to allow of the existence of beings something like ourselves. In estimating such probabilities we must always remember how extraordinarily adaptable our own organisms are. To a native of the Congo the life which the Esquimaux lead might very well seem impossible; still more is it remarkable how on the expedition to Lhasa the British and Indian troops were able to pass almost with impunity from the heat of India through the terrible cold of the passes into Thibet.

Mars has polar caps—not indeed so large as the polar caps of the earth, nor so permanent —but still caps of brilliant white which form in the winter and melt in the spring very much as if they were formed of snow or ice. Their existence shows that a circulation takes place in the Martian atmosphere as in our own, and we shall not be straining probability too much if we assume that they really are composed of water frozen into ice or snow. In saying this we do not overlook the arguments which have been put forward to prove that water vapour cannot exist in the Martian atmosphere; and the suggestion that the polar caps of Mars may be made of frozen carbonic acid gas. The possibility of the escape of gases at the limits of a planet's atmosphere has been held, in recent years, to account for the absence from our atmosphere of the rare gas helium, which is given off by certain minerals and mineral springs. While no one doubts that a certain quantity of gas may thus escape, it is by no means certain whether the process is effectively rapid or ineffectively slow. And we may very well hold it as still uncertain whether water vapour, which cannot escape at all from our own atmosphere, can escape so effectively, and in such quantity from the atmosphere of Mars, that that atmosphere is practically drained of moisture.

Until something more than thirty years ago the drawings of Mars which were made at the telescope showed a distribution of light reddish areas, taken for land, and of greenish areas, which were supposed to be seas; and though the relative distribution of land and sea was a good deal different in its character from ours, there was no difficulty in accepting it as a reasonable arrangement which might serve very well the parts of water and land upon the earth. At that time no one had heard of the canals.

The canals of Mars were discovered by the Italian astronomer Schiaparelli at the very favourable opposition of the planet in 1877 when its two satellites were also found. Their discoverer was struck by their narrow straightness, quite unlike any features that had been seen before upon the planet, and he almost at once considered the possibility that these features might be artificial—an hypothesis, he affirmed, which is in no way impossible. For many years, however, they remained invisible to all but Schiaparelli; then gradually, with increasing skill, perhaps with increasing desire to see them, other observers succeeded; and now it is only occasionally that a drawing of Mars is published which is not covered with a network of fine black lines, almost as conspicuous as the hands of a watch upon the dial. No one looking at a modern drawing of Mars, and comparing it with an old one, would be disposed at first to believe that the two drawings could possibly represent the same object. And it is this striking dissimilarity between the old drawings and the new which leads sceptics to deny even now the existence of the canals, and to affirm that they are visible only in virtue of a contagious enthusiasm and almost hypnotic suggestion. If that explanation fails, then they are ascribed to an alleged natural tendency for an observer to represent in the form of hard dark lines any fortuitous arrangement of minute and intricate detail very imperfectly seen.

We may say at once that it seems impossible to maintain this line of argument. The canals have been observed by many independent observers, who are positive that they are not deceived; they admit the difficulty of the observation, but maintain that at the rare moments when the air is really steady the canals come out clean and sharp.

They say, and apparently with justice, that critics who have never undergone the long training in observation, and especially critics who have never spent weeks at the telescope waiting for the occasional moment when a really fine climate gives really good seeing—those critics have no right to say what can or cannot be seen upon the disc of Mars. The casual visitor who asks for a glimpse of Mars is invariably disappointed. He expects to see the planet looking at least a little like the spider webs of canals which figure so conspicuously in the published drawings. He is disappointed, and he thinks poorly of the telescope. The wise astronomer does not, if he can help it, allow visitors to look at Mars at all. For the casual visitor never realizes that the published drawing represents the very most that can be seen at the most exceptional moment, and that the observer himself, with all his training, cannot see five per cent. of the whole for ninety-nine per cent. of the time.

We do not propose, then, to enter upon the thorny question, whether the published drawings of Mars are representations of genuine detail or are untrustworthy guesses at something which really lies beyond the range of accurate visibility. It is far better to admit the whole of the evidence, and to see whether with that the advocates for life on Mars can prove their case.

To do so they must establish two propositions :—

1. That the canals are not natural.

2. That if they are artificial they can be supposed to serve a useful purpose, a specific purpose, which we can believe is valuable or necessary to intelligent inhabitants.

Now it is impossible to lay down hard and fast rules for distinguishing the artificial from the natural. A graceful piece of engineering which fits its surroundings, such as the railway bridge below the Victoria Falls, looks almost natural ; a natural crystal may look exceedingly artificial. Hence to establish the first proposition must involve what is really an appeal to reasoned faith. If a man will not believe that a certain pattern is artificial, it is hard to make him do so; the most one can do is to point out that if it is artificial, it has a very natural interpretation.

The arguments, as they now stand, for asking the world to believe that Mars is inhabited, must always be associated with the name of Professor Percival Lowell. He built his observatory in the hope of proving his point, and on the observations which proceeded from it he has constructed an argument of high interest. His chief observations are these: The polar caps melt in the spring and a broad belt of dark colour surrounds the place where they have been. At that time the canals are invisible, but they soon begin to appear, and they appear in order, first those near the poles, then those in temperate, then those in equatorial regions. As Lowell puts it, with advancing spring in either hemisphere a wave of seasonal change sweeps down from the pole to the equator.

The second fact is that there are canals in the dark areas which were formerly supposed to be seas. In the summer they are not visible; but in autumn the dark greenish areas begin to turn brown—perhaps to dry up—and then it is seen that they are crossed by narrow dark lanes which remain dark when the regions they traverse have turned brown.

Further, it is assumed by Lowell, as a general principle, that planets tend to lose their water and to dry up. We do not think that this principle is accepted by geologists, nor does the evidence which Lowell brings forward convince us that he is right. The fact that certain districts of the earth, once well watered, are now dry desert, proves nothing. England in Silurian times was a dry desert swept by sandstorms, as the polished granite boulders testify, and since then England has been submerged a few times and is now pleasantly watered. However, not to raise objections on the threshold, we will admit that Mars may be drying up, losing water at the top of his atmosphere or absorbing it into his interior; and we will see how the explanation proceeds.

Mars, we are asked to believe, is now badly watered, and the inhabitants are put to great straits to live. Their atmosphere is thin; they have little or no cloud or rain;

they have only one source of water supply which can be utilized, and that is the flood of water released by the melting of the polar cap. The way in which the canals appear successively from the polar regions towards the equator is then quite simple. No one supposes that we see actually the canals themselves; but we are invited to believe that we see the effect of the flood, the burst of vegetation which turns the dusty fields to a deep green colour as the water is led over them. Such an effect would no doubt be visible to the inhabitants of Venus, if they could watch the Nile flood spread over the plains of lower Egypt.

The supposed proof of the existence of Martian inhabitants depends wholly upon this theory that the canals form a gigantic irrigation system, preserving the people for a time from the fate which threatened them when their crops began to fail for want of water. To the working out of this part of the theory we must devote a little space; and this is the more necessary since Professor Lowell has nowhere explained how his irrigation system would work. Yet there are several points which are, to say the least of it, somewhat obscure.

In the first place, if water is carried every spring from the pole to the equator, by the canals, it must return in the winter by the atmosphere; yet there is no evidence of sufficient cloud upon the planet to produce a heavy snowfall. On the contrary, the detail of the planet is but little obscured. Moreover, the polar cap forms so late in the winter and melts so early in the spring that it is impossible to imagine it of any considerable thickness, and very hard to believe that by its melting it can supply water for half a world.

In the second place, it is very difficult to believe in the possibility of carrying water in canals three or four thousand miles long. The waste by leakage and by evaporation would be enormous. Or if one replies that the water is carried in leak-proof covered channels, the same difficulty remains when the water is spread over the land. By hypothesis the planet is becoming drier, and has lost the greater part of its water by absorption or escape. In the absence of any evidence to the contrary, one must believe that those processes are active still. How then has the scanty supply which remains been saved from the fate of the rest, during the immense time which would have been occupied in constructing an irrigation system over the whole Martian world? No answer to this important question has ever been suggested.

Again, there is a difficulty with those dark areas which were called seas, until Lowell found that they also have their canals; these remain dark in the autumn, thin black lines across the areas which, from being dark and green, are becoming light and brown. Why waste precious water in preserving verdure at a time when things do not want watering, but only heat and drought to ripen them? It seemed to the author that this was a serious difficulty, until he realized that the purpose of irrigation in Egypt is not what he had imagined. The aim of the irrigation engineer is to supply water in spring, when neither river nor clouds give any, and by this means make it possible to grow the very valuable cotton, for which the natural water supply of Egypt is quite ill-arranged. There is, then, one way out of the difficulty. If it is alleged that the Martians have given up growing rice and have taken to growing cotton, the objector is silent.

He may, however, be quite sceptical of the engineering merit of the canals, which do not run down the meridians from the poles, but in many conspicuous cases prefer to go at a long slant towards the equator. Then comes the question of the dark spots at the junctions, which give the system an appearance like the conventional railway map. What are these round dark areas at the junctions? We are invited to believe that they are the naturally fertile regions whither people are attracted, and where they make their homes. Why then should they lie ten or a dozen in a direct line?

In fact, when one begins to examine the canal system as a working irrigation scheme one finds all kinds of features which are not to be explained as reasonably artificial. The works of man upon the earth are so restricted by the conditions which nature imposes that they must follow natural lines, and from a little distance they generally lose their crudely artificial look. But we are asked to believe that on Mars no natural obstacles have stood in the way of a perfectly geometrical design. Artificiality has grown up unchecked by natural restrictions.

To cut short an argument which may proceed too long, we must conclude that we do not, with the best intentions in the world, understand how the irrigation scheme would work. "Very well," say its advocates, "that proves only that the Martians are more intelligent than you are!" But we are loth to admit that, as the final conclusion of the whole matter, and so we can say only that there is as yet no proof at all of the actual existence of intelligent life on any world but ours.

To turn now to another part of our subject we must say a few words on the possibility whether great planets may exist, beyond the orbit of Neptune, but undiscovered by reason of their distance and faintness. It is not surprising that the romantic story of the discovery of Neptune should have fired other astronomers to emulate the achievement of Leverrier and Adams, who calculated the position in which Neptune was to be found, from the existence of unexplained irregularities in the motion of Uranus. Naturally enough the motion of Neptune has been scrutinized, in order to see whether there are traces in it of disturbance by some planet yet further away, and more than one search has been instituted, on more or less plausible grounds. Up to the present these attempts have met with no success, and it seems likely that they are premature; for Neptune has not yet completed half a revolution in his orbit, since his discovery in 1846. Another, and apparently for the time being more promising chance, is to look for further irregularities in the motion of Uranus, left unexplained after a careful examination of all that Neptune can do; but this, like the other, has led as yet to no result.

If, however, an extra-Neptunian planet exists, it is highly probable that it will be discovered some day, not by any kind of mathematical prediction, but by direct comparison of two photographs. The chart of the heavens which is now under construction by a co-operation of observatories is supposed to show all stars down to the 14th magnitude, and the plates overlap so that every region of the sky is shown twice. When adjacent overlapping plates are compared, it is not infrequent to find an object shown upon one which is not upon the other. Generally it is a variable star; sometimes it is one of the minor planets, and if the plates have been taken at only a short interval of time, the planet which has gone from one place can be found in another on the same plate. More generally the interval is too long, it has travelled too far, and wants a deal of finding. The motion of an extra-Neptunian planet must be, however, so slow that it might not be so hard to recover as a minor planet is.

The search has, so far, been conducted in and around the ecliptic; but recent discoveries of new satellites to Jupiter and Saturn suggest very forcibly that the outer bodies belonging to a system may deviate very greatly from the rules which govern the inner. Quite the most unexpected discovery of recent years has been the finding of very distant faint satellites whose paths are very excentric, very much inclined to the plane of the planet's equator and of the inner satellites, and in two cases are pursued in a retrograde direction. That all the planets revolved in the same direction round the sun, and each family of satellites in the same direction round their respective primaries, was supposed, until a few years ago, to be one of the canons of astronomy, inevitable according to the nebular hypothesis, and necessary to the stability of the solar system. In the year 1905 the discovery that a new distant satellite of Saturn, named Phœbe, moving in a direction opposite to that of the others, excited great enthusiasm. The satellite had been announced by Professor W. H. Pickering, of Harvard, some years before. Nothing further had been heard of it, and its very existence became doubted, when an English astronomer visiting Harvard brought back the news that a great surprise was in store. It came very quickly in the announcement that Phœbe's motion was retrograde. About the same time Dr. Perrine, at the Lick Observatory, discovered first one, and then another, new satellite of Jupiter, lying in paths far outside the older ones, highly inclined, nearly equal, and interlaced. They were put under observation at Greenwich, and in February, 1908, Mr. Melotte, of the Royal Observatory staff, discovered another, which, like Phœbe, moved in a retrograde orbit.

These capital discoveries are due to photography. The small bodies discovered are barely visible in the largest telescopes, and they lie so far from their planets that they are outside the range of ordinary observation. It is highly improbable that they would ever have been found visually, while to obtain photographs of them is arduous in the extreme. But it is imperative that every effort should be made to get observed places of Jupiter's eighth satellite, for its motion offers the most curious and fascinating problems. Of any other satellite one may say that it moves in an ellipse, and continues to revolve nearly in the same orbit for ever. Small modifications will be caused by the influence of the sun and the other planets, and the other satellites, but these changes are not large, and they balance one another after a time ; the motion is said to be stable, which means in effect that the planet has an absolutely controlling influence over its satellite. The eighth satellite of Jupiter is so far away from him that this can scarcely be true for it. We may say that the satellite is always trying to get away from Jupiter and move round the sun as a planet—which is true of all satellites—but in this one case it comes pretty near to succeeding. The result of this is that the satellite describes a highly irregular curve which does not repeat itself in the least, and which defies all the usual methods of calculation. Very fortunately the remarkable methods which Cowell and Crommelin had applied with so much success to the motion of Halley's comet were all in working order ready to tackle the new satellite, which they have done with complete success.

Having spent so much of a limited space

on the new and doubly fascinating because quite unexpected discoveries of these distant satellites, we have but a few lines to give to the old. The theory of their motions, like that of our own satellite, is exquisitely complicated, and for that reason hardly to be touched upon here. As to their physical appearance, there is not very much to be said. Only the four satellites of Jupiter discovered by Galileo are large enough to show any visible disc. From the little that can be seen, it appears that they are belted, and that they rotate on their axes in the same time that they revolve about Jupiter. Whether, like our moon, they are barren and airless, has yet to be determined. As objects for the smallest telescope, these four satellites have an unequalled interest. Their eclipses by the shadow, their passages before and behind the planet, their ever varying groupings, provide for us, as they did for Galileo, an object-lesson in celestial motions, which all may see and which none should miss. There are few to-day who would miss them deliberately, lest they should be compelled to believe what they are anxious not to believe. One can but hope with Galileo that his obstinate opponents saw them on their way to Heaven.

CHAPTER IV

COMETS AND METEORS

A COMET, and an expected shower of meteors have been responsible in recent years for two great public disappointments. The famous Leonid meteors, which in 1799, in 1833, and in 1866 provided displays of a grandeur never forgotten by those privileged to see them, were due to return in 1899 or 1900, and they failed to appear in any considerable numbers. Halley's comet—famous in the history of the world, and not less renowned in the particular history of astronomy—was awaited with a degree of interest without parallel in our recollection. In Britain and in northern latitudes generally, it proved a miserable object, and it was hard to credit the accounts cabled from the south, of the tail of the comet stretching from horizon to zenith like a gigantic searchlight.

The great public interest which Halley's comet aroused, the extraordinary precision of the prediction of its return, and the remarkable work done by Cowell and Crommelin in tracing its past history, render it inevitable that we should make this comet our leading theme in a chapter written before it has yet passed completely from our sight. Halley's comet is unique. There are many comets which have made a splendid appearance, and have passed never to return, or to return after so many thousand years that we may as well say never. There are many comets which return regularly to the sun at intervals of comparatively few years; but they are faint and undistinguished things, hardly, if at all, visible to the unaided eye, and never likely to cause public excitement or to be taken as heralds of war and the death of kings. Halley's is the one comet which is periodic, coming back again and again, and is at the same time a magnificent object, a comet of the first class, worthy to play the spectacular part which it has in fact played for more than two thousand years of history.

Halley, by whose name the comet is known, was a man who, more than most, deserves a biography—which has not yet been written. The scattered references to him in the early history of the Royal Society picture for us a man of excellent capacity, energy, and common sense—not a profound philosopher, but one of those equally valuable people who have the faculty of undertaking the right thing and making a success of it. At an early age he was sent in command of one of his Majesty's ships on a long voyage to study the deviation of the compass in different parts of the world. On his return he was employed by the Royal Society to measure an arc of meridian, for determining the size of the earth. Later we find him Secretary of the Royal Society, active in promoting enquiry of all kinds, and particularly interested in the problem which was then unsolved, what causes a planet to describe an ellipse about the sun. Halley made a journey to Cambridge in the confidence that if anyone could throw light upon the matter it would be Newton; to his delight he found upon arrival that Newton was already in possession of the theory. But that eminent philosopher does not seem to have had any ambition to publish his great discovery to the world. It was Halley who urged upon him the importance of writing the book which became the immortal "Principia." It was Halley who persuaded the Royal Society to publish the book, Halley who paid for it out of his own pocket, and Halley who occupied himself with applying the new theory of gravitation to calculating the orbits of comets, while Newton had become Master of the Mint and was refusing to "be dunned and teased by foreigners about mathematical things, or to be thought by our own people to be trifling away my time about them, when I should be about the King's business."

It must be remembered that when Halley began his work, no one had the least idea that a comet could reappear after a period of years. Newton himself had actually scouted the suggestion that a comet seen on one side of the sun might be identical with another seen some weeks later on the other side. Halley calculated the paths of twenty-four comets—all that he could find sufficiently observed—and of these three moved in orbits which were very nearly the same. Was it possible that they were the same comet returning again and again to the sun? There was a grave difficulty in believing it possible. The intervals between the successive returns were not equal. Between 1682, September 4, and 1607, October 16, was a period of 74 years 11 months: between the latter date and 1531, August 24, was 76 years and 2 months. Now, if a comet returned again and again to the sun, it must be to all intents and purposes a planet; and if so, why did it not perform its revolution in a regular time? In seeing the answer to this difficulty Halley showed how keen was his intelligence. Jupiter and Saturn disturb the revolutions of one another to the extent of a few days only, for they are massive bodies to perturb. A comet is lighter and sensitive to disturbance, easily captured and easily turned adrift; it might be the attractions of the planets which caused the irregularities in its re-appearance. Satisfied with this solution of the difficulty, Halley staked his reputation upon a prediction that the comet would return again in 1758. He knew that he could never live to see the truth of his prediction; but in a fine sentence, often quoted, he expressed himself content with the faith that impartial posterity would not refuse to admit that this was first discovered by an Englishman.

Two circumstances of the last return are noteworthy. So accurate was the prediction of Cowell and Crommelin, that the comet was found within two seconds of arc of its predicted place, on the first photograph which showed it in 1909, August. It had made the most rapid revolution on record. Between 1759, March, and 1835, November, was 76 years 8 months. If it had traversed exactly the same path again, it would not have been due at perihelion until 1912, July; whereas it actually reached perihelion in 1910, April. This acceleration of 27 months was explained as the effect of a near approach to Jupiter in 1835. The disturbance then impressed upon the path of the comet had little time in which to affect the date of the 1835 return, but its effect was felt throughout the succeeding revolution, with the result that the comet made its record passage, as we have said. Its actual return to perihelion, as observed in 1910, differed by 2.7 days from the prediction —an insignificant error, one might suppose, in so immense a prediction. But the authors were not satisfied. They repeatedly checked their work, and they are able to affirm that if the theory of gravitation is rigorously true, and no forces except gravitation act upon the motion of the comet, then not more than a fraction of a day can be attributed to defects of calculation. The balance of some 2½ days can be explained only by the existence of forces which are not pure gravitation. It is not the first time, as we have seen, that a reputation has been staked upon the comet. We cannot but hope that the faith of Messrs. Cowell and Crommelin will be justified as fully as was the confidence of Halley in his prediction.

The second circumstance which made the last return of Halley's comet noteworthy was the passage of the comet across the plane of the earth's orbit at a point exactly between the earth and the sun. If the comet's head were large and solid, we should have seen it projected as a dark body against the sun; but nothing was seen; nothing was photographed. If the comet's tail had been straight, the earth would have passed through it, and this probability gave rise to a quite remarkable excitement. People who should have known better predicted that we should be asphyxiated by poisonous gases. Some cautious people laid in bottles of oxygen to sustain life during the fatal night. One or two committed suicide to escape death from the comet. And after all, nothing very much happened to the earth, perhaps because the tail was not straight.

There were in fact at least two tails. As the time for the passage across the sun approached, the comet in the morning sky, before sunrise, became each day a more wonderful object to those observers, south of the Mediterranean, who had the good fortune to see it in a clear dark sky. As the comet closed up to the sun its tail grew longer till it reached the zenith. The day of the transit passed, and next evening the tail was seen, as was expected, in the west. But the following morning, quite unexpectedly, it was still visible in the east. There must have been two tails, and that night the earth was between the two.

Before we turn to the discussion of other comets, let us glance for a few moments at some features of interest in the work of tracing back into the remote past the history of Halley's. We have seen already that the comet returns after periods which vary by a couple of years on each side of seventy-six years. That makes it impossible to go back by a simple series of subtractions, to say that Halley's comet should have appeared in such and such a year, and to search the chronicles for records of a comet that will more or less fit the date. We have, in fact, to perform two distinct operations. First, from the recorded path of a comet among the stars, we must calculate its orbit and see if its position can be reconciled with the path of Halley's. Next, having made a preliminary identification, we must examine whether the actions of the planets can account for a return at that particular date. For the first step the European chronicles are useless. They tell that a comet appeared, and they usually attribute to it whatever disaster, pestilence, or illustrious death took place about that time. But they tell nothing of how the comet moved among the stars; they give us no details by which we can calculate even roughly what orbit the comet pursued. To gain the information we want for this purpose we must go to the Chinese annals, the dynastic histories and the great encyclopædias of ancient wisdom. From these records Hind was able to trace the appearances of comets which might very well have been Halley's, as far back as 11 B.C. It remained for Cowell and Crommelin to examine whether these dates were consistent with the calculated duration of each revolution, when perturbations by the planets are taken into account. This formidable task they completed with success, showing that Hind's identifications were nearly all correct—that it was Halley's comet which is represented in the famous panel of the Bayeux Tapestry; that it was Halley's comet which is pictured in the Nuremberg Chronicle for the year A.D. 684; that it appeared about the time of the battle where Attila the Hun was defeated by the Romans and the Goths. Halley's comet was the sign in the shape of a sword which hung over Jerusalem before the destruction of the city by Titus; it hung over Rome in 11 B.C. before the death of Agrippa.

Thus are comets remembered in history by the disasters which are ascribed to them, and the terror which they caused, when it was supposed that they swept through the upper air, almost within reach. By proving that at any rate they are far beyond the moon. Tycho Brahe rendered the world a service for which mankind should be grateful—those who have not still a lurking dread of what they fear a comet may bring. These were the reflections proper to the year 1909, when Halley's comet was approaching—a quiet satisfaction that we lived in enlightened days. The year 1910 came; in January the people of Paris watched the so-called "Daylight comet" reflected in the flood which threatened to destroy their city; in May, while the people of London watched for Halley's comet, the body of King Edward the Seventh lay in state at Westminster. What wonder that the imagination seizes upon these deplorable coincidences, and the fear of comets dies hard among us.

Leaving now the historical side, let us see how far it is possible to understand what a comet is, and how its extraordinary phenomena are produced. The formation of the tail was well shown in the comet of 1882, which passed so close to the sun that it nearly grazed the solar surface. In three and a half hours the comet described 180 degrees of its orbit. One morning the tail stretched a hundred million miles in the east; the same evening it was a hundred million miles long in the opposite direction. Now it is impossible to suppose that the head of the comet swung that tail round through 180° in a few hours; it is certain that the comet discarded its old tail and made a new one. With the advent of photography it became possible to study the tail in greater detail and more completely. Bright patches in the tail, photographed on one night, were replaced by other patches on the following night, but it was impossible to trace any detailed connection between one series and the other. Very often a comet is so near the sun that not more than one picture can be obtained each night. But occasionally a comet may be photographed several times in one night, and then it becomes possible to see what is really happening in the tail. The bright patches are travelling very fast along the tail, in the direction away from the sun. They tell us that the whole tail is being renewed continually by some influence proceeding from the sun, and that every comet's tail is practically new each day.

To explain such a surprising fact is not easy, but we have now at least two possible explanations, whereas a few years ago one could say only vaguely "electric repulsion," which did not work out very well. Nowadays one can speak with some confidence of the part which the pressure of light may play in the formation of comets' tails, for did not an American professor come to Cambridge with a comet's tail sealed up in a glass tube? By a beautiful piece of experimenting Messrs. Nichols and Hull were able to show in actual operation the process that Maxwell had seen with the mathematical eye, thirty or forty years before. The experiment was this: the finest sand, and a very fine light vegetable powder, lycopodium, were mixed, and put into the top compartment of a tube like an hour-glass, which was then exhausted of air. Through a small aperture the stream of powder could trickle into the lower compartment of the glass, across which a concentrated beam of light was passed. The dense, comparatively large grains of sand dropped through without deflection, but the smaller and lighter lycopodium was swept against the side of the tube by the pressure of the light falling on it.

Now it may be that a comet's tail is formed in this way. Light pressure would account for the fact that the comet's tail is always turned away from the sun, the powerful source of light. As the comet approaches, the tail streams behind; as the comet recedes, the tail goes before; so that evidently the source of repulsion lies in the sun. But light pressure does not explain how the comet's nucleus produces the fine dust, in the first instance—a part of the question to which perhaps insufficient attention has been given. In the second place, light pressure does not explain why or how the dust can remain luminous during the hours in which it is being rushed through space literally on the wings of light. And thirdly, light pressure does not fit in particularly well with the measures which have been made of the actual accelerations of bright patches along the tail. In fact, the explanation by light pressure does not work out in detail quite so well as had been hoped.

The rival theory, which has some brilliant possibilities, supposes that the comet's tail does not really belong to the comet, but is a local lighting up of matter which is continually streaming out in all directions from the sun—it may be dust, or it may be in the form of one of those subdivisions of the formerly indivisible atom, of which astronomers are as yet justly a little shy. If it be dust, there is no difficulty about the formation of the dust, for the neighbourhood of the sun must be all smothered with impalpable powder, the condensation of the metallic vapours so frequently ejected. The dust which is about the right size—one-third of the wave length of the light producing the pressure—must be sorted out and driven away. Very probably at the start of its journey it has something to do with the long streamers of the corona. Then it ceases to be visible unless something excites it, and—here we have the explanation of comets' tails—the exciting cause may be the passage through the shoal of meteoric stones which is supposed to constitute the comet's head. This theory explains some things rather well—but it would seem to require that all bodies in the solar system should have tails. In that case, we are answered, no doubt the "Gegenschein"—the faint patch of light opposite the sun—is the earth's tail, which we can faintly see because we are looking along its length.

A weak part of both explanations is that they do not help very well to explain the fountain-like envelopes that are seen, in the telescope, surrounding the bright nucleus of a good comet. These envelopes were particularly remarkable in the head of the great comet of January, 1910. At first sight they suggested very strongly that fountains of brilliant particles were ejected towards the sun from the nucleus, and meeting the pressure of light were deflected and turned back into the comet's tail. A very complete study of this action has been made by Eddington from the wonderful series of photographs of Comet Morehouse (1908) made at Greenwich with the thirty-inch reflector. His conclusion is that the explanation is certainly incomplete. Calculation shows that very variable and very large light pressures are required, while we find no sufficient explanation of the multiple envelopes. In fact, with our present limited knowledge it is difficult to make the theory work; while it is a serious embarrassment to remember that light pressure has no effect upon gas, but only upon solid particles; for the evidence of the spectroscope certainly seems to show that the tails are gaseous.

Now let us turn to the composition of comets. It is generally supposed that the nucleus of a comet very much resembles a swarm of meteorites. The connection between comets and meteor streams is certain. Several well known streams of meteors travel in orbits which are identical with the orbits of known comets. In one celebrated case a comet has been seen to break up and disappear, and afterwards a bright shower of meteors came from the path in which the comet had moved. Yet it may be putting the connection too positively to state, as it has been stated, that "Biela's comet was shedding over us the products of its disintegration." There does not seem to be any definite evidence that the material which makes the comet's nucleus is the same as the material which flashes into the atmosphere of the earth and blazes for a moment as a shooting star. Indeed the spectroscope shows that the light proceeding from the nucleus is generally, or perhaps always, the light emitted by the vapours of hydro-carbons; while Fowler, in a most successful series of experiments made recently at South Kensington, has been able to produce in the laboratory a spectrum exactly corresponding to the spectra exhibited by many comets' tails, and has been able to show that it belongs in all probability to an oxide of carbon. In brief, then, the light of comets proceeds chiefly from certain compounds of carbon; and it cannot be said that we have as yet any precise idea how these compounds are carried by meteors in the comet's head, nor yet why light pressure or any other form of radiation from the sun should be capable of lighting up the tail in so magnificent a way. When a comet goes very near the sun, we can imagine all kinds of violent actions upon it. But many comets—Halley's for example—do not approach the sun very much nearer than does the earth. How then, by what mechanism, by what internal energy, or by what method of extracting energy from solar radiation of any kind, does a comet shine with so brilliant a light, when planets may pass through such radiation without experiencing any very noticeable effects? In spite of all the work which has been done upon comets in the last few years, it does not seem that we are really much nearer to a solution of this mystery. We want a brilliant comet that shall be visible all night for several months, so that there shall be some opportunity of continuous work. It happens too often that an astronomer gets an hour's observation, under rather poor conditions, with the comet low down in the vapours after sunset or before sunrise. That hour's work shows him what he should do next, suggests the questions demanding solution. Then perhaps there is a week of cloudy weather, and the comet is gone. Comet interests are put aside, other work is taken up, and suddenly after ten years another comet demands instant attention with everything in complete order for its observation. Under such circumstances progress is necessarily slow.

We are speaking of brilliant comets, in which it is possible to see some structure, and which give light enough for analysis. The ordinary small telescopic comet, of which we have four or five a year, has little interest except the interest of calculating its path, finding whether it belongs to the solar system or is a casual visitor, and watching to see that it strictly obeys the law of gravitation. That is to say, its interest is not physical, but almost entirely geometrical. It may be of interest to describe the history of a typical telescopic comet, so far as we are concerned with it.

A few astronomers devote clear nights when the moon is absent to comet-seeking. The more fortunate have an instrument specially designed for the purpose, which allows an observer to sweep broad areas of sky without changing his position. He sweeps and picks up nebulæ, which must be identified from the catalogues or observed until it is certain that they are not moving among the stars. After wasting very many hours with nebulæ, a fortunate observer discovers a faint object, almost invisible, which is not catalogued as a nebula, and which in the course of an hour has moved perceptibly among the stars. It is a comet. The observations made at the beginning and end of the hour serve to give the position of the comet at the instant of the first observation, and a first rough estimate of the daily motion. A hasty search through the astronomical journals shows that it is not a comet already announced, or one whose return is expected. It may be new, and it may have been discovered by another observer the night before.

The observer's duty is to communicate his discovery to other astronomers as soon as possible, that there may be no delay in getting to work. A telegram is sent to Kiel, where there is an organization for circulating astronomical news without delay, to all subscribers, in a special code designed for the purpose. Should the discovery have been made in the early hours of the night it is actually possible to send the news to Kiel, get out the code cable to America, and have the comet observed that same evening at Lick. Within a few hours, then, other observations of the comet may be secured, and cabled to Kiel, which sends out again the earliest second observation received; and all the subscribers have now two places of the comet available. Directly a third is observed the comet's orbit can be calculated; so there is some competition to get the third observation. When it is obtained the fortunate observer should be prepared to call up all available assistance and to proceed straightway with the calculation of a parabolic orbit. Eight or ten hours' continuous work will finish it, and perhaps provide a little ephemeris or prediction for the next few days. Then for the first time it is possible to say what will happen—whether the comet shows any promise of becoming brilliant, whether it will be visible for long, and what constellations it will traverse. The elements—that is to say the numbers which define its orbit—and the ephemeris—the numbers which define its apparent path—are telegraphed to Kiel, and sent out this time upon a "jellygraphed" post-card.

Time passes; the comet is passing beyond our range of observation; and there is some competition to secure the last observation. In a few months all the observations obtained are published in the various astronomical journals, and it is time for some one to announce that he proposes to undertake a definitive determination of the orbit—a determination, that is to say, which shall take account of all the niceties which were neglected in the first hurried calculations. The definitive orbit tells us whether the comet belongs to the family which have been captured for the sun by Jupiter, and it provides the means of rediscovering the comet at its next return, perhaps some five or six years afterwards. Before the next return a "search ephemeris" is calculated and published as a guide to rediscovery. Again there is some emulation, to be the first to pick up the faint object as it approaches the sun and grows gradually brighter.

The interest in these faint comets is thus almost entirely mathematical. They furnish beautiful exercises in calculation and admirable tests of the completeness of theory.

Once in a while a comet will be lost—it has made a near approach to Jupiter or Saturn, and has been deflected violently from its path: sent out of the system by a reversal of the process which originally captured it from outer space, or turned into a new path having so little affinity with the old that only a complicated calculation could show the connection. Thus these faint comets afford puzzles and problems which are some of the most elegant in astronomy. In particular they become beautifully intricate in such a case as that of the celebrated comet Encke, which approaches very close to the planet Mercury, and suffers besides from an acceleration to its motion, exceedingly difficult to explain—especially since it has been shown by the elaborate investigations of Backlund that the disturbance has changed its value suddenly three times in the last century. But we have no space to deal with the problem of Encke's comet. We must pass to the meteor showers, with which, as we have seen, comets are closely allied.

The point which strikes us first as we consider the question of meteors, is that we see only the final catastrophe, and know nothing except by inference of the events that lead up to it. A bright streak of light across the sky, perhaps if the meteor is large a train of sparks which persists for a few seconds, and the career of the meteor is ended. It volatilizes with the heat engendered by its rush into the air, and eventually settles down upon earth as the fine meteoric dust which has been recognized in samples from the deep sea bottom. One might think it hard to discover very much from an observation so momentary; but by patient observation and comparison of results it is possible to deduce a good deal. The important fact to be observed is the direction of flight, which must be plotted on a star map or globe. As tracks accumulate it is seen that they are not at random. A good proportion of those observed on any one night, or on corresponding nights in different years will be found to radiate from a single point or small area. From this we draw the conclusion that the meteors, before they collided with the earth, had been moving in parallel paths, coming towards us from that point of the sky in which the radiant is situated. We have now information as to the orbit of the meteors: it passes through the point which the earth occupied at the time of observation —which can be taken out of the tables— and it passes through that point in a certain direction, given by our observations. Without going further into the details, we may say that in some cases, though not in all, it is now possible to calculate the actual orbit of the meteors. Thus, Newton (of New Haven, Conn., U.S.A.), in the middle of last century, found that magnificent showers of meteors were recorded in history at intervals of 33 years, identified them with the shower of Leonid meteors—that is, meteors radiating from the constellation Leo—and predicted their return in 1866. This prediction was a brilliant success. An immense shower of meteors was seen in November of that year, and a scarcely inferior display in the following November. Newton showed that there were five possible orbits in which they might be travelling; Adams proved that the largest orbit was the true one; and Leverrier found that this path took them so close to the planet Uranus in the year A.D. 126, that it is very probable that the whole shower was captured by Uranus and introduced to the solar system at that date.

Everything being thus completely worked out, one expected with some confidence a return of the great shower in 1899 or 1900. As the time approached, however, some calculations published by Johnstone-Stoney and Downing suggested the probability that the meteor stream might have been perturbed so much by the action of Jupiter that its path would no longer intersect the path of the earth, and thus we should have no display. It was impossible to be certain, for there was no means of estimating the size of the swarm, and the prediction of failure did not attract as much attention as did the general expectation of a grand display. Hence, when the shower provided no more than the usual scattering of meteors seen every November, there was great disappointment, and some mockery of the science which claims to be the most exact of all sciences. As to the mockery, it is often forgotten that many astronomical events cannot be predicted with the certainty of an eclipse or an occultation. They are expected, in the sense that it advisable to look out for them; the expectation is, or should be, accompanied by a clear statement of the certainty or the probability of failure involved. Doubtless the warning will be neglected; the newspapers will magnify the expectation, and there will be disappointment, as there was with Halley's comet and with the meteors. All that the astronomical prophet can do is to be moderate in his promises, and cautious how he excites public interest. Whether in these two cases he was not indiscreetly confident is perhaps arguable.

In comet-seeking and in meteor-observing there is a wide field for the enthusiastic non-professional astronomer. Now that there is no longer a substantial prize in money for each comet discovered, it is not possible to subsist upon comet discovery, as a very distinguished astronomer did for some time, in his amateur youth. But the possibility of attaching one's name for ever to a well remembered comet must stimulate enthusiasm, and is in truth a very real though unsubstantial reward. How much was the fame of Donati increased by his discovery of the comet which at first promised badly, but manifested itself in the end as the most beautiful of its kind. In a less ambitious way, but one which offers more continuous employment, the amateur observer, with no equipment but a star map, can do very valuable work in the observation of meteors. There are still important matters to be settled, and some doubts to be resolved. Take, for example, the question of the "stationary" radiant points. If a shower of meteors is active on several successive nights, the radiant point should, in theory, shift a little each night. The August meteors from Perseus do show this shift. It is alleged by the best observers that other showers do not, but that their radiant points, in defiance of theory, remain stationary. Now this is a question which must be settled; one of the principal difficulties in so doing is to decide what constitutes a radiant point. Two meteors cannot, for their paths produced backwards must meet in some point. A third passing through the same point, suggests a real radiant, but it is much more difficult to decide if the third passes, say, two degrees away from the intersection. How much may we allow for errors of observation, and how much for a possibly real diffuseness of the radiation? That involves questions which are not easy to answer, and it is certain that there is great scope for the patient observer of shooting-stars, in elucidating this curious difficulty.

One word more, as to the nature of meteors, or shooting-stars. We have heard of the novelist who made her hero notice the absence of a star, and remember that he had seen one fall the night before! In Cambridge a college porter, asked to be on the look-out in 1899, reported to a festive party of watchers indoors that "they had none of them shot yet, but some of them looked as if they were just going to." We may laugh at this confusion, and at the same time be surprised at the minute size of the bodies which can produce for the moment so brilliant an effect. The ordinary shooting-star weighs not more than a grain, but it enters the atmosphere with a planetary velocity, and in a brief instant its energy of motion is all changed by friction of the air into energy of light and heat. And bearing this in mind we may see in each meteor an illustration of the principle which we invoked to explain the maintenance of the sun's heat, that arrest of motion means the production of heat. It may produce brilliant incandescence in a meteor if the motion is swift and the stoppage sudden; and the effect is none the less sure in a body contracting under the influence of its gravitation, though the motion is slow and the stoppage gradual.

Occasionally a meteor of large size, entering the atmosphere slowly, because it is overtaking the earth, will escape vaporization, and will fall to the ground. It can then be examined and analyzed, and in a great number of cases this has been done. In no case has there been any evidence of the presence of an element not found in the crust of the earth. Space then, if filled with flying fragments of matter, is filled with quite ordinary materials. Whether they have been thrown out in primitive days from the volcanoes of the moon, whether they come from the sun or from the distant stars, or whether they are merely fragments of stuff which had escaped absorption into a larger body, and had wandered indefinitely in space, they tell us at any rate that we have in the earth most if not all of the elements which occur throughout the universe.

CHAPTER V

MOVEMENT UNDER THE LAWS OF GRAVITY

THE claim of astronomy to be considered the most exact of all the sciences rests, in popular acceptation, upon the seemingly miraculous way in which the astronomer makes predictions which come true to the second. At the moment of writing several parties of astronomers are on their way to a small island in the Pacific Ocean, to observe a total eclipse of the sun. They know the moment when the shadow of the moon will come sweeping across the sea and strike them, and they know for how many seconds it will envelop them. If the eclipse were due a hundred years hence instead of within a few weeks of the time of writing, the prediction could be made with almost equal accuracy; while if it should by chance be a matter of historical interest to determine whether an eclipse of the sun were visible in that island two thousand years ago it could be done.

Now let us consider for a moment what this involves. The moon is moving round the earth, and both together are moving round the sun. At each instant the motion of the moon, which were it not for the sun would be very nearly a simple elliptic motion round the earth, or were it not for the earth would be very nearly a simple elliptic motion round the sun, is distracted by the competing attractions of both, and by the feebler but by no means negligible disturbances produced by the attractions of the planets. The task of the mathematical astronomer has been this, that starting with the simple Newtonian law of gravitation—every particle in the universe attracts every other particle with a force depending directly upon the product of the masses of those particles, and inversely as the square of the distance between them—he has to disentangle this infinite number of actions, to calculate the path which each body will describe, and the way in which it will be gradually modified with the lapse of time. He must weigh each body by studying the disturbance it produces in the motion of other bodies; he must reduce the whole infinitely complex and laborious calculation to such a shape that it can be performed by companies of calculators who do not understand the theory of what they are doing, but perform an array of specified operations according to fixed rules. Finally, he must organize this arithmetical machine so that it can produce year by year, three or four years in advance, the numerical boiling down of all this mathematics and arithmetic in the form of a Nautical Almanac—or as it is more properly termed, an Astronomical Ephemeris or prediction. There is a forcible passage in Newcomb's *Reminiscences of an Astronomer* which we may quote: "A more hopeless problem than this could not be presented to the ordinary human intellect. There are tens of thousands of men who could be successful in all the ordinary walks of life, hundreds who could wield empires, thousands who could gain wealth, for one who could take up this astronomical problem with any hope of success. The men who have done it are therefore in intellect the select few of the human race—an aristocracy ranking above all others in the scale of being The astronomical ephemeris is the last practical outcome of their productive genius."

In its most general form the "problem of three bodies" is insoluble by any mathematical processes that have yet been invented; in other words, if only the three bodies, the sun, the moon, and the earth existed, it would still be impossible to obtain a solution of the problem—what will be the motions of these three bodies under the influence of their mutual attractions. Still more, then, is the problem insoluble when, instead of three, we have a great number of bodies involved. There are, however, in the solar system certain conditions which greatly modify the impracticable nature of the general problem, and as very often happens, it becomes possible by special devices to obtain the solution of particular cases of the problem, even though we recognize that its most general case is too complex to deal with. For example, the sun is very much more massive than any of his planets; the planets are at great distances from the sun; they move in paths which are approximately though not accurately circles; and they all move round the sun in the same direction. All these special conditions simplify the problem so immensely that it is found possible to construct a theory which can deal in a laborious but sufficiently accurate and thorough manner with the actual problems that are presented to us in the solar system.

The principal solutions which have been obtained are respectively the Lunar Theory, dealing with the motion of the moon, and the Planetary Theory, which can be applied separately, with suitable modifications, to the motions of the planets.

Without the special symbolical language of mathematics one can hardly hope to indicate in a general kind of way what is the nature of these theories. To say that they are all developments in infinite series does not perhaps take us very far, yet it is not difficult to understand the meaning of this brief explanation. Let us suppose that we are dealing with the longitude of the moon; we wish to construct a mathematical expression which shall so represent this quantity that we can calculate it for any given instant of time, and obtain our result, the longitude of the moon for that instant, expressed in degrees, minutes, and seconds of arc. Now if the problem were really completely solved, we should have only a limited number of terms in the expression which we are going to calculate. The problem is really insoluble, and we cannot set down that limited number of terms, but several different ways have been found of putting the solution into an infinite number of terms. Everything then depends on the consideration: Do these terms, as we take them one by one in the series, get rapidly smaller and smaller, so that although their number is really infinite, we can stop at a certain point in the confidence that no terms which come afterwards are large enough to be appreciable ?

We shall readily believe that the decision, whether we may or may not stop at a certain point, involves questions of the most delicate nature. Who is to say that, although the second thousand terms may be negligible, there is not one among the third thousand that will be quite large enough to matter ? And again, though all the terms after a certain point may be each extremely small, their number is infinite ; and how shall one be sure that the sum of an infinitely large number of very small quantities may not be really quite large ?

It must be sufficient to say here that within the last few years, an apparently satisfactory answer to these questions has been found. From the enormous amount of work done on the problem during the nineteenth century three separate and considerably different ways of dealing with the Lunar Theory emerged ; they are known by the names of

Hansen, Delaunay, and Hill. The two former had been worked out in complete arithmetical shape for many years; and after long trial they had been found insufficient. It remained to see what could be done with the later and considerably different way of dealing with the problem devised by the American mathematician, G. W. Hill. The enormous work of putting his mathematics into arithmetical form was undertaken by E. W. Brown, a Cambridge mathematician who became Professor of Mathematics at Haverford, U.S.A., and is now at Yale. After fifteen years of unremitting labour his task is now complete. Beyond question the complete solution of the motion of the moon under gravitation has been obtained; and the suggestion has been made recently on the highest authority that it will be unnecessary for Greenwich Observatory to spend in the future so much energy on observing the moon at all hours, convenient and inconvenient; for it may be supposed that we know more of the minute irregularities of her motion, from the Hill-Brown theory, than we could possibly observe.

Apart from its importance in providing secure data for the Nautical Almanacs, a complete Lunar Theory enables one to examine one of the most fascinating of all scientific questions. Is Newton's law of gravitation rigorously true? In the absence of any sufficient physical explanation of the *cause* of gravitational attraction—which remains almost completely unknown—there is no convincing reason to be given, why the attraction should vary exactly as the square of the distance between the attracting bodies. Instead of having distance to the power of two, might not the true law be distance to the power of (two + a very small quantity). This curious possibility has been examined several times, and always with a negative result. The last examination is due to Brown, who showed that we cannot substitute for the simple 2 a quantity greater than 2·00000004, without getting into more difficulties on one side than we escape on the other. And as it is a good principle to accept the simplest explanation which is consistent with known facts, there seems to be every reason to believe that the simple square law is exact.

But on the other hand there is no longer any reason to doubt that gravitation alone is incompetent to explain completely the observed motion of the moon.

Some thirty years ago the laborious researches of Newcomb made it clear that the moon never had, to that day, conformed to the programme devised for her by the lunar theorists; but then we had no solid assurance that the theory was without doubt complete. Now we have that assurance, and still our wayward satellite refuses to conform. At the end of the eighteenth century the moon was fourteen seconds of arc ahead; by the end of the nineteenth she was ten seconds behind her calculated place. "I regard these fluctuations," says Newcomb in his last paper on the subject, "as the most enigmatical phenomenon presented by the celestial motions, being so difficult to account for by the action of any known causes, that we cannot but suspect them to arise from some action in nature hitherto unknown." Several fascinating possibilities are examined, only to be rejected. Is the fluctuation only apparent, not due to the moon at all, but caused by a trifling fluctuation in the speed of rotation of the earth, so that our measure of time is now about 9 s. fast—for that is all that is required? If so, the transit of Mercury across the sun's disc in 1907 should have shown it; but Newcomb has worked up the observations of that transit, and finds that if anything they require that our time should have been slow, not fast. Is it possible to suppose that the force of gravitation between the moon and earth is itself not constant, but subject to slight fluctuations, dependent perhaps on the varying magnetic activity of the sun ? That, says Newcomb, would be the simplest sort of action to explain all the phenomena, but he cannot find any justification for it. "At present," he concludes, "I see nothing more to do than to invite the attention of investigators to this most curious subject."

The motion of the moon—or the accuracy of our knowledge of it—may be tested in another way which is full of interest, and which has of late years received much attention. Ancient historians and poets have recorded the occurrence of total eclipses of the sun—events so striking and awe-inspiring that they naturally made a profound effect on spectators and were inevitably associated with whatever important business might have been on hand at the time. Indeed it is not a little remarkable how many decisive events seem to have been accompanied by eclipses of the sun, and this has even led to the suspicion that historians, writing long after the event, were tempted to embroider their narratives with any picturesque details that seemed appropriate. Such a suspicion does but add zest to an enquiry which has been pursued with great industry and remarkable success. History records that more than 300 years before Christ the tyrant Agathocles left Syracuse by sea, and on the second day of his voyage an eclipse of the sun enabled him to escape his enemies. Whether he sailed round Sicily to the north or the south is not recorded; but it is not improbable that astronomers will one day settle the question decisively. For the present, however, the interest lies in the test which ancient eclipses afford of the uniform movement of the earth and the moon throughout historic time. Is it possible for example by calculating back to explain a Babylonian inscription which was translated at the British Museum in 1905? "On the 26th day of the month Sivan, in the 7th year, the day was turned into night, and fire in the midst of heaven." That is to say, there was a total eclipse, and the fire was, of course, the corona, observed in the year 1062 B.C. A remarkable controversy has raged lately over these ancient eclipses — a controversy which has shown that mathematicians may be learned historians, and historians may develop unexpected mathematical talent. The work of Cowell, Nevill, and Fotheringham has ranged over Babylonian, Assyrian, Greek, Egyptian, and Chinese records, but the complexity of the puzzles which these records have presented has not altogether defied solution, though the solution is of an unexpected kind. It is found by Cowell that the tangle can be straightened out if we suppose that the length of the day is increasing at the rate of one-two-hundredth of a second per century! Nor is such a change insignificant; on the contrary it is ten times as large as what had before been considered possible, and its effect, gradually accumulating through centuries, is sufficient to bring into line with modern theory a whole series of eclipses of two thousand years ago. And it is needless to point out how much depends upon this question. The ultimate test of the truth of our theories of the celestial motions is the power to predict the future and to satisfy the records of the past. When we are concerned with an exceedingly slow change, which takes many hundreds of years to develop its effects, we cannot afford to wait for the future developments, but must be content to do our best with the past; and it is all important that we should be certain of our measure of time. Otherwise we shall find it very difficult to decide whether a slow divergence from theory is to be attributed to defects in our theories or to our having no constant standard of time by which to measure.

But how, it will be asked, should the length of the day increase, and why should the earth turn more slowly than it did? There are, in fact, several minute causes at work to affect the length of the day; but they do not all operate in the same direction. The continual deposit of meteoric dust must lengthen it; so must the friction of the tides. Shrinking due to cooling will shorten it, as will the gradual wearing down of the land by the action of ice and rain. But on the other hand the uplifting of the whole mass of the Himalayas within times geologically recent must have lengthened it, and to a less degree the building of skyscrapers in New York is doing the same. On what side the balance of these opposing tendencies will lie cannot be predicted. But it can be discovered by researches such as those on the ancient eclipses, and as we have seen, there is evidence that the balance lies on the side of lengthening the day, by a very small but not altogether inconsiderable amount.

There are several other matters we must take into account, before we can say with certainty whether or no the moon and the planets move precisely according to the simple law of gravity. We must inquire, it is evident, whether any other forces than gravity can act upon those bodies in their orbits. What about friction? It is undoubtedly true that space is so very nearly empty as to offer scarcely any resistance at all to motion through it; but it is still uncertain whether there may not be some very slight effect of the kind. The instance which has been brought forward time and again is the retardation in the motion of the small comet known by the name of Encke—not because he discovered it, but because he investigated its motion, and found that its revolution round the sun was quickening—a paradoxical effect of resistance, one is tempted to say, but one which is easily demonstrated with a little mathematical reasoning, which we shall not, however, attempt here. Comets, whatever may be their nature, are such light and fragile things that they may be very easily disturbed by forces which the heavy planets would not feel ; yet it has always been felt that one would like to know why one comet only should show the effect. The long researches of Backlund have thrown much light upon the problem, while showing at the same time that it is much more complex than had been supposed. By immense calculations, extending over many revolutions of the comet, he has been able to show that the peculiar effect in the motion of this comet is not regular, but has been decreasing by fits and starts. For twenty years or so it will remain the same ; then suddenly it will lose a quarter of its power ; and again, after a lapse of years, another sudden change will take place. Most curious of all, these changes take place about the time when the sun is most affected with spots, and his general activities are most pronounced. It is early as yet to predict whither this remarkable result will lead us. But it is not the only indication we have obtained that the sun may exercise powers other than those belonging merely to his mass.

The question had often been raised, whether light falling upon a body exerted any pressure upon it. Within the last few years the question has been answered in the affirmative, both by calculation and by experiment. It has been shown mathematically that light does exercise pressure, but that it is effective only on very minute bodies comparable in size with the length of a light wave. Experimentally the effect has been shown in the way which has been described in our chapter upon comets. There seems to be no reason to suppose that the pressure of light can be called to account for irregularities in the motion of the moon or the planets.

To sum up a difficult but fascinating enquiry—one which has made great progress of late, yet is far from being worked out—we may say that the planets and the moon obey the strict laws of gravitation with remarkable, but not perhaps with absolute fidelity. Here and there one finds a small discordance which defies adjustment—a wandering of the moon from her predicted path, a trifling shift in the orbit of a planet, a sudden effect upon the motion of a comet, all of which drive us to the conclusion that when the effects of gravitation have been calculated to the most minute perfection, there will still be something left over for explanation.

It is always so, and herein lies the charm of our subject. It is never worked out.

CHAPTER VI

CELESTIAL MEASUREMENTS

A VERY important matter in any science is the numerical determination of the fundamental constants. A chemist is concerned for the accuracy of his atomic weights and specific heats, a physicist for his units of mass, of electricity, of light, and what not. In the same way the astronomer has to face continually the necessity for greater refinement in his determinations of the distance of the sun, of the masses of the planets and of the moon, of the aberration of light, the amounts of the various motions of the axis of the earth, and the velocity of the solar system in space.

As an example of such investigations we will take the determination of the distance of the sun from the earth, or its equivalent, the quantity known as the solar parallax; and we will select this for three reasons. It is the fundamental distance in terms of which all others may be expressed; its determination involves processes which are thoroughly characteristic of those operations of astronomy related to measurement; and thirdly, a personal reason, the author has himself spent ten pleasant years upon the problem, and feels for it a lively affection.

There are many ways of attack—some direct, others leading through a maze of by-paths in which one may always suspect ambush and surprise. We will deal first with the direct frontal attack, which is in principle simply a case of surveying upon a gigantic scale. The sun is an inaccessible point, but that is no fatal obstacle to finding its distance. The summit of Mount Everest is inaccessible, yet its distance from Darjeeling can be found with great accuracy, provided that there is at the summit some definite point on which to make the observations. Given that, the accuracy of the determination depends principally upon the length of the base which can be employed at Darjeeling; for the operation consists of course in observing from each end of a base, whose length is known, the angle between the distant point and the other end of the base.

Every surveyor knows that, if he wishes to get accurate results, his first care must be to lay out what are called "well conditioned" triangles, that is, triangles of which all the angles are moderate, neither too large nor too small; and of course the further away an object is, and the smaller by comparison is the available base, the more difficult and uncertain is the resulting measurement. Now as our earth is but 8,000 miles in diameter, we cannot possibly have our base, from which to measure the sun's distance, more than 8,000 miles long, and practical considerations limit the distance to about 7,000. But the sun is more than 13,000 times this distance from the earth; and even if we were able to select stations 7,000 miles apart, and observe simultaneously at each the angle between the sun and the other, our triangle would be hopelessly long and narrow, with the angle at the sun less than 15 seconds of arc. Moreover, the two ends of our base will be nearly on opposite sides of the earth, altogether invisible one from the other, and we must get over this difficulty by a roundabout process that greatly complicates the work, and makes it much less accurate. As a problem of pure surveying, the measurement of the sun's distance by direct settings upon the sun himself is quite hopeless, unless we are content with a probable error of two or three per cent. in our result.

The problem would be much simpler if we could see stars close to the sun. In that case it would not be necessary for us to be able to see from one end of the base to the other. Instead, we could make simultaneous observations of the position of the sun among the stars, which may be considered infinitely far away in comparison with the sun. When viewed from two different points, on nearly opposite sides of the earth, the sun would appear differently placed among the stars, by that small amount of fifteen seconds of arc, invisible to the unaided sight though measurable with telescopes. This involves the true principle of all astronomical determinations of distance, that a near body is displaced with reference to more distant bodies, when observed from different points of view. In astronomical language, the nearer body has a parallax (Greek for a shift) relative to the distant bodies, and all such determinations are called determinations of parallax.

Though we cannot see stars close to the sun, we can, on rare occasions, see the planet Venus pass across the face of the sun as a small dark body; on which occasions Venus, being only at about one-third the distance of the sun, has a parallax relative to him. The consequence of this parallax is then that the time at which Venus appears to enter upon and to leave the sun, and the duration of the transit, depend upon the place on the earth which is chosen as a station from which to view the event. According as we select our station we may find an acceleration or retardation of about sixteen minutes in the moment when we observe a particular phase of the transit.

Historically, the transits of Venus have supplied the most famous occasions for the solution of our problem. The necessity of travelling to remote places invests the undertaking with an air of importance and adventure. It must be something worth winning, to induce Governments to send large and well-found expeditions all over the earth, at a total cost of many hundreds of thousands of pounds. Thus the solar parallax has among astronomical problems a dignity all its own, associated in public repute with the famous voyage of Captain Cook to the South Seas, and with the tremendous efforts put forth within recent memory in 1874 and 1882. Unhappily, one must admit that the money spent in sending expeditions to Siberia, to the Pacific, to Kerguelen Island in the Antarctic

Ocean, was practically thrown away, through no fault of the astronomers, unless indeed they can be blamed for failure to foresee the difficulty which wrecked the whole enterprise. They had gone to determine the exact instant at which Venus appeared in contact with the edge of the sun's disc. They expected to have a perfectly clear cut view of this contact, and they did not at all anticipate what actually happened, that the atmosphere round the planet, filled with sunlight, blurred the sharpness of the phenomenon and left it uncertain within a large number of seconds at what instant the critical phase took place.

In transits of Venus we observe, not the parallax of the sun directly, but the difference of parallax between Venus and the sun; so that to complete our problem it is necessary to know what proportion the distance of Venus bears to the distance of the sun. But this involves no special difficulty From Kepler's Third Law—the squares of the periodic times are proportioned to the cubes of the major axis of the planetary orbits—we know the relative sizes of all the orbits with great accuracy. Find one, or the difference between two, in miles, and the rest is simple.

This principle gives us the key to all modern work on the solar parallax. Choose an exterior planet as the stepping-stone, and you have at once a great number of advantages. The planet is nearest and its parallax greatest, when it is in opposition to the sun, visible all night, and observable consequently after sunset and before sunrise. The planet can be compared with the stars about it, so that the whole parallax, and not the difference between two, can be measured. And greatest advantage of all, the observations can be continued over weeks or months; failure at one moment can be repaired by success at others, and the astronomer is relieved of the disquieting anxiety lest a temporary break-down in instrument or in weather should ruin everything at a critical moment.

The first modern application of this method was made by Sir David Gill, who had observed at Mauritius the Transit of Venus in 1874, and had made up his mind very definitely that little good would come of the transit of 1882. So in 1877, at a very favourable opposition and close approach of the planet Mars, he borrowed Lord Lindsay's heliometer and established himself on the island of Ascension, near the equator. Every night the observing station in Mars Bay was carried some seven thousand miles by the rotation of the earth, and the planet thereby displaced among the stars by about forty seconds of arc. Several months of successful work on the island, and a couple of years of severe calculation afterwards, produced the result of 93,080,000 miles for the mean distance of the sun. Then, as so often happens in scientific enterprises, when it was hoped that all was complete, some one suggested a possible flaw—the red colour of the planet might have produced a systematic error in the observations.

The next step was to try again, this time with three minor planets so small that they were indistinguishable in appearance from stars, and presumably free from the possibly damaging effect of that difference of colour which rendered the observation of Mars a little uncertain. The same courageous observer, by that time Her Majesty's Astronomer at the Cape, organized a co-operation between his observatory in the south and four in the northern hemisphere, to observe the small planets Victoria, Iris, and Sappho at their oppositions in 1888 and 1889. The labour was immense, the observations proving so accurate that they demanded the use in a great part of the work of eight-figure logarithms;

but when the results were eventually published in two enormous red volumes of Annals of the Cape Observatory, it might well have seemed that observation had now done its uttermost to solve the problem. Nevertheless, within a few years the whole question was revived on a more extensive scale than ever.

To understand how this came about, and why it was necessary, we must look at some of the indirect ways of solving our problem. It will be worth our while to do so, even at the expense of a little difficulty, because they illustrate in a peculiarly interesting way how widely different branches of our science have delicate relationships with one another. There is an irregularity in the motion of the moon which depends upon the distance of the sun, though in a complicated way which we shall not attempt to follow. If this can by observation and calculation be disentangled from the many other irregularities which affect the motion of our satellite, then its value will serve to determine the solar parallax. Or again, the mass of the earth affects by its attraction the motion of the other planets, and they all affect one another by amounts depending upon the distances of the planets apart. If the action of the earth can be disentangled from the others, the mass of the earth can be found, and thence, by a process much too difficult to describe here, we have another value for the distance of the Sun. Or again, the motion of the earth about the sun, combined with the velocity of light, produces the well known effect of the Aberration of Light, which causes all the stars to move apparently in little ellipses each year. The size of the effect can be measured; the velocity of light can be measured independently in the laboratory; and from the two results we may deduce the velocity of the earth in its orbit. Then, knowing the length of the year, it becomes a simple matter to find the size of the orbit, and the distance of the sun.

These three are typical, and the most important, of the indirect methods of finding the quantity of which we are in search. Opinions will always be divided upon the question, how far any of the indirect results should be preferred to the direct results of measurement; and it is probable that those who have made the direct measurements will be the least willing to attach an exaggerated value to the indirect. But it cannot be denied that when the results fail to agree it produces an awkward feeling that all is not well, which is intensified when all the indirect methods appear to conspire against the result given by the direct. Such a crisis occurred about the year 1898. The indirect results combined at that time to make the distance of the sun several hundred thousand miles greater than direct measurement would allow, from which arose a renewal of interest and a general anxiety about the great problem.

At this moment a happy accident occurred. In Berlin there is a society which maintains a small observatory where non-professional astronomers may find an equipment beyond their private means. With the photographic telescope of this observatory Dr. Gustav Witt was engaged in a search for certain minor planets which had been neglected and had become lost. With the good luck which often rewards good work, he found a prize far exceeding in value that at which he aimed—a minor planet indeed, but moving in a path remarkably situated, very eccentric, and bringing the planet on rare occasions within fifteen million miles of the earth. So soon as a first rough approximation to the path of the new planet had been calculated, the great value of the discovery was apparent:

it provided an opportunity of determining the distance of the sun with an accuracy much greater than had been possible before. The opportunity, it is true, would occur but rarely, and the most favourable possible occasion had just been missed, having occurred four years before the discovery. But in the autumn and winter of 1900-01 a quite satisfactory opposition of the planet was to occur, and it became immediately a pressing matter to ensure the necessary co-operation between astronomers. Here again fortune was kind. A meeting of the large permanent international committee which controls the photographic chart of the heavens had been called for July, 1900, in Paris, and the accompanying congress brought together a very large and representative body of astronomers. It was thus easy to arrange a joint programme of work for the ensuing winter, to ensure that every possible advantage should be taken of the new opportunity of making a fundamental improvement in celestial measures.

Throughout Europe the skies of that winter were very unfavourable. At Cambridge the author had the pleasure of being on duty with the photographic telescope from dusk till dawn for nearly three months, during which time only five or six nights were more or less clear throughout. Had visual observations alone been possible, one could scarcely, in those brief and broken intervals of clear sky, have obtained a sufficiency of measures. But the photographic plate, in work of this kind, is so much more expeditious that it was possible to obtain at Cambridge alone about five hundred photographs, on which the position of the planet among the stars could be measured with great precision. Many other observatories obtained as much material; a few, happily situated in fine climates, obtained much more; while the visual observers had been equally active and had accumulated an enormous mass of measures, referring the planet to great numbers of faint stars whose positions at the time were entirely unknown.

Here, then, was an overwhelming richness of raw material for the solution of the problem. How to deal with it economically and effectually, and how to promote the necessary and laborious supplementary investigations, the observation of seven or eight hundred stars with meridian circles, or the calculation of the path of Eros with the most refined care —became a question of the highest interest.

In this work the Paris Observatory took a leading part, under the direction of the late Monsieur Loewy. His aim was to bring together the material produced at each co-operating observatory into a systematic form. It would then be readily available to the hand of whoever might feel drawn to undertake the not inconsiderable labour of extracting the ultimate four or five figures from the existing millions. By a preliminary treatment of a small part of the whole the author had been able to discover the appropriate methods of dealing with the problem, and to assure himself that the enterprise was going to be a success. He will always be grateful for the happy combination of circumstances which resulted in the whole final discussion being placed in his hands, with the means necessary to secure help in the calculations.

In an enterprise of this kind, which must necessarily take some years to complete, the unkindest thing that can happen is for all to go smoothly; for calculation is apt to be tedious, and requires enlivenment. The fascination of the work lies in the solution of the puzzles which hardly ever fail to present themselves. On the occasion of which we speak there was no lack of this sporting element. It was the first time there had been the opportunity on a grand scale of comparing minutely the results turned out by different photographic telescopes, and by different ways of measuring the plates and reducing the measures to a common form. Naturally enough, when one searched deep one found discordances and disagreements of small amount, but of amount which could not for a moment be neglected. One had to try all kinds of experiments to discover what was wrong with photographic object glasses; to find out why one calculated prediction of the planet's path did not accord with another, though both were built upon the same foundations. One had to maintain correspondence with observatories all over the world, leading to the formation of many valued friendships; and one had continually tc steer in a narrow channel between dangers on either hand, each offering the chance of shipwreck. On the one hand there was the danger of neglecting some minute precaution against error which might affect the whole result. On the other hand there was the danger that pursuit of the tempting enquiries presenting themselves at every turn might so prolong the work that it would never get finished.

Some one has laid down the principle that it is a mistake to plan work which will take longer than ten years to complete. Judged by their standard the solar parallax determination from the planet Eros just escapes censure. It was begun in the Paris Congress of 1900, and it was practically finished when the final results were communicated in brief to the Académie des Sciences in 1910. Whatever judgment may be passed in the end upon the results of that enterprise, they present at the moment two features which are eminently satisfactory; the photographic and the visual observations give the same result; and that result is the same as the previous result made by direct measurement.

Moreover, within the last two years a beautiful piece of work has come from the Cape Observatory—a determination of the distance of the sun from spectroscopic observations, long anticipated, but only to be achieved after great efforts at conquering instrumental difficulties. We have seen how the spectroscope is able to measure the velocity in the line of sight between the earth and a star. This velocity necessarily varies with the season of the year, and the variable part due to the motion of the earth round the sun can be disentangled from the constant part due to the combined velocities of our sun and of the star in space. Hence one finds the ratio between the velocity of the earth and the velocity of light—that is to say one finds the constant of aberration by a new and direct method; and the well known values for the velocity of light and the length of the year give us eventually the distance of the sun, which by this new and quite independent method comes out in almost precise agreement with the old.

The result which emerges from a very big enterprise such as the Eros campaign may be expressed in four figures, which are usually written thus:—

Solar Parallax from Eros = 8″.806.

For the moment one is left wondering how this can be called a determination of the sun's distance. Even if the sign for seconds of arc is misread as inches, which is only too common, the result has the appearance of being absurd. The fact is that the astronomer does not want the distance of the sun expressed in miles; for his purposes the numbers are inconveniently big and the whole thing in the wrong shape. What he really cares about is the ratio between the equatorial radius of the earth and the mean distance of the earth from the sun. This small quantity is the sine of a certain angle; the angle is what we ordinarily call the Solar Parallax, and it is independent of any reference to miles or kilometres. These units, more or less arbitrarily defined by standard bars of platinum, are indeed purely mundane, and have no real place in celestial measurements. One may very well question how far it is desirable to use them for celestial distances, though there is no difficulty in doing so if it is wished. Geodesy has measured the radius of the earth's equator, and makes it about 6,378 kilometres. Using this value we find at once from the above value for the solar parallax that the mean distance of the sun from the earth is 149,400,000 kilometres, or 92,830,000 miles.

An interesting by-product of an investigation such as we have described, is a value for the mass of the moon in terms of that of the earth. This quantity becomes involved in our problem in the following way, which illustrates very clearly the complexity of measurements made from a moving earth. We have been speaking of observing the planet Eros night and morning at a single observatory, or more or less simultaneously at two observatories far apart upon the earth's surface—Greenwich and the Lick Observatory for example, and for simplicity we have spoken of the displacement of the planet among the stars, as thus viewed from different points, as though it were the only displacement that the planet suffered. But, in fact, the planet is moving all the while in its own path round the sun, and so is the earth; while the motion of the earth is complicated by the fact that the earth has a partner, the moon. They turn about one another in their progress much as a heavy man waltzing with a little girl progresses round a ballroom, the light partner turning in a larger circle, the heavy in a smaller. In the case of the earth and the moon the one is so much heavier than the other that the common centre of gravity about which they turn is within the surface of the earth, though far from the centre; but the effect of this monthly oscillation of the earth is to produce a monthly irregularity in the apparent motion of any planet viewed from its surface. Disentangle this monthly oscillation in the place of the planet, determine its amount, and one can deduce the size of the oscillation in the earth's motion which produces it. Thence one derives the proportion between this displacement and the distance of the moon, and from that proportion the mass of the moon in terms of the earth. The observations of Eros gave for this proportion 1 : 81.5.

We have dealt at length with a single problem of celestial movement in order to get an idea of the manner in which such measures are made, and of the way in which the results are interwoven one with another; of the continual endeavour to refine and refine again the important determinations of fundamental quantities like the distance of the sun, and of the fascinating enquiries which are opened up when the results of different methods disagree. With a foundation thus made secure, we may proceed with the more confidence to build.

CHAPTER VII

THE STARS

IN modern astronomy the stars have individualities: we know them as near or distant; having large or small velocities among their neighbours; surpassingly brilliant of surface or relatively dull; single, or double to the telescopic sight, or double only to the searching scrutiny of the spectroscope; steadfast in light, or variable by eclipse, by outburst of flame, by causes unknown. With individuality they lose their name of fixed stars. No longer can they be regarded as the unchanging background before which the sun and the moon and the planets move: as useful points like the beacons of a survey, pegs upon which to hang the delineation of the country, uninteresting in themselves and likely to disappear from the finished work when they have served their purpose. We are not, and never shall be, in a position to neglect this aspect of the stars, expressed in the word "fundamental," but we shall find that in the light of modern methods and modern results the fundamental star catalogue no longer appeals to us as the last word in stellar astronomy. The distinction is much the same as the difference between the study of the planets and the planetary theory. In the latter the planets are treated as mere geometrical points in which is concentrated a certain mass; the planetary theory has no concern with the physical state of the planets, their structure, or their fitness for habitation. So with the stars: we may think of them as mere points in space, and be interested in their positions, their distances, and their proper motions—or we may think of them as suns, having different compositions, different masses, and all kinds of different physical properties. The first is astronomy in its restricted sense; the second is the modern subject of astrophysics; and though the two have no sharp division, and are of course continually indebted one to another, we shall find it natural to consider them in some measure apart.

To begin then with astronomy proper—the science of star places and apparent motion, often called astronomy of position. Its foundations were laid, it is believed, by Hipparchus more than a century before Christ. Hipparchus was probably the first who constructed a catalogue of star places. The catalogue has not come down to us under his name, but there are good reasons for believing that Ptolemy's famous catalogue in the Almagest is founded upon an earlier catalogue of Hipparchus. Right well, in that case, does Hipparchus deserve the name of founder of practical astronomy, for no sensible improvement on that catalogue was made until the days of Ulugh Beigh, who reigned at Samarcand, founded a great college and observatory, and made a revision of Ptolemy's catalogue which is dated 1437. Tycho Brahe's star catalogue, made with the greatly improved instruments of his observatory at Uraniborg, was published in 1602, the first of many famous catalogues that have come from round about the Baltic. Then we come to the invention of the telescope, and its application by Halley to measurement as well as to description; to the names of Flamsteed and Bradley, Lacaille and Lalande, Mayer, Piazzi, and Bessel. By their labours was reared the substructure, on which in a sense the whole modern astronomy of position is carried.

Without attempting to follow in detail the development of star cataloguing, we may consider the main purposes to which it is directed. Firstly, accurate places of the stars form an essential basis of the useful applications of astronomy—the determination of time, navigation, surveying. Secondly, accurate places of the stars are required as reference points, to which the sun and moon, planets and comets, are all referred. Without good star places we can have no theory of the motions in the solar system, no orbits of planets or of comets. And thirdly, most important for our present purpose, without accurate catalogues of the stars we can know nothing of the grander problems of the universe, the motion of our sun among the stars, or of the stars among themselves

When one compares a catalogue of to-day with a catalogue made a hundred years ago, one finds discordances between the places of certain stars now and then. Some of these are due to nothing but errors of observation in one catalogue or in both. We have means of estimating how large these errors are likely to be, and can say approximately what the average accidental discordance should amount to. When we find a great number of observed differences much greater than these, we must recognize that the stars are not fixed; that some, on the contrary, have motions proper to themselves which become quite evident in the course of fifty or a hundred years. A most important purpose of star cataloguing is to determine the magnitude and direction on the celestial sphere of these proper motions of the stars.

Having determined a great number of proper motions we have next to enquire whether they show any significant arrangement. Suppose for the time being that the real motions of the stars are haphazard, distributed according to the laws of chance with which mathematics can deal. Then, if the sun is at rest, the proper motions of the stars, as we see them, will be haphazard also, and there are definite tests which can be applied to see how nearly haphazard the motions are. But if the sun is not at rest, his movement will introduce at once a certain degree of order into the otherwise random proper motions. The nearer stars on either hand will have a general tendency to move backwards, that is to say, to appear to do so; they will, in fact, on the average, be left behind, while the stars in the part of the sky to which the sun's motion is directed will be very little or not at all affected; and so will the stars in the opposite quarter of the sky. Working upon this principle, many astronomers have made discussions of observed proper motions, with a view to finding the point towards which the sun is moving; or in technical language, to finding the "apex of the sun's way."

As might be expected, the result depends to some extent upon the selection of the stars to be discussed. Different lists of stars give somewhat different values for the position of the apex, which means of course that one has to use a great number of stars before their individual peculiarities are averaged out, and their motion in the mean is zero. There is, however, a general consensus of result that the motion of the sun is directed towards the constellation Lyra, or at any rate towards that region of the sky.

Ten years ago there was no suspicion that the basis of this investigation might be unsound, because of the fundamental assumption that the real motions of the stars are random. Seven years ago, at the Scientific Congress held in connection with the St. Louis Exhibition, Kapteyn, of Groningen in

Holland, introduced to us the new idea of "star-streaming"—an idea which has upset many of our previous notions. Kapteyn had been engaged for many years on a series of intricate researches into the arrangement and the motions of the stars in the universe. He had prepared an elaborate machinery of theory and calculation which would work if the stars were moving at random, and would not work if they were not. On arriving at the crucial point, where theory was confronted with observation, the machine would not work, and its manner of breakdown showed at once what was wrong. The stars under discussion were found to be moving in two streams, crossing through one another and pretty well mixed, but with an average velocity so considerable that it destroyed any possibility of treating the motions of the stars as random.

Since the announcement of this revolutionary discovery much work has been done upon the subject, particularly by British astronomers. Eddington, Dyson, Hough and Halm have examined the question from many different points of view, and their results leave no doubt that star-streaming is a real and embarrassing fact. It can be represented in several different ways, none of which is very easy to understand. The broad result is clear, however. We had been in error when we assumed that the motions of the stars are haphazard, with no general system superimposed upon their infinite variety. On the contrary, we are now compelled to recognize that considerable numbers of them are drifting together, and that these drifts must have an important influence upon all such discussions as that of the movement of the sun in space. Epoch-making as are these results, it appears to the author that in one respect they have been overrated. We begin to hear talk of the stars being divided into two universes, as if the stars which can be assigned to one drift or another were representative samples of the whole contents of the sky. It is far more probable—indeed we may say it is practically certain—that all the stars with a sensible proper motion, all the stars, that is to say, which belong to these drifts, are relatively close neighbours of the sun. The bulk of the stars in the sky, and especially the stars of the Milky Way, are untouched by these discussions, because they have no determined proper motions to be discussed. In the course of centuries we may discover that there are a thousand drifts—a thousand universes, if one will. Meanwhile we shall do well to avoid the false suggestion of completeness implied by the statement that there are two.

The greatest impediment to the study of star distribution is the extreme difficulty of determining a star's distance from the sun. The principle is easy: the nearer stars should appear to oscillate in a yearly period, as the earth moves round the sun; the range of the oscillation should give the distance of the star. The practical difficulties of turning this simple principle to account are due to the immense distances of even the nearer stars, which make the oscillations infinitesimal. Measurements for the determination of a star's distance are generally made nowadays upon a photographic plate, taken with a telescope of considerable focal length, so that the plates are on a large scale. The Cambridge Observatory photographic telescope, for example, is nearly twenty feet long; a degree on its plates is about four inches. Now if that telescope were photographing the star which is nearest the sun, its image would in the course of the year oscillate a thousandth of an inch on each side of its mean place; this is all the effect produced by the displacement of the earth across the 186 million miles which is the diameter of the earth's orbit. And this is for the very nearest star. Very few stars—perhaps two or three hundred in the whole sky—are within ten times its distance; very few stars, then, will be displaced a ten-thousandth of an inch either way by the earth's revolution in a year. In fact their distances are so great that the nearer stars are on the borders of possibility, while the great bulk of the stars in the sky are utterly beyond the reach of this method of measurement. And since at present for the generality of stars there is no other way available, it seems that we are debarred from extending our knowledge in this most desirable of directions.

It was thought at one time that a rough estimate of a star's distance might be deduced from its brightness; but the relatively few measurements we have show that this idea is illusory. Most of the bright stars are quite out of reach; some few of the faint ones are among the sun's nearer neighbours. Hence, though no doubt it is true that on the whole the fainter stars are the more distant, for any particular star or star group its brightness tells us nothing about its distance. The only criterion of nearness is proper motion. If a star has a large proper motion there is some chance that it is within measurable distance; so that stars with large proper motions get selected for measurement. But it is evident that another star without proper motion may be just as near, and will escape observation because there is no reason to select it as promising some return for a great deal of work. This is doubly unfortunate, because it gives to all the statistics of stellar distances a bias which can scarcely be avoided.

Many have tried to find some way of picturing the distances which separate star from star in the sun's neighbourhood. Perhaps there is no way better than to imagine a model in which the sun is represented by a grain of sand one-hundredth of an inch in diameter, and the earth by a quite invisible speck one inch away. Upon this scale the nearest star will be another grain of sand some four miles away, and the other stars will be scattered at somewhat greater distances apart. To this incredible sparseness are the stars reduced when we try to look at them in three dimensions.

While the model is in mind, let us anticipate a result of spectroscopic work which gives us the velocity of the sun in space—four radii of the earth's orbit per year, or four inches on our model. At this rate the sun would take some seventy thousand years to reach his nearest neighbour. Now so far as is known we have no real reason for assuming that the stars are gathered anywhere much more closely than this, except of course in the star clusters. We shall do well, therefore, to view with scepticism any theories which require crowding or collision among the stars. Despite the richness of the sky, the emptiness of space is its most striking character.

From time to time a new star will suddenly blaze up, telling of some sort of catastrophe in the heavens, and inspiring the sensational press to enquire whether such a conflagration might not overwhelm us any night. Having no taste for the lurid, we will dismiss this last idea without anxiety; but the general question of what happens when a new star appears is well worth discussion, in a more cautious and sober spirit than that displayed by the collision-mongers.

The appearance of a new star is not an exceedingly rare event. Every year or two they find, on the stores of photographs which are accumulated at the Harvard College

Observatory, evidence that a star has appeared suddenly, declined gradually, and generally disappeared again before it had been recognized. Occasionally, when the star is bright, it is caught before its decline has proceeded very far, and once, by good fortune, the most splendid new star of modern times was detected when it was still upon the upgrade. The new star in Perseus—Nova Persei—was found in February of the year 1901 by Dr. Anderson, of Edinburgh. This accomplished observer—not an astronomer but an "astrophile" he calls himself—has a habit all too rare among astronomers proper, of getting out of bed every hour or two of a cloudy night, to see if the sky has cleared. On February 21, 1901, the sky cleared after midnight, and Dr. Anderson began his regular work of comparing the sky with star charts, field by field, in the hope of discovering new variables. He once described to the author how, becoming tired at the telescope, at twenty minutes to three in the morning he leaned back to rest a moment, and saw in the constellation Perseus the bright new star. Unable to believe that so bright an object could have escaped previous notice, he made diplomatic enquiries early next morning at the Royal Observatory

Edinburgh, and learned that they had no news of any special interest. Then, said he, he knew that he was all right.

At the time of discovery the star was of magnitude 2.7; that is to say, about half as bright as the Pole Star. A photograph taken only twenty-eight hours before, by Stanley Williams at Brighton, showed stars nearly to the twelfth magnitude, but no trace of the Nova, whose light must have increased, therefore, at least four thousand times during that interval. In another day and a half it had become the brightest star in the northern sky; then it began to diminish, at first rapidly, afterwards more slowly, with large fluctuations which embarrassed alternately those who sought to show the new star, and those who wished to point out that it had disappeared. By June it had become invisible to the naked eye, and thenceforward it faded slowly in the way characteristic of new stars.

Until the invention of the spectroscope very little could be made of these phenomena; to observe the amount of light, and its colour, did not carry the observer very far towards explaining what had caused the sudden outburst. The new possibilities of light analysis have shown that the sequence of events in the life of a new star is generally as follows: When it is bright, a great part of its light comes from outbursts of glowing gas; as it fades, it becomes a gaseous nebula; in its final stage it is a very faint star of no special characteristics. The very prompt discovery of Nova Persei enabled the astronomers of Harvard to photograph it while it was still increasing in brightness—to the general surprise they found a spectrum like that of the stars in Orion, with no bright lines. This first chapter is unique, and it adds very much to the difficulty of explaining a new star, a difficulty already grave enough. We must, in fact, admit that the evidence has not as yet received any satisfactory interpretation. Actual collision between two stars is always possible, though extremely unlikely. Did it occur, there would be inevitably a terrific production of intensely hot vapour as the energy of the collision was changed into heat. We should expect that analysis of its light would show bright lines from the first. As a fact, they did not appear for several days, and seem to have been the result and not the cause of the brilliance.

All kinds of theories have been put forward to account for the sudden appearance of these new stars— most of them of the catastrophe or collision class. Perhaps the most plausible is the suggestion that a new star is due to the passage of a faint, cool, and almost dark star through a nebula, in which case we must suppose that the surface of the star becomes suddenly heated by friction, and thus the outburst of light is produced. On this theory a new star would be a kind of celestial meteor. Without going into the very complex facts revealed by the spectroscope, we may say that no satisfactory explanation of those facts has been put forward; the light analysis defies interpretation.

But it is by no means improbable that the eventual solution will be found in regarding new stars simply as exaggerated cases of variable stars. We have in such a star as Mira Ceti a case quite as difficult to explain, which attracts rather less attention only because the outburst happens every 330 days, more or less. The brightness of this star is continually varying, and at maximum it is anything from a hundred to a thousand times as bright as at minimum. Another variable star of long period, χ Cygni, has a total range of nearly ten magnitudes, or shines 10,000 times as bright on some days as on others.

We can hardly believe that a catastrophe or collision affects such stars every three or four hundred days; there must be some more normal explanation of the light-outbursts, though none as yet has been established.

It cannot be said, therefore, that the outbursts of light in a new star are so immensely greater than those in some of the variables, that they must without doubt be ascribed to different causes. Yet it is not easy to assign them to the same cause, because the spectra of new stars are very different from the spectra of long period variables. The latter, when they are bright, show exceedingly brilliant lines of hydrogen in their spectra. The former show the hydrogen lines, but they show also other lines, quite as bright, whose origin is unknown. And most curious and inexplicable, each principal bright line is accompanied by a dark absorption line alongside it, which is the characteristic spectrum of all new stars. It is not, however, quite unique. There is one star in the constellation Cygnus which has it permanently, and this one star is a great asset, because it suggests, nay compels us to believe, that the new star spectrum is not necessarily associated with a catastrophe. Some day, perhaps, we shall see a way of combining these facts into a single story, and of finding the new stars a proper place at the extremity of our classification of variables.

What would make this the more natural is the fact, well established, that the longer the period of a variable star, the less is it disposed to follow regular laws; the shorter the period, the more precisely does the star repeat, time after time, its fluctuations in light. So great has been the amount of information collected within the last few years, so complicated are the classifications proposed to fit the observed facts, that within the limits of a book like this we can do no more than indicate how wide is their variety. One has classes of stars which increase quickly and diminish slowly; others, less numerous, which do the reverse. Some have irregularities during the ascent, some during the descent Some have double, unequal maxima, separated by equal minima. Some have prolonged maxima, and minima quickly passed; in others the minimum is prolonged, and the maximum no sooner reached than the light begins again to decline. Of all these types there is only one that is certainly and completely explained—the Algol-type of eclipsing variable, named from the bright star Algol in

Perseus which was first remarked and has been most completely studied. In this type the star is normally at full brightness—at regular intervals its brightness fails and quickly recovers. It is now fully established that this is due to an eclipse. The star has a dark companion revolving round it, like a gigantic planet, very close. The plane of the companion's orbit is so placed that our sun lies nearly upon it, from which the consequence follows that at each revolution the star is partially eclipsed.

A particular interest attaches to these Algol stars, because Algol was the first member of the class which has now become enormously important under the name Spectroscopic Binaries. A binary star is a double star in which the components are observed to revolve about their common centre of gravity. We shall deal presently with the cases in which both stars can be seen in the telescope. A spectroscopic binary is a star which under the highest telescopic power remains single, but which with the spectroscope is proved to be a pair of stars in revolution. To this, the most remarkable of all the doings of the spectroscope, we must devote a brief explanation.

The position in the spectrum of any line, bright or dark, depends upon the number per second of the corresponding waves of light which enter the spectroscope. If the star and the spectroscope are approaching one another, the number of the waves of light from the star which the spectroscope meets is naturally increased; if the star is receding, the number is diminished. It follows from this that if a star is alternately advancing and receding, the lines in its spectrum are shifted alternately towards the violet and the red ends of the spectrum. The shift can be measured and the measurement can be translated into the statement that the star's velocity is variable by so many miles per second. It was in this way that Vogel showed that Algol receded and advanced exactly in the way that it should do if it were revolving about the common centre of itself and a companion.

In the Algol pair the companion is dark, or at least gives very little light compared with Algol itself. But this is not always so. Somet mes both stars of a spectroscope binary are bright. Then the spectrum belonging to the one is shifted towards the violet while the spectrum of the other is shifted towards the red; in this case all the lines in the compound spectrum belonging to both components are alternately double and single. Such a doubling was recognized by Pickering in two stars in the year 1889, about the time that Vogel published his observations of Algol.

Since then an immense piece of work has been done in some of the principal observatories of the world, in examining all the brighter stars one by one, to search for variable velocities; and a surprising degree of success has rewarded this search. One star out of every four or five examined proves to be a spectroscopic binary, and the proportion seems to grow steadily larger—which after all is to be expected when one remembers that the appliances for detecting variable velocity grow year by year more powerful and more delicate. Remarkable instrumental refinements are demanded in this work. A great step forward was made by Campbell at the Lick Observatory, when he devised means for keeping his spectroscope at a constant temperature during the hours required for photographing a spectrum. The most perfect apparatus at work is probably that attached to the Victoria telescope of the Royal Observatory at the Cape, where by a system of electrical control the prism box can be maintained at a constant temperature, automatically, for as many days or weeks as may be desired.

The time is not yet ripe for the formation of an opinion as to the final significance of this remarkable fact, the large proportion of spectroscopic binaries among the stars. From one point of view it is a triumphant vindication of the mathematicians, who have shown how easily an elastic globe of gas, like a star, may become unstable, and after passing through a sequence of changes become of a pear-shaped figure which is preliminary to separation into two parts. From the mathematical analysis of the problem this ought to happen sometimes; observation has now shown that it happens very frequently—once out of three or four times, perhaps. From another point of view the discovery of so many spectroscopic binaries disturbs the idea, drawn from our own solar system, that the function of a star is to nourish with heat and light a family of planets. The terrible problem of the motion of a planet round a pair of suns has not yet been solved, but it seems quite unlikely that such a planet could pursue an equable way conducive to the development of life upon its surface.

The same method which allows us to detect variability in the motion of a star, allows us to measure the velocity, whether variable or not, with which that star is approaching or receding from the sun; while, when enough of these velocities have been determined, we may derive at once the direction and the velocity of the sun's motion in space. It will be remembered that other methods gave us a determination of the direction, but were not powerful to show the velocity. The spectroscope possesses what the other methods lacked, and makes it evident that the sun, with all the planets, is moving with a speed of some twelve miles a second towards the neighbourhood of the constellation Lyra. By a very recent extension of this work Hough and Halm at the Cape have been able to separate the spectroscopic, or radial, velocities of the stars into several drifts, partly confirming and partly denying the evidence obtained from proper motions, to which we have already referred. The subject is too new and too complex for discussion here, where one may not talk the simple mathematical language in which alone these things become intelligible.

Now to return to those binary stars which are visibly double, and which can be measured and studied by the telescope without the aid of the spectroscope. Sir William Herschel began to measure such pairs in the hope that he might detect a yearly oscillation of one star with respect to the other, from which, in the way we have already discussed, the distance of the brighter might be determined. It did not occur to him that the two stars of a pair, though of very different magnitudes, might be at the same distance, and in fact a pair of suns. His measures served, however, as the foundation of what has become a great branch of stellar astronomy, the measurement of double stars and the calculation of the orbits of all those pairs which prove to be in revolution. When it was first shown that such pairs obeyed the laws of gravitation in their motions, it seemed a great thing to have proved that those laws were no local regulations of the solar system, but had universal force. Now that the spectroscope has shown that the most distant stars are formed of very much the same materials as the sun and the earth, we should find it strange if they were not subject to the same laws of gravitation. It would, in fact, be more interesting if one could find a pair of stars whose motions were demonstrably not in accordance with those laws; but this has never been done. When irregularities of motion exist, they can be explained very plausibly as due to the disturbing action of some third, invisible body.

The measurement and calculation of binary stars is a relatively simple matter, requiring a good telescope without over elaborate accessories, but requiring above all a perfect eye and delicate hand for its successful prosecution. The days are not long past when a man with a genius for double star work could pick up new discoveries with a comparatively small instrument. Within recent years the northern hemisphere of the sky has been so thoroughly searched by the great telescope on Mount Hamilton, that it has become hard to find anything which that telescope has overlooked. The southern hemisphere is not nearly so thoroughly explored, and very much remains to be done there in double stars, as in many other branches of astronomy.

Yet it must not be imagined that there is not plenty of work left for telescopes in the north. The very close pairs, which demand large telescopes for their observation, demand also very frequent observation, for many of them are in rapid motion, completing their revolutions in relatively few years. The wider pairs, which are easier to measure, are less likely to be in rapid motion, but must not on that account be neglected. Still wider pairs may not be in orbital motion at all, but merely two stars, very far apart, which chance to be in nearly the same line of sight from the earth. None the less, they are worth measuring from time to time, for these measures serve to detect straightforward proper motion in the nearer or faster star.

One might reasonably enquire how such measures could be valuable, since only the difference between the motions of the two stars could be observed. The answer depends upon the view which one may take as to the "background" of stars, as we have come to call it in recent years; and different astronomers view the background in different lights. Some there are who hold the view that none of the stars may be called fixed: that all are in relative motion, which no one can deny, and more, that the motions of all are probably large enough to be distinguishable after a few centuries of observation. Others there are who see things in a different perspective: they feel that the stars which show any measurable motion are relatively few, and comparatively near the sun, while the great majority of the fainter stars lie at distances so vast that for a few centuries they may be considered fixed. The time has not come to decide between these two opinions, which affect, nevertheless, for better or for worse, a great many of the programmes of work laid out for the next twenty years. In particular they affect any estimates which may be formed of the ultimate value of that magnificent scheme, the Photographic Chart and Catalogue of the sky, planned in 1887 at Paris, and pursued since then at eighteen observatories. To appreciate the bearings of some of the questions involved we must go back to star cataloguing, forget the stars' physical individualities, and regard them once more as mere points upon the celestial sphere.

Since the first star catalogues were made before the invention of the telescope, they were necessarily limited to the stars visible to the naked eye; and since it was desired to observe stars in twilight, to obtain the place of the sun, the brightest stars were naturally selected for especial care. The possibility of observing bright stars with a telescope in full daylight, and the necessity of so doing, have maintained them in the select list of fundamental stars, in spite of the fact that a very bright star is much harder to observe than a moderately faint one, and the observation affected with particular errors. It thus happens that all successive catalogues of precision, as they are termed, start with the brightest stars as fundamenta, and refer the fainter stars to them. And further, it follows that the most important quantities in astronomy, such as the constant of precession, or the obliquity of the ecliptic, rest at the bottom upon the places of a small list of bright stars, nearer to us, no doubt, than the average fainter star, and affected by larger proper motions.

Now it is practically impossible to observe with the meridian circle—the instrument used for star cataloguing—stars fainter than magnitude nine and a half; while the number of stars down to that limit is already so large that it has been found impossible to deal with more than a selection of them. For the places of stars fainter than this, we must rely on measurements made on photographs. But a photograph will not determine the absolute places of stars, as the meridian circle can. It will do no more than refer the faint stars to such brighter stars already catalogued as may happen to be upon the plate. Thus the faint stars are referred to others of intermediate brightness, and those again to the bright fundamental stars. There are several steps, at each of which it is easy to accumulate error—indeed very hard, if not impossible, to avoid doing so.

One cannot help feeling that this process of working down from the bright to the faint, from the foreground to the background, though it was doubtless an inevitable process, is nevertheless far from being ideal. It would have been far more satisfactory to begin at the background, nearly if not quite immovable, and to work up to the shifting foreground; the precise gain depends of course in great measure on the degree of assurance that there really is a background practically fixed. Such advantage we may gain by using photography. Consider two plates taken fifty years apart, showing a few stars of intermediate brightness and a great number of faint ones. We wish to find what movements have taken place among those stars in the course of the fifty years. Two courses are open. We can fit each plate on to the celestial sphere by means of the few intermediate stars, whose relations to the fundamental stars are more or less imperfectly known; or on the other hand we may fit the two plates together directly by the large number of faint stars, so as to discover the degree of misfit of the brighter. Without entering further into details, it may be clear that in the first instance we are working down from bright to faint; in the second we are working up from faint to bright.

For some years it has been the belief of the author that we have in the second a method of great power and promise, capable of avoiding most of the snares and pitfalls which beset the first course; and certain to give us independent information of the most valuable kind, so soon as a sufficient time shall have passed since photography was applied to recording the places of the stars. In the great Astrographic Chart and Catalogue we have the material for putting this method to the test.

The original idea was to make a photographic map of the sky. The brothers Henry, who combined in a singular way the dual roles of instrument makers and astronomers, had constructed about the year 1884, and had used with great success, a photographic telescope at the Paris Observatory. Filled with enthusiasm at the results obtained, the astronomers of France called a congress at Paris in 1887 to prepare plans for an international undertaking: no less a scheme than photographing the whole sky down to a limit of magnitude 14. They proposed to divide the sky into eighteen zones, and to find eighteen observatories prepared to take nearly equal shares in the business.

It was a remarkable feature of this enterprise that it was undertaken with hardly any preliminary knowledge of the difficulties to be overcome. The brothers Henry had shown the possibility of charting the sky with an instrument of a certain type, and the photographic dry plate, which had not been available for earlier workers in the same field, was now an established success. But there was no general experience at the disposal of the Congress, and few of its members had ever taken an astronomical photograph. Under such circumstances it was inevitable that some initial mistakes should be made, and that resolutions should be formed and adopted, only to be broken. Few would deny now that it was a mistake to adopt the ordinary photographic object glass for making a chart. But out of the chart grew the much more important project of the catalogue, to give the places of all stars down to the eleventh magnitude, measured upon the photographic plates; and for this purpose the instrument actually adopted was probably the best. The estimate of the time required to finish the work was very naturally over sanguine. At the time of writing (June, 1911), twenty-four years after the inception of the scheme, only one observatory—Greenwich—has finished and published its share of the chart; and two observatories—Greenwich and Oxford—have practically finished their catalogues. In other observatories great progress has been made, and large sections have been published, but the end is not yet in sight.

Putting aside the chart of stars to the fourteenth magnitude—whose utility is obvious—let us consider for a moment the great photographic catalogue of all stars down to magnitude 11, which when complete will contain the places of some two million stars. Not one of these places can be determined entirely by photography; all must depend upon a sufficiency of stars on each plate observed in the older way with the meridian circle. What the adoption of photography for star cataloguing has done is therefore briefly this, that it has relieved the meridian observers of the necessity to build large instruments and to strain after fainter and fainter stars.

Speaking broadly, meridian observers may now stop at some point about the ninth magnitude, or rather brighter. If they can supply the places of these stars, photography can tie up to them all the fainter stars, practically to any degree of faintness desired.

But photography demands a very high degree of precision in these stars of reference, and meridian observers are hard put to it to furnish the accuracy desired. The Astrographic Congress has therefore to take into its counsels all the men who are engaged on the meridian observation, that by a united effort of organization and economy, the whole co-operation may proceed harmoniously to the desired end. At the last meeting of the Congress, in 1909, this enlarged scheme was put into working order, and with it the Astrographic Congress became in effect an alliance binding a great part of the world's astronomical forces—truly a splendid development of an undertaking to make a single photographic chart of the sky. All honour to the judicious guidance of the successive directors of the Paris Observatory, to the enlightened and liberal support of the French Government, and to the powerful assistance of the famous Academy of Sciences of Paris!

And honour also to the participating observatories, who with less powerful backing have laboured to make the enterprise truly international, and each country's share worthy of the whole.

The immediate aim of the International Committee of the Chart of the Heavens is to produce a chart and a catalogue. Its inevitable destiny is to produce, during the present century, a second chart and catalogue, to discover all the movements that are taking place among the fainter stars. Then will arise the question of the background, the question which has been raised before in this chapter. We have seen how the astronomy of the stars has become involved with star drifts and eddies, which threaten the foundations of much that had been reckoned secure. It is the great merit of the new photographic method that it enables us to start at the other end of the scale, when we are determining the motions of the stars. The fashion has been in the past, not by caprice but by necessity, to make the faint stars depend upon the bright; at a future day the comparison of photographs taken then, with photographs taken now, will make it possible to regard the bright stars as passing hither and thither before a distant and nearly if not quite stationary background. That, in the author's belief, is the right way to look at the problem; he will be honest and confess that few agree with him.

We cannot close a chapter on the stars without reference to the attempt which has been made to classify them in order of evolution; yet there is little that a mere astronomer can venture to say, when astro-physicists are by no means agreed. The early work of Secchi and Vogel suggested a classification of stars by their spectra. Some gave light that had suffered strong absorption in passing through atmospheres of hydrogen, and apparently of little else. In others, like our sun, the vapours of heavier metals played a large part in the absorption of light. A third series had banded spectra, suggestive of lowered temperatures. At first the classification was merely for convenience, and was simple. As instruments became powerful, and differences of detail could be recognized, the old classification was found inadequate, and new ones were proposed, depending upon minutiæ which are for the expert to appreciate. When many have been content to accumulate facts, a few ~~e sought to arrange the stars, by their spectra, in order,, and to say: These ar young, and are still growing hotter; these have reached their greatest intensity; these others have passed their prime, and those again are on their way to extinction. No one doubts that such a classification will be possible in the end; but it is more than doubtful if the end is yet in sight.

CHAPTER VIII

THE NEBULÆ

AT several points in the sky the unaided eye can see something hazy and ill-defined, which is certainly not a star, and scarcely looks like a group of small stars close together. One is in the sword of Orion, another in the constellation Andromeda, a third in Hercules; and there are others in the southern sky. One at least, that in Andromeda, had been known to the Arabian astronomers, if not to the Greeks. Galileo's telescope discovered that in Orion, and Halley rather more than two hundred years ago found the third. All three are easily seen without telescopic aid when one knows where to look. As telescopes were gradually improved these patches of light were carefully studied to see what could be made of them, and the heavens were scanned to discover more objects of the same kind. A French astronomer, Messier, was foremost in the search, so that in the year 1776 he was able to publish a catalogue of 103 of these objects, and they are still for convenience and brevity often referred to by their numbers in this celebrated little catalogue. Some of these Messier himself could see were nothing but clusters of faint stars close together; others could not be resolved into stars, and these were the *nebulæ* proper (Latin, *nebula*—cloud).

The most famous and the most enduring work of the Herschels is their catalogue of nebulæ, recorded in the course of their survey of the sky from pole to pole. When William Herschel began his methodical examination of the sky, field by field, scarcely more than a hundred nebulæ were known. When his son, Sir John, long after his return from the Cape and the southern skies, was able to bring together into one general catalogue (dated 1864) every nebula and star cluster that was known, their number exceeded five thousand; and all but a small fraction had been found by his father and himself

The nebulæ and the star-clusters were catalogued together, for the time had not come to draw any clear line of distinction between them. In early days it had often happened that what had appeared to one observer as a filmy patch of light had been resolved into clearly separate stars by the more powerful instrument of another. What more reasonable than to suspect that every nebula could be so resolved, had we telescopes sufficiently powerful? So far as was then known, stars, and stars only, shine by their own light. Until proof to the contrary was obtained the simplest, and therefore the preferable, explanation of the nebulæ was to suppose that they were clouds of stars, but so immensely distant that the stars could not be separated one from another.

Thus each builder of a greater telescope strove after the resolvability of nebulæ. But the nebulæ in general refused to be resolved. Some of their secrets they gave up, one by one. It became more and more evident that they were by no means all of the same kind. In some the light was curdled, like the filmiest cirrus cloud high up on a fine summer day; others were made of wisps and streaks like a gauze veil torn to shreds; others again were clear cut and sharp in outline, like pale ghosts of planets. Sharply separated from these were other nebulæ of a most remarkable form first revealed by Lord Rosse's great telescope at Parsonstown in Ireland—nebulæ in which spiral arms were wound about a central condensation. So exceedingly difficult to see, so much more difficult to understand and explain was this complicated structure, that astronomers with feebler instruments were not unnaturally at first a little disinclined to accept as reality the fantastic spirals that Lord Rosse and his assistants drew at the eyepiece of the great telescope. That nebulæ should be resolved was indeed to be expected, but that they should be resolved not into stars, but into spiral arms still nebulæ, was extraordinary, almost beyond belief.

Somewhat after the middle of the nineteenth century, then, the problem of the nebulæ was so far from being solved that it had become much more complicated, and apparently more hopeless of solution. The limits of telescope construction had been reached —so far at least as mere power to gather more and more light was in question. Great indeed as has been the advance in instrument construction since that day, the advance, as we have already seen, has been along the lines of greater perfection, and for sheer grasp of light no telescope has yet surpassed that constructed by Lord Rosse some seventy years ago. The next great step in our knowledge of the nebulæ was to come by the application of a new principle, of analysing the light collected by the telescope, of making it reveal by that analysis the nature of the source from which it had come.

We have already seen something of how this spectrum analysis is applied to the stars, and how it has shown that the stars are suns, and that they are like our own sun in being surrounded by layers of vapours which absorb and cut out certain of the rays, so revealing what substances they are which are vaporized above the glowing surface. The light of the stars shows on analysis that most of the rays are present, but that some have been absorbed in their passage through the overlying vapours —in more technical language, we have learned in a preceding chapter that stars give a continuous spectrum crossed by dark absorption lines. Here then we have the power to discover if the nebulæ are merely masses of stars at a great distance.

The crucial experiment was made by Sir William Huggins. In the year 1864 he turned his spectroscope to a nebula in the constellation Draco and read its secret at a glance. The light was not starlight, but something very different; light which came from glowing gas, shining in space by itself. In a short while came the explanation of the fact that the nebula in Orion could never be resolved into stars. It was not made of stars but was an immense cloud of self-luminous gases—of hydrogen, and the gas which we now know as helium, which since then has been found in small quantities upon earth, and another gas which is still unknown except by the green ray which it contributes to the spectrum of the gaseous nebulæ.

We can easily picture to ourselves the enthusiasm with which this achievement was received. Here was a difficulty which had puzzled several generations of astronomers now completely solved, and by the simplest of means. Further, the discovery gave us the new conception, that a simple gas, not condensed about a solid body, but free in space, and excessively rarefied, can shine by its own light and let us see in what arrangement it is disposed. True, the conception is not without difficulties, for we are unable to imitate in our laboratories the physical condition of the gas which is thus self-luminous in space. A very small quantity of hydrogen can be enclosed in a tube and excited by an electric discharge so that it emits light of the same kind as that which comes to us from the hydrogen in a nebula. But it is altogether impossible to suppose that the hydrogen in the laboratory tube and the hydrogen in the nebula are under conditions even remotely similar. The former is rarefied, as we understand rarefaction in the laboratory, but it is immensely more condensed than the latter. The former is comparatively warm; the latter is exposed to the absolute cold of space. The former is put into a state of intense agitation and made to glow by the electric discharge; the mechanism which excites the latter may be electric, but we have no knowledge as to how it is operated. When, therefore, we are told that the spectroscope has shown the Orion nebula to be a mixture of several self-luminous gases, we are elated for a moment at the brilliance of the discovery, but the next moment we are sobered by the reflection that the solution of one problem has but proposed to us several new ones, each more difficult to solve.

Let us pause for a moment and make some attempt to realize precisely what we mean when we say that the density of the Orion nebula must be inconceivably small. It covers quite a considerable area of the sky.

The part that can be photographed readily is roughly half a degree in diameter—the apparent breadth of the moon. And it is immensely distant. How distant we have not at present any means of determining; but we shall probably be well within the mark if we take it at 150 times the distance of our nearest neighbour among the stars. Now the sun is about one and a quarter times as dense as water, or about 1000 times as dense as the air under average conditions at the surface of the earth. A simple calculation shows us that if we assume the Orion nebula spherical its volume, compared with the volume of the sun, is expressed by the number 58 followed by twenty-one noughts. Suppose now that we took a thousand million suns, each as large as our own, and expanded them until they were of the density of air. They would then occupy a space equivalent to a million million of our sun's. That would cancel twelve of the above twenty-one zeros, and we are left with the result that a thousand million suns expanded into the size of the Orion nebula would have a density one fifty-eight thousand millionth that of air at sea level upon the earth. And even this density, inconceivably small as it is, is probably far too great. There are good grounds for believing that the whole amount of matter within range of our telescopes does not equal a thousand million suns. It is vastly improbable that any sensible fraction of that mass is contained in the Orion nebula. Hence we can hardly doubt that the real density of the gas in the nebula is immensely less than what we have found above, and we may begin to realize how powerless are our brains and our language to express what is really involved in the statement that the Orion nebula is a region of glowing gas.

But it is time that we took up again the thread of our story. And we must not run away with the idea that all the nebulæ are gaseous, for this is far from being the case. We have pictured to ourselves the delight with which Sir William Huggins examined his first nebula and found that it was gaseous. We may now imagine with what surprise he found, on turning to the great nebula in Andromeda, that it was not gaseous. Gaseous nebulæ, indeed, have proved to be the exception rather than the rule. Of the many thousand nebulæ which are now catalogued, it is probable that only a few hundred are gaseous. We must be careful to say, probable, for the truth is that most nebulæ are so faint that their light is not sufficient for analysis. We may, however, very properly argue something from appearance; and when we find that the nebulæ which are certainly gaseous differ very much in appearance from those which the spectroscope seems to tell us are certainly not; when further we find that a very great majority of the nebulæ belong in appearance to the latter class; then we may say with some confidence that the majority, the so-called white nebulæ, are very different from the small minority, the gaseous green nebulæ.

The power to discriminate, to classify the nebulæ by their apparent structure, has come with the employment of photography. The eye and the pencil are incompetent in their attempts to portray such very faint and delicate objects; the eye cannot distinguish with any certainty what even in the largest telescopes is on the borders of invisibility; the pencil of the most skilful draughtsman fails to represent those slightest variations of light and shade which are all important. How well the sensitive photographic place succeeds where eye and pencil fail may be seen in any one of the many collections of astronomical photographs which are now available, and to which reference will be made in the list at the end of this volume.

We have spoken already of the famous achievement of Lord Rosse's telescope—the discovery of the spiral nebulæ. We have spoken also of the natural half incredulity with which this surprising discovery was received. It was embarrassing, this complication of structure, so unlike anything else in heaven or in earth. But there was the consolation that the strange spiral form was rare and exceptional. It was curious and interesting and most impossible to explain, yet at any rate there was no reason to suppose, twenty years ago, that the spiral nebula was a common feature of the universe. Only a few were known, and if they could not be understood, they could at least be set apart as rare creatures, outside the ordinary run of things.

By the application of photography this comfortable belief was shattered. In the early eighties Common showed that with the invention of the dry plate nebular photography had become possible. By the middle eighties Isaac Roberts had begun his steady systematic photography of all the brighter nebulæ; ten years later Keeler with the Crossley reflector at the Lick Observatory made a great advance towards perfection of results; and in the last few years Ritchey has shown what great results can be obtained by courage and consummate skill in instrument construction.

The workers in this field of photography have not been many, for it demands uncommon qualities of skill and persistence. Even with the rapid plates of to-day the exposures must run into hours. Be the instrument ever so perfectly built and driven, it is still required that the astronomer shall keep incessant watch upon a guiding star, to ensure that there shall be not the slightest wandering and consequent deformation of the images upon the plate. To maintain this exacting watch night after night, year in and year out, whenever the sky is clear and the moon is absent, makes demands on resolution and temper which few can meet. Hence the successful photographers of nebulæ may be counted on the fingers. When they have overcome the difficulties of tuning up their instruments to the highest pitch, they have still to contend with the more formidable difficulties of the weather; and until one has tried one will scarcely believe how difficult it is to obtain four complete hours of serene, transparent sky. The hours spent upon the plates which succeed bear but a small ratio to the hours that are wasted by unsteadiness of the air, by premature clouding, by dewing of the mirror, or by the hundred other ills that vex the soul of the celestial photographer, especially in those pleasant but changeable climates where civilized man has generally made his home.

But the revolution in our knowledge of the nebulæ which these photographs have made is very remarkable. At the very outset they confirmed to the full the accuracy of Lord Rosse's early observation, that many of the brightest and finest nebulæ are formed of spiral arms, generally two. They spring from a central condensation at two diametrically opposite points, wind with more or less perfectly regularity around the centre, and are studded along their length with bright knots that look as if the nebulous stuff were there condensing into stars. Year by year the number of these spirals has grown. They have rapidly outnumbered the gaseous nebulæ; it begins to look as if the chances are in favour of nearly all the white nebulæ proving themselves spirals, so soon as they may be successfully photographed. And most remarkable of all, nearly every plate of a large spiral shows a number of smaller uncatalogued nebulæ of which many are spiral also; there are even regions in the sky where the nebulæ are more plentiful than the stars.

It would be premature to estimate the number of the nebulæ which are within reach of existing telescopes and plates; but it seems likely that the number will run into hundreds of thousands. Whoever essays to fashion a theory of the universe structure must be prepared to reckon with a hundred thousand spiral nebulæ; and at the same time he has to admit that he knows scarcely anything of them beyond their shapes and their apparent places in the sky. How far away there are, how large they are, of what they are made, whether movement is in progress in those spiral arms, whether the knots are becoming more condensed, whether the condensation will eventually produce a real star, no one can say. No one has as yet formulated any mathematical theory of the structure; no one has any real grounds for affirming that the spiral nebulæ are members of our stellar universe; still less, in the opinion of the author, is it legitimate to suppose that they are distant universes of stars outside our own.

This hypothesis that each spiral nebula is a distinct and separate universe of stars, comparable perhaps with the whole system which forms the Milky Way, has undoubtedly a grandiose attraction. But there is a good scientific rule that an hypothesis which cannot be supported or tested by facts, is not a very valuable asset. Unfortunately in this case there are hardly any facts available. Consider first what the spectroscope has to say as to the spirals. In the few cases where there is light enough to make a proper examination, the quality of the light seems to be the same as that of the stars: technically, their spectra are continuous, with dark absorption lines. Were we sure that the source was intensely hot, as the stars are, we might take this as proof that the spirals are really formed of condensed bodies something like stars. But it is a mistake to suppose that white light necessarily has a hot source. The glow-worm, the firefly, and the innumerable minute but intensely luminous creatures that live in the surface layers of the sea are sufficient to prove the contrary. Hardly anything is known, it seems, of the way in which these creatures produce their light; but it is so completely out of proportion to the size of their mechanism that we may be sure it is produced very economically, with but little expenditure of energy. If then we prefer caution to extravagance, we may very well prefer to believe that the spiral nebulæ shine white in a way that we do not understand, rather than jump to the conclusion that they must be masses of distant and perhaps only partly developed stars.

The distribution of the spiral nebulæ is very curious. In some parts of the sky they are as numerous as the stars; in others they seem to be entirely absent. In the northern sky they are found in greatest number where the stars are few, far removed from the region of the Milky Way, and from this fact some have been led to draw far-reaching conclusions which we shall find it more convenient to discuss in a later chapter. For the moment let us confine ourselves to bare facts, which are a little difficult to present without the aid of elaborate diagrams. We must appeal to the reader's knowledge of the geography of our terrestrial sphere, to provide a rough illustration of the way in which the nebulæ are arranged upon the celestial. The Milky

Way may be considered very conveniently as the equator of the sky; the region of the Milky Way we may call the celest al tropics. What then of the spiral or white nebulæ? They are decidedly arctic. It is somewhat the fashion to speak of them as so peculiarly arctic in their distribution that they are definitely condensed at the pole, and become fewer and fewer as one goes towards the equator. It seems, however, to the author that the facts are against this very formal view, and that we shall be very much nearer the truth if we liken the distribution of the spiral nebulæ to the distribution of perpetual snow upon the earth. Perpetual snow, no doubt, is rather conspicuously related to the poles, but it is very far from being distributed regularly with regard to them. Greenland and Iceland make big unsymmetrical patches on one side; on another the Rockies carry the snow down towards the equator. The Caucasus and the Himalayas, the Andes, Ruwenzori, and the snow mountains of New Guinea give us patches of ice far down into temperate and tropical latitudes. And how does our comparison fare in the south? We have not much perpetual snow in the southern hemisphere of the earth, because there is not much land until we reach the Antarctic continent. We do not for the moment want it to represent the spiral nebulæ of the southern sky, because as a matter of fact we know very little about them. There is not as yet a photographic reflecting telescope at work in the south.

Having once established this idea of speaking geographically of the white nebulæ, we shall find it convenient to deal with the green or gaseous nebulæ in the same way. And here the facts are much more simple, for the gaseous nebulæ are entirely tropical; scarcely any big ones, and no little ones at all are found outside the tropics. This rule is true of the small planetary or almost stellar globes of gas which have of late years been found in considerable numbers at the Harvard College Observatory. It is even more conspicuously true of the immense extended nebulæ in the Milky Way which are represented on the photographs of Professor Barnard and Professor Max Wolf. These objects are much the largest things in the sky, extending each over quite a number of square degrees. To call them nebulæ hardly does justice to their size, as indeed Sir William Herschel must have felt when he brought them together in a special list, not of nebulæ, but of "regions affected with nebulosity." Of late years this happy phrase has been revived, and for the first time they have been worthily portrayed. Before the advent of photography this was quite impossible, for the simple reason that they are too big to be seen in a visual telescope, very much in the same way that the zodiacal light and the Gegenschein are also too big. For it must not be overlooked that the field of view of a telescope is practically limited to a single degree, and in general the larger the telescope the smaller the field of view. Hence it is no paradox to say that an object is too big to be seen in a telescope.

When a nebula more than fills the field of view, one is conscious only that the background is dimmed and that the faint stars appear dulled for want of contrast. But by sweeping the instrument backwards and forwards one is able to trace out the nebulous region to this extent, that one can say: Here the background is clear and black; there it is full of vague light and the stars are dulled. The camera, with its big field ten or fifteen degrees square, has superseded this unsatisfactory groping, and is now able to let us stand back, so to speak, and take in at a glance these wonderful regions. Let us imagine ourselves looking at one of Professor Barnard's pictures of the Milky Way in the constellation Scorpio, near the star lettered ρ . We are struck at once by a curious dark lane, almost empty of stars, which runs across the plate for several degrees, and appears to be blacker than the background of sky. For a moment we wonder how this is possible, and then we notice that over the greater part of the plate the background is not really black at all. We are in one of the regions affected with nebulosity.

To obtain the spectrum of such a very faint and extraordinarily diffuse object has proved impossible up to the present; but all analogy is on our side if we assume that the nebulosity is of the gaseous kind. There are, therefore, immense regions of space filled with a luminous haze; and we may go back to our geographical comparison to find a parallel. On the high veldt in South Africa the air is marvellously clear, and it is almost impossible for a visitor from misty England to realize that mountains apparently just outside the town are really fifty miles away. But further north, in tropical Africa, the case is very different. There the air is almost continually filled with a thick haze, the Harmattan, which is eloquently described in the doleful reports of the surveyors. So it is in the sky. Away from what we have called the tropics, the Milky Way belt, the background is clear; but extensive regions in the tropics are filled with a luminous haze which partially dims the stars. And what are we to make of those dark lanes which interpose apparently between us and the background, blotting out haze and stars alike? They remind us that space may be filled with the invisible as well as with the visible, and they introduce us to the difficult question of the absorption of light in space. Whether it is possible to detect a universal absorption of light or want of transparency we must discuss in another chapter. But we shall do well to learn from these extensive nebulæ that there is plenty of evidence of local fogs; and we have a ready means of deciding whether the dark fog is gaseous—the same gas as the luminous part of the nebulæ—or solid dust. The former may cut out the nebula, but cannot do much towards dimming the stars. The latter will be equally effectual on both; and since both nebulæ and stars are in some cases equally affected we shall have no difficulty in coming to the conclusion, at least provisionally, that the dark lanes in the Milky Way are due to something like dust interposed between us and the stars.

In one celebrated case we had independent evidence of the existence of such streams of dust. Many will remember the "new star of the new century" which blazed up in the constellation Perseus early in 1901. In the space of a few hours it rose from insignificance to a splendour greater than any other star in the northern sky, and within a few weeks it was gone again. This veritable light explosion sent a blaze of light speeding through space at the rate of nearly two hundred thousand miles a second, able to illuminate suddenly anything that came in its path. And something of the kind was seen to happen. The direct flood of light reached us early in the year. In the autumn it was discovered that at some distance from the star—now grown comparatively dim—patches of nebulæ were appearing, and moving away from the star. Now motion in a nebula was unheard of, and motion so quick as this was unimaginable, unless it was the motion of the light itself that we could observe. The explanation is not altogether free from difficulty, but it seems to the author that the moving nebula round Nova Persei is best explained by the idea that the light explosion travelling outwards illuminated drifts of celestial dust as it passed, and thus allowed us to see them for the moment.

In a chapter dealing with the nebulæ it would be impossible to pass in silence the celebrated Nebular Hypothesis; and yet it is not clear that there is much of value to be said at the present time in a work such as this. For in recent years the subject has been highly controversial. Laplace, the eminent author of the hypothesis, never worked it out in detail; he contented himself with sketching in a general kind of way how a sun and a system of planets circling round him might be formed by the condensation of a nebula. On the mathematical side the difficulties are formidable, and they do not tend to disappear with years. It is natural, then, that speculative astronomers should be on the look-out for some visible example of the processes which Laplace imagined; that they should scrutinize the details of the nebulæ as they are revealed by photography; and should seize on this or that detail as a verification of the hypothesis. Now to make a system of bodies out of one nebula, condensation must take place about a number of centres. We can find nothing of the kind in the gaseous nebulæ; but the spirals are very conspicuously condensed about centres which are strung along the spiral arms like beads upon a string. We cannot hope within a reasonable time to see signs of the actual operation of the process. There are, however, an immense number of spiral nebulæ in all apparent stages of condensation, and it is not very difficult to pick out a sequence which serves to illustrate a supposed succession of stages in the process. So far, so good. But the cautious astronomer is more than justified in feeling some doubts whether it is legitimate to appeal to the spiral nebulæ to illustrate a theory which was devised to explain the formation, not of a gigantic system of stars, but a single star and its attendant planets. We have laid stress before upon the enormous size of the spirals, even when one takes the most modest estimate of their distance from us. What we see in them may be the evolution of a star system; it can hardly be a solar system. And it is a well-known principle in mechanism that a device which will work perfectly on one scale, will not work at all upon another. We appeal to gravitation as the moulding force when things are reasonably close together; but it is by no means certain that gravitation is the sole or even the most potent force in controlling an immensely extended nebula. For these reasons it may be said to be rash even to the point of danger, to summon the nebulæ as witnesses to the Nebular Hypothesis

CHAPTER IX

THE MILKY WAY

On a clear but moonless night in July or August, as soon as it gets dark, suppose that we lie back in a garden-chair and study the sky from the zenith down towards the south. The most conspicuous feature of the sky is the broad irregular band of milky light that stretches through Cygnus, Aquila, Monoceros, down to Sagittarius and the constellations further south. We notice at once that it is not uniform, but lies in bright patches with dark intervals between. We see also that it is straight, or to be more precise, that it lies along a great circle of the sky. If we travel south we find that this straightness continues through the southern constellations, and brings it back again to the north, along that part of the Milky Way through Cassiopeia, Perseus, and Auriga, that we in the Northern hemisphere see in the winter months. So far as we may speak precisely of such a broad and irregular belt, we may say that it divides the whole sky into two equal halves—a fact which immediately arrests our attention as likely to give a clue to the enquiry we have always in mind, the problem of the structure of the Universe.

In the northern sky there is nothing apart from the Milky Way which resembles it at all; but early travellers to the south brought back news of wonders not only on earth, but in the sky also. The learned doctor Pietro of Abano has preserved for us the tale told him by Marco Polo, that in the south he had seen "a star as big as a sack," which he described as being like a piece of cloud, and having a long tail. This early reference to the Great Cloud of Magellan is made doubly interesting by a figure reputed to be drawn by Marco Polo himself, which, though not very successful in representing the most wonderful object in the whole sky, is worthy of all respect as the first attempt to portray a celestial wonder for the benefit of those who had never seen it. The Clouds of Magellan—for there are two of them, the greater and the smaller—are named by us after the great Portuguese navigator, from the account which his fellow adventurers gave on their return from the famous journey which cost their leader his life in the Philippines. They are the objects, far more than the Southern Cross, which interest an astronomer visiting the south for the first time, for he has learned that the greater of the clouds is a veritable celestial museum, crowded with the most beautiful specimens of star clusters, nebulæ, and stars of curious kind; and he is anxious to see what has, within recent years, been regarded as the key which may unlock the whole secret of the Universe. Let us examine the line of thought which leads to such a fascinating possibility.

It was one of the earliest discoveries of the telescope that the Milky Way can be resolved into stars. Along its central line the stars lie thick; towards each side their numbers fall off, until at its poles they are relatively few. This is the main fact which invests the Milky Way, or the Galaxy as we may prefer to call it, with a profound significance. This fact is responsible for the one feature that is common to all theories of the Universe, that it is greatest and richest and most extended in the Galactic plane.

The grand survey of the sky which was made by the Herschels, father and son, provided the foundation upon which all but the most recent investigations were based. They determined by their "star gauges" or counts, the way in which the richness of the sky in stars varied from place to place; and they catalogued many thousands of star clusters and nebulæ. When all this material was discussed and examined, there emerged several important principles.

(1) As one approaches the Galaxy the number of stars per square degree continually increases.

(2) The star clusters (other than the globular clusters, which are nearly confined to one half of the sky) lie closely along the Galaxy, and evidently form part of it.

(3) The nebulæ, on the other hand, seem in general to avoid the Galaxy, the region where they are found in greatest number being about the northern galactic pole.

(4) But star clusters and nebulæ, elsewhere seemingly antagonistic, are brought together in profusion in the great cloud of Magellan.

Upon these facts the philosopher Herbert Spencer based his celebrated argument, that the stars and the nebulæ, just because they seem to avoid one another, must for that very reason be opposite parts of one system; were they independent of one another, they would not avoid one another so markedly

The very fact of avoidance, according to this argument, is convincing evidence that the stars and the nebulæ are not indifferent to one another. We must look then for some great cause which has been potent to set the stars on the one hand, the nebulæ on the other, in one immense ordered system embracing all the universe, and following laws which we may hope to understand by study of their effects.

This argument has had a great success for many years, so that at last it needs a distinct effort to ask ourselves: Are the facts as to the distribution of the stars and nebulæ so clear cut and definite that they admit only of this one interpretation? For a long time it has seemed to the author that this is not the case, and in a lecture delivered before the British Association meeting at Bloemfontein in 1905, he ventured to try the experiment of denying that there is only this one conclusion possible. To a great extent one's judgment on such a question must depend upon temperament. One man will be seeking always to trace how a complex and delicate system may have evolved from something simple and widespread. Another will instinctively prefer to believe that there is diversity in the Universe, and that no single controlling principle has moulded all its constituents as essential and intimately related parts of a single mechanism. In short there may be diversities now because there were diversities to start with, however that start may have been made.

Let us, however, before we attempt to pursue this enquiry, try to see how the immense growth of our knowledge since Herbert Spencer's day may have undermined the foundations of his argument. In the first place, when Spencer wrote, the nebulæ, if classified at all, were divided into irresolvable nebulæ, and those which it was supposed showed some sign of resolvability. More recent enquiry, and especially the application of the spectroscope and the photographic plate to the study of the nebulæ, has shown that the distinction drawn fifty years ago is illusory. No nebula is resolvable; but we have a new line of distinction to draw between the gaseous and the white or spiral nebulæ. The larger gaseous nebulæ do not by any means follow the same law of distribution as the spirals; on the contrary, they are found most often in dense regions of the Milky Way, entangled with the stars, and sometimes, associated with matter absorbing the light of the stars beyond them. Such are the great extended nebulæ in Scorpio and Ophiuchus which have been so magnificently shown on the portrait lens photographs made by Barnard at Mount Hamilton and Mount Wilson.

The smaller gaseous nebulæ, of round and definite form, the so-called planetary and stellar nebulæ, are not so markedly confined to the Milky Way, but at any rate show no tendency to avoid it. We must, therefore, exclude them from any statement that the stars and the nebulæ are oppositely distributed.

The gaseous nebulæ, are, however, a small minority in the total ; by far the greater part belong to the class of white nebulæ, of which the most conspicuous are spiral, and which very probably are really all spiral. In the last chapter we have seen how the number of these is increasing by leaps and bounds as the well known objects are photographed with powerful reflecting telescopes. In the face of such a fact, it becomes impossible to say with certainty what is the distribution of spirals in the sky, for a dozen plates may conceivably reveal a thousand new nebulæ, in a part of the sky supposed to be nearly free from them. The probability that this will happen is not however very great, for it seems that the new nebulæ are most frequent precisely in those parts of the sky where the well-known nebulæ are most thickly strewn. A recent study of the distribution of spiral nebulæ made by the author shows, in fact, very little sign that the newly discovered small nebulæ differ at all in their distribution from the brighter and better known. So far as one can see, the main facts about the distribution of spirals are indicated by the fifty brightest, and are confirmed without sensible modification as one brings into account list after list of new discoveries.

This study does, however, show clearly that the older views as to the distribution of the white nebulæ require very considerable modification. It is usually said that they cluster closely about the poles of the Galaxy, and avoid the Galaxy itself. The clustering near the northern pole is certainly very marked; that about the southern is so little marked that it can scarcely be said to exist. Moreover, the spiral nebulæ do not markedly avoid all parts of the Galaxy, but only certain stretches of it, and it is quite evident that when Sir William Herschel wrote that the nebulæ lie in a zone of the sky roughly at right angles to the Galaxy, he was more nearly right than were those who held the more modern view, that they are clustered about the Galactic poles.

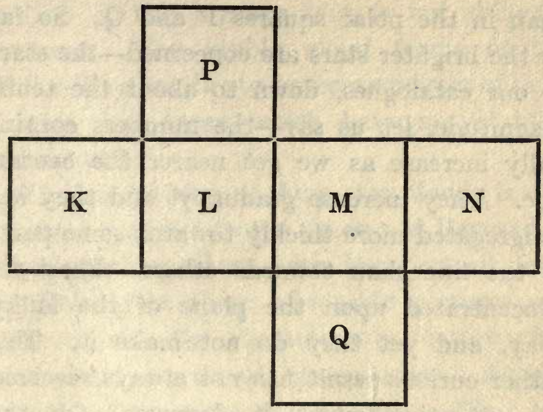
In the chapter on Nebulæ we have given a geographical illustration of the distribution of spirals, comparing them with the snow mountains of the world. A more formal statement may be obtained from our figure, in which the celestial sphere is supposed to be projected upon a cube circumscribing it, and so arranged that the Milky Way runs along the centres of the middle line of squares into which the cube is unfolded. The poles of the Galaxy will then lie in the centres of the top and bottom squares. For convenience of reference we letter the Galactic polar squares P and Q, while the four squares in which the Milky Way runs are lettered K L M N. We can now express what is known of the distribution of spiral nebulæ very briefly :—

They are exceedingly numerous in P.

They are fairly numerous in Q, L, and N.

There are a few in M.

There are none at all in K



This last is a very striking fact, that in a space of one-sixth of the whole sky there is not at the moment of writing a single spiral nebula known; doubtless further photographic search may bring to light a few; but it is significant that whereas long exposure photographs with a powerful reflecting telescope reveal great numbers of new spirals in other parts of the sky, in this great region they have up to the present produced none.

With the aid of this diagram we may now put very briefly our knowledge of how other classes of objects behave, with reference to the Galaxy

The stars are immensely more numerous along the Galaxy, in the squares K L M N than in the polar squares P and Q. So far as the brighter stars are concerned—the stars in our catalogues, down to about the tenth magnitude, let us say—the numbers continually increase as we get nearer the central line. They increase gradually, and they are congregated more thickly towards some parts of the line than towards others. They are concentrated upon the plane of the Milky Way, and yet they do not make it. This rather curious result has not always received the attention which it deserves. On the contrary, it would seem that many writers have been over-ready to argue thus: The Milky Way is composed of masses of stars; the stars in our catalogues are concentrated towards the Milky Way; it must be they that form it. In reality this argument is quite unsound, as can be proved very easily. We have only to remember that in the visible Milky Way there is a great dark rift up in the constellation Cygnus; and then to look for it in the celebrated chart on which Proctor plotted the 300,000 brighter stars in the northern sky. The general course of the Milky Way can be traced easily enough by the conspicuous crowding of the stars along it, but the rift is nowhere to be seen. The explanation of this strange fact is not difficult, however. The visible cloud forms of the Milky Way are due to immense masses of stars fainter than those which have as yet been catalogued.

The recognition of these star clouds is due in great measure to the success of Barnard in photographing them with the portrait lens. In looking at these admirable pictures one sees at once the characteristic feature of a star cloud, its almost sharp edges. As one travels down towards the Milky Way the stars increase gradually in number; then suddenly one runs into a star cloud, and the richness increases tenfold. The conclusion is almost obvious: the Galaxy is no single structure, no single mass of stars grouped according to a definite law pervading the whole, but is an assemblage of some few hundred more or less distinct clouds of stars, tumbled roughly into one plane.

Among these clouds of stars there is considerable diversity. Some seem to consist of stars only; in others there are immense stretches of the vague and almost shapeless nebula which we recognize as having all the appearance of gaseous nebula, though its exceeding faintness has prevented, up to the present, the gathering of any very definite evidence upon this point. In some places the nebula is traversed by the long dark lanes which have been already described, characteristic especially of the Milky Way in the regions of Scorpio and Sagittarius, and to all appearance cutting out the light of the star clouds. These dark lanes present a most interesting problem, far easier to state than to solve. Is the obstructing medium just on the near side of the star cloud, or is it very much nearer to us? When direct evidence is wanting we must content ourselves with the very general line of argument that the dark lanes are apparently in the gaseous nebula, and the nebula is not found except in apparent connection with the star clouds; we may for the present suppose, then, that the connection is not only apparent but real, and that these great nebulæ are part of the distant structure of star clouds.

There are also closely associated with the Milky Way stars of a peculiar type—the Type V of the early Harvard classification. These stars are not common, only about one hundred being known in all; and they show a remarkably close concentration on the central line of the Milky Way—or along certain stretches of the central line; for their distribution in longitude is far from uniform. Placed as they are almost exactly upon a great circle of the sky, they show that there is something extraordinarily flat in their arrangement, and one naturally expects to find them in the dense star clouds along the Galaxy. It has been remarked, however, by Newcomb that they display no such preference. Some are found in the star clouds, and others in the vacant spaces between the clouds. Why, then, they should be arranged in their very well marked plane remains for the present one of the most interesting puzzles that we have to solve.

A considerable bunch of these Type V stars is found in the greater Cloud of Magellan, associated with the globular star clusters, the loose star clusters, the star clouds, and the numerous nebulæ which make up that wonderful object. If one charts the fainter stars one can trace in the result the course of the Milky Way, and the two Clouds of Magellan. If one charts the loose star clusters or the Type V stars one can do the same; the Milky Way and the greater Cloud of Magellan are full of them. If one charts the nebulæ in general—of which, as we now believe, the greater part are spirals, one finds the Milky Way zone made rather conspicuous by their absence, but the greater Cloud of Magellan made brilliant by their presence in great numbers. So that an inspection of the chart gives rise to the belief that here we have brought together objects which in other parts of the sky seem to avoid one another. This is the fact to which we referred at the outset, the suggestion of the idea which has found some favour in recent years, that we may seek in the greater Cloud of Magellan for the key to our long standing puzzle, why nebulæ and stars should avoid one another.

It will not have escaped the notice of the reader that the crucial question is, are the nebulæ in the cloud spirals, or are they gaseous. If, like the majority of the nebulæ, they are spirals, then we must consider it remarkable that they are associated here with Milky Way objects. But if on the other hand they are gaseous nebulæ, then there is nothing remarkable in their association with objects which are equally with them characteristic of the Milky Way. It may seem strange that on such an important point we should still be left in doubt; but it must be remembered that the only instrument which is thoroughly competent to decide the matter is the modern reflecting telescope adapted to photography, and there is not yet such an instrument in the southern hemisphere. It is, however, possible for instruments of other kinds to do something to decide the problem: they have picked up spiral nebulæ in other parts of the southern sky, but never in the greater Cloud of Magellan. Negative testimony such as this must of course be used with all reserve. But on a recent study of all available evidence it does seem to the writer that the balance of probability lies in the direction of finding that those nebulæ are gaseous, like the extensive nebulæ of the Milky Way; that they have intruded themselves on our notice because they are particularly bright and particularly numerous in that region, to such an extent that they have reversed the rule which holds elsewhere—the rule that spirals are very much more common than gaseous nebulæ.

If in our attempt to disentangle the threads of a complicated problem we have laid too much stress upon the doubts and difficulties which beset it, we must excuse ourselves upon the ground that a difficulty defined is half-way solved. The difficulty in this case is perfectly definite. In the comparatively few observatories of the south no opportunity has yet been found to instal a photographic reflecting telescope, and to do for the southern hemisphere of the sky what Isaac Roberts and Keeler have done for the northern. Such a telescope is not in fact—or has not been in the past—an instrument usually found in a Government or University observatory. In an earlier chapter we have already remarked upon this, and have shown that the mirror has often done its best work in the hands of enthusiastic amateurs. For the moment the great success of the five-foot reflector on Mount Wilson has rather obscured this fact; but it must be remembered that Professor Ritchey was an enthusiastic instrument-maker for many years before he became professionally associated with an observatory, and his present success should encourage rather than dissuade an amateur who might be contemplating filling that void in our knowledge of the southern nebulæ which for the moment leaves incomplete our survey of the Milky Way.

We have found reason for interpreting thus our present knowledge of the Milky Way: It is made of a great number of more or less distinct and separate star clouds, tumbled roughly into one plane. Probably a few of the nearer clouds supply that condensation of stars towards the galactic plane which has an undoubted relation with the Milky Way, and yet is not responsible for its visible aspect. Perhaps the stars in the parts of space nearer our sun do but form one of those star clouds. At least it is certain, from the precision with which the Milky Way divides the sky into two equal halves, that the sun lies pretty close to that plane. But can we go further, and say whether our own star cloud is towards the middle, or near the borders of the whole assemblage. Probably we can. The question whether or no the universe has an infinite extent is a very difficult one, and its investigation is much complicated by the probability that light is gradually extinguished in its passage through space, so that our range of telescopic vision may be bounded. If that is so, then of course an observer at any point of a very large universe must imagine himself at the centre, since he can see for a limited but equal extent in any direction, and the signs which he takes as evidence the falling away of star density near the limits of his vision are merely the results of the extinction of light in its passage through space. Apart, however, from this difficulty, the near equality of brightness of the Milky Way all round its circumference seems to assure us that we are situated somewhere towards the centre of the *visible* universe; more than that it is impossible to say

But let us avoid most carefully the error into which more than one writer has fallen, of imagining that this result gives us any pre-eminence of position in the universe. Our sun seems to be one of the stars that form one of the star clouds lying towards the centre of the visible universe, but it is in no sense more favourably placed than any one of ten thousand other suns. There is no justification whatever for asserting that modern astronomy assures us we occupy a particularly favourable, perhaps a unique position: not the slightest vestige of an excuse for concluding that our own earth, owing to that unique position, may very probably be the only abode of life.

CHAPTER X

ASTRONOMY IN DAILY USE

There can be no doubt that a science gains very much when it is able to play a practical part in everyday life; when it can, so to speak, earn its own living. Only a few men have the intellectual capacity to revel in the intricate theories which are beautiful of their kind as exquisite works of art. A very much larger number find keen pleasure in the beauty of the facts and of the simple laws which govern the universe, and there is no danger that its study will ever be neglected for want of widespread interest and general curiosity to know as much as possible of all the bodies in the sky. But it is of real importance that the science of astronomy can earn its living by rendering direct practical service to all mankind. We propose, in this last chapter, to see what these services are.

The most obvious, and the one most taken for granted, is the supply of accurate time, which yearly becomes of greater importance in industry and commerce. The day is past when each city can keep its own local time. Railways and telegraphs have made it imperative that as far as possible a country should keep one standard time throughout, while the great increase in the international traffic has made it at least very desirable that when there must be a change, the change shall be an exact hour. Further the early predominance of British shipping and the wide use of British admiralty charts made the meridian of Greenwich the natural zero to adopt when it became a question of agreeing upon some one meridian for general use. Thus it has come about that a great part of the civilized world bases its time on Greenwich, in so far that the time which the official observatories send daily or hourly by telegraph differs by an exact number of hours—occasionally with an odd half hour thrown in—from the time which Greenwich itself distributes throughout England

It is a common mistake to suppose that the time is determined by observing the sun at noon. The mistake is natural, for the time we keep is mean solar time. Nevertheless, it is determined not from the sun directly, but from the stars, which is possible because we now know very accurately the relative position of the sun and of the stars at each instant; and is preferable, not only because it is both easier and more accurate to observe a star than to observe the sun, but also because the use of the stars makes it possible to determine the time whenever either by night or day the sky is clear for a few minutes. Had we to depend in England upon catching the sun exactly as it crossed the meridian, it is probable that our clocks would sometimes be rather seriously wrong—wrong at any rate by a second or two, which from the point of view of strict time-keeping is very serious. A determination of time, then, consists in observing one of the well-known "clock" stars, as it crosses the wires in the field of view of the transit instrument mounted in the meridian. The observer has a kind of telegraph key in his hand. As the star comes to a wire he taps the key, and a record is made on the chronograph sheet, alongside the signals which the clock itself is making automatically every second. It will be readily understood, without going into details, that this observation gives the time as shown by the clock at which the star crossed the meridian. The clock star list in the Nautical Almanac gives the time which the clock should have shown at that instant a comparison between the two gives the error of the clock.

Now it is nothing to the discredit of the clock that it has an error. No clock will go perfectly right for ever; no clock likes being interfered with more than is absolutely necessary; and no clock of the highest precision can be set backwards or forwards by less than one whole minute without unnecessarily interfering with it. Hence the clock of which we are speaking is rarely right. It gradually accumulates an error one way or the other until it is a whole minute wrong and then the minute hand is altered. "Clocks," said the first lecturer on astronomy whom the author ever heard, "clocks always have errors; you observe them and allow for them, as you do with your friends."

The clock of which we have been speaking is a sidereal clock, keeping star time, which is very convenient in the observatory, but gains a whole day a year on the solar time which is more convenient outside. It is, however, an easy matter to compare the sidereal with the mean time clock. The Nautical Almanac tells what should be the difference between the two at any instant, and the error of one being known you have the error of the other. Now this latter error must be corrected before the time signal goes out. The correction is probably only a fraction of a second, but we require a delicate and certain way of applying this to the clock with as little disturbance as possible. It can be done by adding or taking away little weights from a dish on the pendulum rod; but a prettier way is to use some arrangement of electro-magnets to hurry or retard the swing of the pendulum until the desired correction is obtained. Thus it is possible to keep right within a small fraction of a second the clock at Greenwich which sends the time signals to the General Post Office. These signals are transmitted hourly or daily over the telegraph system to every post office, and can easily be made to control public clocks or drop public time balls wherever the local authorities are sufficiently keen to provide the small outlay necessary to give their city really accurate time.

The recent development of wireless telegraphy has made possible a remarkable extension of this time service. At stated times, both by night and day, the wireless stations at the Eiffel Tower in Paris, and at Nordreich on the German coast, send out time signals which can be picked up over a great part of Europe and far out into the Atlantic. We shall see immediately the immense value of this new development.

On land, in a civilized country, the ordinary man is apt to take the provision of time as a matter of course, and hardly realizes that he is indebted to astronomy for it. At sea he can hardly fail to know that the ship's officers are constantly taking "sights" of the sun, moon, or stars, and are depending upon these observations to find the place of the ship. But there is a great deal of misapprehension as to the actual process, for it is commonly supposed that the observation of the sun at noon gives the complete position of the ship, and enables the captain to set the ship's clock. In reality the noon observation gives only the latitude, which it does directly and simply. The equally important longitude must be found by an indirect and much more elaborate process. Early in the morning, and again late in the afternoon, the navigating officer takes a "sight" of the sun which after a brief calculation gives him the local time. Now the longitude, east or west of Greenwich, is the amount by which this local time at the ship is fast or slow of Greenwich time which the ship must carry with her, or obtain in some way independently. She carries it with her by means of chronometers which have been brought to an astonishing degree of perfection since the days when the celebrated John Harrison gained the prize of £20,000 offered by the Government for a time-keeper which would keep the time at sea within specified limits of error. But chronometers, even the best, sometimes go wrong, and we have provided an alternative, that the ship may obtain the Greenwich time independently. The historical method of so doing is by "lunar distances," or more briefly, by "lunars"; that is to say, by measuring with the sextant the angular distance of the moon from a bright star or planet, and after a somewhat difficult calculation comparing the result with the distance predicted in the Nautical Almanac. On the first proposal of this method it was found impracticable, for neither the motion of the moon, nor the places of the stars, were known with anything like sufficient accuracy. To remedy this defect King Charles the Second founded the Royal Observatory at Greenwich, for the benefit of his seamen, and since that day the first duty of Greenwich Observatory has always been to improve the catalogues of star places and to observe at all opportunities the position of the moon.

Of late years the famous method of "lunars" has fallen completely into disuse; partly because chronometers have become more reliable; partly because steam has made voyages so much more rapid that the chronometers have in any case less time to go wrong; and partly because the vibration of a fast steamship is so great that it is impossible to make observations from her bridge with the required nicety. Finally, the establishment of the wireless service of time signals has already made it possible for a ship to pick up Greenwich time when hundreds of miles at sea, or to compare one ship's chronometers with another's whenever the two come within wireless range. The day is probably not far distant when such a service will be almost world-wide.

Besides the processes of finding the latitude and longitude, which in their effect, at any rate, are familiar to all, there is another observation made at sea of which one hears far less, but which is equally important, and even more frequently performed; the determination of the error of the compass. As ships become larger and faster, an error of half a degree in the compass becomes more and more important and at the same time the difficulty of keeping the compass free from the disturbing attraction of the iron in the ship becomes more and more serious. Hence it arises that in a well run ship the most frequent astronomical observation made is the sight of the sun with the standard compass, which gives the deviation of that compass from the true north.

It will be sufficiently obvious that what applies to the mariner applies with slight modifications to the explorer on land. To find where he is he must make his observations of the sun and the stars as the mariner does, and the first rude maps of an unsurveyed country depend very largely upon such astronomical positions. Afterwards comes the surveyor with his more precise methods; but he again has to rely upon astronomical observations for a fundamental need. With his theodolite and plane table, it is true, he might make a map which would show the whole country's topography without using sun or star. But in that case the map would be lacking in one serious respect—there would be nothing to show where in the world it was, nor which way up it should be viewed. The surveyor has, so to speak, to pin his map down upon the world, in the right place, and in the right orientation, and this he can do only by the methods of field astronomy.

Moreover, he relies in yet another way upon astronomy, for in the process of his map-making he needs to know the size and the shape of the earth. Geodesy is the science which combines the highest and most refined processes of survey with some of the most delicate operation of astronomy. Without going into details it is easy to see how the geodesist must proceed. Suppose, for simplicity, that by his survey processes he has measured the distance between two points, one of which is due north of the other. The distance between these two points he expresses in terms of the standard unit of length adopted by his country. He determines in effect that the distance he has measured upon the earth is so many times the distance between two fine lines on the standard bar which is preserved with all imaginable care as the absolute standard of length. He next determines astronomically the latitudes of his two points; that is to say, he finds that they are so many degrees, minutes, and seconds of arc apart. It needs now but a very elementary knowledge of geometry to see that when we know a length in degrees, and also let us say in feet, it is easy to find in feet the size of the whole circle, of which the part measured is an arc; it is easy, that is to say, if the curve is really a circle. But is it? That can be determined only by measuring different arcs, at different distances from the equator. When this is done, it is found that a degree near the poles contains more feet than a degree near the equator; the immediate inference is that the earth is not truly a sphere, but is somewhat flattened at the poles; and the amount of the flattening is determined from the discordances in the measured lengths of a degree in different latitudes. The process is as old as the time of Ptolemy the Alexandrian, who actually measured in a rough way the size of the earth from his observations in Egypt.

After a regrettable interval of some eighteen centuries work has now been resumed in Egypt, which finds itself on the line of the great meridian arc 30° E. of Greenwich, passing over the greatest north and south extent of land. From the northern shore of the lovely harbour of Hammerfest, near the North Cape, down to the mouth of the Danube, was measured in the middle of the last century by the joint efforts of Russia, Norway, and Sweden. Starting from the south of South

Africa, many years later, the various governments of South Africa, British and Boer, have measured up to near Lake Tanganyika, to the borders of German territory. North of that the British have begun again, on the Uganda-Congo boundary; and British officers have been responsible for the new work in Egypt. Much remains to be done, in tropical Africa, where the actual field work is terribly exacting, and through Asia Minor and Turkey, where the political difficulties may prove more formidable than the natural. Gradually, however, both along the 30th meridian, and in India, in the States and Canada, in Central Asia and in Central America, the surveyors are measuring their triangles and observing their latitudes astronomically, adding step by step to the accumulating knowledge of the true size and shape of the earth.

A curious outcome of these researches deserves a passing mention. One would hardly expect that the science which is concerned with regions far outside the earth could tell us much of the inside of the earth itself. Yet it is the fact that nearly all our knowledge of the earth's interior is derived from astronomy and sciences closely allied. The complicated phenomenon of the " variation of latitude" seems to show that the earth as a whole is about as rigid as steel; the tidal investigations of Lord Kelvin and Sir George Darwin are against the old idea that the interior is molten; and a most interesting chapter of modern geodesy has established the fact that the great mountain masses of the world, the Caucasus, the Rockies, the Tibet plateau, are, nevertheless, balanced by defects of density below them, just as if they were icebergs floating in the sea.

We have no space to follow further afield the ways in which astronomy is ever lending a helping hand in the operations of other sciences or in good government and administration; but we may, in conclusion, spend a few moments in considering how our everyday lives are affected by stern astronomical facts. We have in the last few years seen a serious attempt to promote "daylight saving" by putting the clock forward an hour during the summer months. It appears to the author that the advocates of this scheme do not always recognize the hard facts about the partition of daylight between summer and winter. The truth is that in any country more than fifty degrees from the equator, the winter day and the summer night are inconveniently short; when one approaches the latitude 60° there is practically no darkness for two or three months in summer. For such countries the summer daylight is more than ample; the difficulty indeed is of the opposite kind, that it is hard to get darkness in which to sleep. For a period of six or seven weeks in spring it would be pleasant to have a longer evening; for the corsponding period in late summer the benefit would be more doubtful, when the day is apt to be too hot, and the cool dusk of the evening is to be desired rather than postponed. Thus a change which might be beneficial in spring and of more doubtful advantage in late summer, becomes almost disadvantageous at midsummer, and this because the length of the day is already so exaggerated in the higher latitudes. Man does not like going to sleep by daylight or getting up in the dark. To some extent he has already to do both when he lives as far north as England or Canada; he will have to do more of both if the clock is put forward an hour for nearly half the year. No alteration of the clock can change the unchangeable fact of astronomy, that the plane of the equator is inclined nearly 23½° to the plane in which the earth moves round the sun.

August, 1911.

NOTE ON BOOKS

The number of books, especially of elementary and popular books, dealing with the many sides of Astronomy, is so great that it is difficult to make a fair selection.

By far the best text-book is Young's *General Astronomy* (Ginn & Co.), which gives a concise and simply expressed account of both theory and practice. An excellent smaller book is the *Primer of Astronomy* by Sir Robert Ball (Cambridge University Press). The same author's *Popular Guide to the Heavens* (G. Philip & Son), contains star maps, good photographs of nebulæ and star clusters, and much information useful to the amateur astronomer. A small book by the late J. E. Gore, *The Stellar Heavens* (Chatto & Windus), gives a valuable summary description of the most interesting objects among the stars and nebulæ. A most valuable account of modern theories of the structure of the universe is to be found in the late Professor Simon Newcomb's book, *The Stars: a Study of the Universe* (John Murray). For an elementary, but not easy, account of tidal phenomena the student must read Professor Sir George Darwin's work, *The Tides* (John Murray). An excellent small book on spectroscopy is *The Spectroscope and its Work* (S.P.C.K.), by Professor H. F. Newall.

For the history of our subject, we must refer to the late Professor Grant's *History of Physical Astronomy* (out of print, but in all good libraries), and to the late Miss Agnes Clerke's well-known *History of Astronomy in the XIXth Century* (A. & C. Black). The same author's *The Herschels* *and Modern Astronomy* (Cassell & Co.) is good; and the *Reminiscences of an Astronomer* (Harper Bros.), by the late Professor Simon Newcomb is a delightful book.

To keep abreast of recent work the student must refer to the periodicals. *The Observatory*, published monthly, edited unofficially from the Royal Observatory, Greenwich, gives reports of the meetings of societies, reviews and notices of recent work, and general astronomical news. The *Journal of the British Astronomical Association*, the *Publications of the Astronomical Society of the Pacific*, and the *Journal of the Royal Astronomical Society of Canada*, are all full of interesting matter, largely unmathematical. The *Monthly Notices of the Royal Astronomical Society*, and *The Astrophysical Journal* are more severely technical, but contain also the best celestial photographs.

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