An atlas of representative stellar spectra from 4870 to 3300

Huggins, William

London, 1899

Chapter I.

CHAPTER I

CHAPTER I

HISTORY OF THE OBSERVATORY, AND THE WORK DONE THEREIN

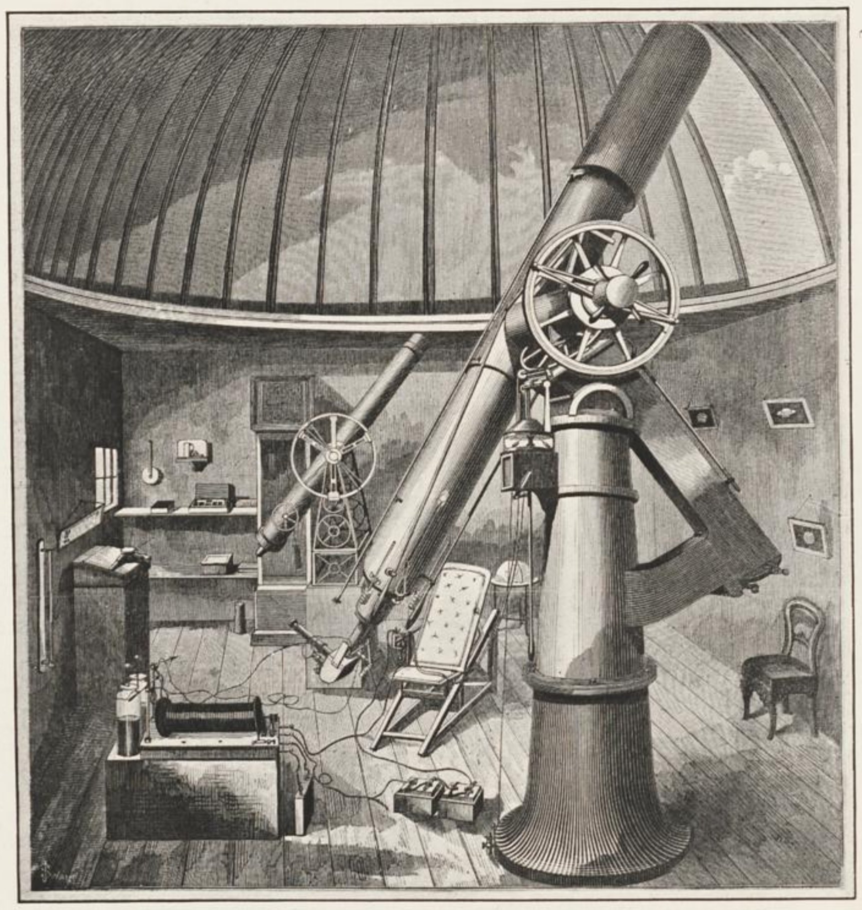
Latitude	51° 26′ 47″
Longitude W.	0° 6′ 56″
Height above the level of the sea .	177 feet.

HE Observatory was erected in connection with my private dwelling-house at 90, Upper Tulse Hill, London, in 1856. The principal instrument at that time was an equatorially mounted achromatic telescope by Dollond, of 5 inches aperture.

Two years later this instrument was taken down to make room for a larger telescope furnished with an object-glass, 8 inches in diameter, of very great excellence; the early work of the distinguished American telescope maker, Mr. Alvan Clark. The telescope was mounted equatorially and furnished with a clock-motion by Mr. Cooke of York.

Heading.—View from the garden, showing the southern horizon. Drawn by Lady Huggins.
Initial.—View from the garden, looking north-east. Drawn by Lady Huggins.

The illustration shows the interior of the Observatory as it was in use for spectroscopic work, from 1860 to 1869. The spectroscope is attached to the eye-end of the 8-inch refractor, with which the earliest work on the spectra of stars was done; during the first few years in conjunction with Dr. Miller, and afterwards by myself working alone. The induction coil, and the wires therefrom for the production of chemical spectra for

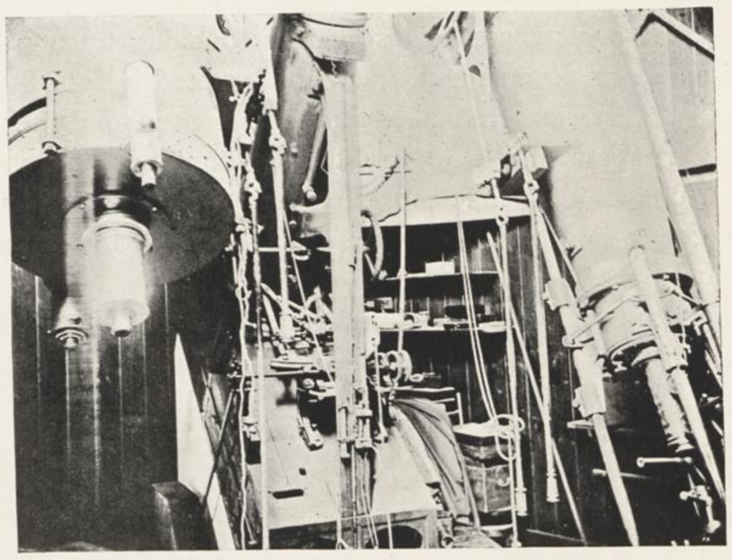


INTERIOR OF THE OBSERVATORY, 1860—1869.

direct comparison in the spectroscope with the celestial spectra, are shown; but the primary battery for exciting the coil, which the artist represents on the floor of the Observatory, was usually placed on a shelf outside one of the windows.

In 1870 the dome of 12 feet diameter was taken down, and a drum having a diameter of 18 feet was erected in its place to receive a larger instrument, placed in my hands as a loan by the Royal Society. This instrument, the work of Sir Howard Grubb, was furnished with two telescopes—an achromatic of 15 inches aperture, and a reflector with metallic specula, of 18 inches aperture, either of which could be mounted on the equatorial stand. Later on, in 1882, by means of a simple device which occurred to me—namely, two independent declination axes, one moving within the other—both telescopes were mounted together, one on each side of the polar axis; and since that time either instrument has been available for use.

Spectroscopes of different constructions have been, from time to time, attached to the two telescopes, which have been described in the Papers on



18-INCH CASSEGRAIN AND 15-INCH REFRACTOR MOUNTED TOGETHER.

the work done with them. The particular spectroscope with which the photographs reproduced in this volume were taken is fully described further on.

An illustration has been placed at the end of this chapter which gives an outside view of the Observatory in its present state, as it has been since the replacement of the original 12-foot dome by a drum, 18 feet in diameter, in 1870.

For the purpose of a freer horizon the Observatory was built upon iron pillars, so that the floor is raised 16 feet above the ground. A solid pier of brick and masonry passes up through the floor for the support of the telescope. In the space below, are two rooms for laboratory and photographic work.

In 1897, at the request of the editor, I wrote an article, with the title "The New Astronomy," which appeared in the June number of the *Nineteenth Century* for that year, and gives in outline a personal retrospect of the early work of the Observatory so far as it contributed to the laying of the foundations of the New Astronomy.

By the kind permission of the Editor, I am able to reprint here, with some omissions, those parts of the article which more directly concern the Observatory and its work:

In 1856 I built a convenient Observatory, opening up a passage from the house, and raised so as to command an uninterrupted view of the sky except on the north side.* It consisted of a dome 12 feet in diameter and a transit room. There was erected in it an equatorially mounted telescope by Dollond of 5 inches aperture, at that time looked upon as a large rather than a small instrument. I commenced work on the usual lines, taking transits, observing and making drawings of planets.

About that time Mr. Alvan Clark, the founder of the American firm famous for the construction of the great object-glasses of the Lick and the Yerkes Observatories, then a portrait-painter by profession, began, as an amateur, to make object-glasses of large size for that time, and of very great merit. Specimens of his earliest work came into the hands of my friend Mr. Dawes, and received the high approval of that distinguished judge. In 1858 I purchased from Mr. Dawes an object-glass by Alvan Clark of 8 inches diameter, which he parted with to make room for a lens of a larger diameter by $\frac{1}{4}$ inch, which Mr. Clark had undertaken to make for him. This telescope was mounted for me equatorially, and provided with a clock-motion, by Mr. Cooke of York.

I soon became a little dissatisfied with the routine character of ordinary astronomical work, and in a vague way sought about in my mind for the possibility of research upon the heavens in a new direction, or by new methods. It was just at this time, when a vague longing after newer methods of observation for attacking many of the problems of the heavenly bodies filled my mind, that the news reached me of Kirchhoff's great discovery of the true nature and the chemical constitution of the sun from his interpretation of the Fraunhofer lines.

Here at last presented itself the very order of work for which in an

* Description of Observatory, *Month. Not. R. Ast. Soc.*, vol. xvi. p. 175.

indefinite way I was looking—namely, to extend his novel methods of research upon the sun to the other heavenly bodies. This was especially work for which I was to a great extent prepared, from being already familiar with the chief methods of chemical and physical research.

It was just at this time that I happened to meet at a *soirée* of the Pharmaceutical Society, where spectroscopes were shown, my friend and neighbour, Dr. W. Allen Miller, Professor of Chemistry at King's College, who had already worked much on chemical spectroscopy. On our way home I told him of what was in my mind, and asked him to join me in the attempt I was about to make to apply Kirchhoff's methods to the stars. At first, from considerations of the great relative faintness of the stars, and from the great delicacy of the work from the earth's motion, even with the aid of clockwork, he hesitated as to the probability of our success. Finally, he agreed to come to my observatory, on the first fine evening, for some preliminary experiments as to what we might expect to do upon the stars.

Let us look at the problem which lay before us. It is difficult for any one, who has now only to give an order for a star spectroscope, to understand in any true degree the difficulties which we met with in attempting to make such observations for the first time. From the sun, with which the Heidelberg professors had to do—which, even bright as it is, for some parts of the spectrum has no light to spare—to the brightest stars is a very far cry. The light received at the earth from a first-magnitude star, as Vega, is only about the $\frac{1}{40,000,000,000}$ part of that received from the sun.

Fortunately, as the stars are too far off to show a true disc, it is possible to concentrate all the light received from a star upon a large mirror or object-glass into the telescopic image, and so increase its brightness.

We could not make use of the easy method adopted by Fraunhofer of placing a prism before the object-glass, for we needed a terrestrial spectrum, taken under the same conditions, for the interpretation, by a simultaneous comparison with it, of the star's spectrum. Kirchhoff's method required that the image of a star should be thrown upon a narrow slit simultaneously with the light from a flame or from an electric spark.

These conditions made it necessary to attach a spectroscope to the eye-end of the telescope, so that it would be carried with it, with its slit in the focal plane. Then, by means of a small reflecting prism placed before one-half of the slit, light from a terrestrial source at the side of the telescope could be sent into the instrument together with the star's light, and so form a spectrum by the side of the stellar spectrum, for convenient comparison with it.

This was not all. As the telescopic image of a star is a point, its spectrum will be a narrow line of light without appreciable breadth. Now, for the observation either of dark or of bright lines across the spectrum a certain breadth is absolutely needful. To get breadth, the pointlike image of the star must be broadened out. As light is of first importance, it was desirable to broaden the star's image only in the one direction necessary to give breadth to the spectrum; or, in other words, to convert the stellar point into a short line of light. Such an enlargement in one direction could be given by the device, first employed by Fraunhofer himself, of a lens convex or concave in one direction only, and flat, and so having no action on the light in a direction at right angles to the former one. By means of such a lens, placed within the focus of the telescope in front of the slit, the pointlike image of a star could be widened in one direction so as to become a very fine line of light, just so long as, but no longer than, was necessary to give to the spectrum a breadth sufficient for distinguishing any lines by which it may be crossed.

It is scarcely possible at the present day, when all these points are as familiar as household words, for any astronomer to realise the large amount of time and labour which had to be devoted to the successful construction of the first star-spectroscope. Especially was it difficult to provide for the satisfactory introduction of the light for the comparison spectrum. We soon found, to our dismay, how easily the comparison lines might become instrumentally shifted, and so be no longer strictly fiducial. As a test we used the solar lines as reflected to us from the moon—a test of more than sufficient delicacy with the resolving power at our command.

Then it was that an astronomical observatory began, for the first time, to take on the appearance of a laboratory. Primary batteries, giving forth noxious gases, were arranged outside one of the windows; a large induction coil stood mounted on a stand on wheels, so as to follow the positions of the eye-end of the telescope, together with a battery of several Leyden jars; shelves with Bunsen burners, vacuum tubes, and bottles of chemicals, especially of specimens of pure metals, lined its walls.

The observatory became a meeting-place where terrestrial chemistry was brought into direct touch with celestial chemistry. The characteristic light-rays from earthly hydrogen shone side by side with the corresponding radiations from starry hydrogen, or else fell upon the dark lines due to the absorption of hydrogen in Sirius or in Vega. Iron from our mines was line-matched, light for dark, with stellar iron from opposite parts of the celestial sphere. Sodium, which upon the earth is always present with us, was found to be widely diffused through the celestial spaces.

The time was, indeed, one of strained expectation and of scientific exaltation for the astronomer, almost without parallel; for nearly every observation revealed a new fact, and almost every night's work was red-lettered by some discovery. And yet, notwithstanding, we had to record "that the inquiry in which we had been engaged has been more than usually toilsome; indeed, it has demanded a sacrifice of time very great when compared with the amount of information which we have been able to obtain."

Soon after the close of 1862 we sent a preliminary note to the Royal Society "On the Lines of some of the Fixed Stars,"* in which we gave diagrams of the spectra of Sirius, Betelgeux, and Aldebaran, with the statement that we had observed the spectra of some forty stars, and also the spectra of the planets Jupiter and Mars. It was a little remarkable that on the same day on which our paper was to be read, but some little time after it had been sent in, news arrived there from America that similar observations on some of the stars had been made by Mr. Rutherford. A very little later similar work on the spectra of the stars was undertaken in Rome by Secchi, and in Germany by Vogel.

In February 1863 the strictly astronomical character of the Observatory was further encroached upon by the erection, in one corner, of a small photographic tent, furnished with baths and other appliances for the wet collodion process. We obtained photographs, indeed, of the spectra of Sirius and Capella; but from want of steadiness and more perfect adjustment of the instruments, the spectra, though defined at the edges, did not show the dark lines, as we expected. The dry collodion plates then available were not rapid enough; and the wet process was so inconvenient for long exposures, from irregular drying and draining back from the positions in which the plates had often to be put, that we did not persevere in our attempts to photograph the stellar spectra. I resumed them with success in 1875, as we shall see further on.

At that time no convenient maps of the spectra of the chemical elements, which were then but imperfectly known, were available for comparison with the spectra of the stars. Kirchhoff's maps were confined to a few elements, and were laid down on an arbitrary scale, relatively to the solar spectrum, It was not always easy, since our work had to be done at night, when the solar spectrum could not be seen, to recognise with certainty even the lines included in Kirchhoff's maps. To meet this want, I devoted a great part of 1863 to mapping, with a train of six prisms, the spectra of twenty-six of the elements; using as a standard scale the spark-spectrum of common air, which would be always at hand. The lines of air were first carefully referred to those of purified oxygen and nitrogen. The spectra were obtained by the discharge of a large induction coil furnished with a condenser of several Leyden jars. I was much assisted by specimens of pure metals furnished to me by Dr. W. A. Miller and Dr. Matthiessen. My paper on

* *Roy. Soc. Proc.*, xii., pp. 444, 445.

this subject, and its accompanying maps, appeared in the volume of the Transactions of the Royal Society for 1864.*

During the same time, whenever the nights were fine, our work on the spectra of the stars went on, and the results were communicated to the Royal Society in April 1864 †; after which Dr. Miller had not sufficient leisure to continue working with me. The general accuracy of our work, so far as it was possible with the instruments at our disposal, is shown by the good agreement of the spectra of Aldebaran and Betelgeux with the observations of the same stars made later in Germany by Vogel.

It is obviously unsafe to claim for spectrum comparisons a greater degree of accuracy than is justified by the resolving power employed. When the apparent coincidences of the lines of the same substances are numerous, as in the case of iron; or the lines are characteristically grouped, as are those of hydrogen, of sodium, and of magnesium, there is no room for doubt that the same substances are really in the stars. Coincidence with a single line may be little better than trusting to a bruised reed; for the stellar line may, under greater resolving power, break up into two or more lines, and then the coincidence may disappear. As we shall see presently, the apparent position of the star-line may not be its true one, in consequence of the earth's, or the star's motion, in the line of sight. Our work, however, was amply sufficient to give a certain reply to the wonder that had so long asked in vain of what the stars were made. The chemistry of the solar system was shown to prevail, essentially at least, wherever a star twinkles. The stars were undoubtedly suns after the order of our sun, though not all at the same evolutional stage, older or younger it may be, in the life-history of bodies of which the vitality is heat. Further, elements which play a chief *rôle* in terrestrial physics, as iron, hydrogen, sodium, magnesium, calcium, were found to be the first and the most easily recognised of the earthly substances in the stars.

Soon after the completion of the joint work of Dr. Miller and myself, and then working alone, I was fortunate in the early autumn of the same year (1864) to begin some observations in a region hitherto unexplored, and which, to this day, remain associated in my memory with the profound awe which I felt on looking for the first time at that which no eye of man had seen, and which even the scientific imagination could not foreshow.

The attempt seemed almost hopeless; for not only are the nebulæ very faintly luminous—as Marius put it, "like a rushlight shining through a horn"—but their feeble shining cannot be increased in brightness, as can be

* *Phil. Trans.*, cliv., 1864, pp. 139-60.

† *Ibid.*, pp. 413-35.

that of the stars, neither to the eye nor in the spectroscope, by any optic tube, however great.

The nature of these mysterious bodies was still an unread riddle. Towards the end of the last century the elder Herschel, from his observations at Slough, came very near suggesting what is doubtless the true nature, and place in the Cosmos, of the nebulæ. I will let him speak in his own words :—

> A shining fluid of a nature unknown to us.
>
> What a field of novelty is here opened to our conceptions! . . . We may now explain that very extensive nebulosity, expanded over more than sixty degrees of the heavens, about the constellation of Orion: a luminous matter accounting much better for it than clustering stars at a distance. . . .
>
> If this matter is self-luminous, it seems more fit to produce a star by its condensation than to depend on the star for its existence.

This view of the nebulæ as parts of a fiery mist out of which the heavens had been slowly fashioned, began, a little before the middle of the present century, at least in many minds, to give way before the revelations of the giant telescopes which had come into use, and especially of the telescope, 6 feet in diameter, constructed by the late Earl of Rosse.

Nebula after nebula yielded, being resolved apparently into innumerable stars, as the optical power was increased; and so the opinion began to gain ground that all nebulæ may be capable of resolution into stars. According to this view, nebulæ would have to be regarded, not as early stages of an evolutional progress but rather as stellar galaxies already formed, external to our system—cosmical "sandheaps" too remote to be separated into their component stars. Lord Rosse himself was careful to point out that it would be unsafe from his observations to conclude that all nebulosity is but the glare of stars too remote to be resolved by our instruments. In 1858 Herbert Spencer showed clearly that, notwithstanding the Parsonstown revelations, the evidence from the observation of nebulæ up to that time was really in favour of their being early stages of an evolutional progression.

On the evening of August 29th, 1864, I directed the telescope for the first time to a planetary nebula in Draco.

I looked into the spectroscope. No spectrum such as I expected! A single bright line only! At first I suspected some displacement of the prism, and that I was looking at a reflection of the illuminated slit from one of its faces. This thought was scarcely more than momentary; then the true interpretation flashed upon me. The light of the nebula was monochromatic; and so, unlike any other light I had as yet subjected to prismatic examination, could not be extended out to form a complete spectrum. After passing through the two prisms it remained concentrated into a single bright line, having a width corresponding to the width of the slit, and occupying in the instrument a position at that part of the spectrum to which its light belongs in refrangibility. A little closer looking showed two other bright lines on the side towards the blue, all the three lines being separated by intervals relatively dark.*

The riddle of the nebulæ was solved. The answer, which had come to us in the light itself, read: Not an aggregation of stars, but a luminous gas. Stars after the order of our own sun, and of the brighter stars, would give a different spectrum; the light of this nebula had clearly been emitted by a luminous gas. With an excess of caution, at the moment I did not venture to go further than to point out that we had here to do with bodies of an order quite different from that of the stars. Further observation soon convinced me that, though the short span of human life is far too minute relatively to cosmical events for us to expect to see in succession any distinct steps of so august a process, the probability is indeed overwhelming in favour of an evolution in the past, and still going on, of the heavenly hosts. A time surely existed when the matter now condensed into the sun and planets filled the whole space occupied by the solar system, in the condition of gas, which then appeared as a glowing nebula, after the order, it may be, of some now existing in the heavens. There remained no room for doubt that the nebulæ, which our telescopes reveal to us, are the early stages of long processions of cosmical events, which correspond broadly to those required by the nebular hypothesis in one or other of its forms.

* * * * * *

It is necessary to bear distinctly in mind that the old view which made the matter of the nebulæ to consist of an original fiery mist—in the words of the poet:

> . . . a tumultuous cloud
> Instinct with fire and nitre—

could no longer hold its place after Helmholtz had shown, in 1854, that such an originally fiery condition of the nebulous stuff was quite unnecessary, since in the mutual gravitation of widely separated matter we have a store of potential energy sufficient to generate the high temperature of the sun and stars.

The solution of the primary riddle of the nebulæ left pending some secondary questions. What chemical substances are represented by the newly found bright lines? Is solar matter common to the nebulæ as well as to the stars? What are the physical conditions of the nebulous matter?

Further observations showed two lines of hydrogen; and recent observa-

* *Phil. Trans.*, cliv., pp. 437-44.

tions have shown associated with it the new element recently discovered by Professor Ramsay, occluded in certain minerals, and of which a brilliant yellow line in the sun had long been looked upon as the badge of an element as yet unknown. The principal line of these nebulæ suggests probably another substance which has not yet been unearthed from its hiding-place in terrestrial rocks by the cunning of the chemist.

Are the nebulæ very hot, or comparatively cool? The spectroscope indicates a high temperature: that is to say, that the individual molecules or atoms, which by their encounters are luminous, have motions corresponding to a very high temperature, and in this sense are very hot. On account of the great extent of the nebulæ, however, a comparatively small number of luminous molecules might be sufficient to make them as bright as they appear to us. Taking this view, their mean temperature, if they can be said to have one, might be low, and so correspond with what we might expect to find in gaseous masses at an early stage of condensation.

In the nebulæ I had as yet examined, the condensation of nearly all the light into a few bright lines made the observations of their spectra less difficult than I feared would be the case. It became, indeed, a case of "Eyes and No Eyes" when a few days later I turned the telescope to the Great Nebula in Andromeda. Its light was distributed throughout the spectrum, and consequently extremely faint. The brighter middle part only could be seen; though I have since proved, as I at first suggested might be the case, that the blue and the red ends are really not absent, but are not seen on account of their feebler effect upon the eye. Though continuous, the spectrum did not look uniform in brightness, but its extreme feebleness made it uncertain whether the irregularities were due to certain parts being enhanced by bright lines, or the other parts enfeebled by dark lines.

Out of sixty of the brighter nebulæ and clusters, I found about one-third, including the planetary nebulæ and that of Orion, to give the bright-line spectrum. It would be altogether out of place here to follow the results of my further observations along the same lines of research which occupied the two years immediately succeeding.

I pass at once to a primary spectroscopic observation of one of those rare and strange sights of the heavens, of which only about nineteen have been recorded in as many centuries:

> . . . those far stars that come in sight
> Once in a century.

On May 18th, 1866, at 5 p.m., a letter came with the address Tuam, from an unknown correspondent, one John Birmingham. Mr. Birmingham afterwards became well known by his observations of variable stars, and especially by his valuable catalogue of Red Stars in 1877. The letter ran:—

I beg to direct your attention to a new star which I observed last Saturday night, and which must be a most interesting object for spectrum analysis. It is situated in Cor. Bor., and is very brilliant, of about the second magnitude. I sent an account of it to the *Times* yesterday, but as that journal is not likely to publish communications from this part of the world, I scarcely think that it will find a place for mine.

Fortunately, the evening was fine, and as soon as it was dusk I looked, with not a little scepticism, I freely confess, at the place of the sky named in the letter. To my great joy, there shone a bright new star, giving a new aspect to the Northern Crown; of the order doubtless of the splendid temporary star of 1572, which Tycho supposed to be generated from the ethereal substance of the Milky Way, and afterwards dissipated by the sun, or dissolved from some internal cause.

I sent a messenger for my friend Dr. Miller; and an hour later we directed the telescope, with spectroscope attached, to the blazing star. Later in the evening a letter arrived from Mr. Baxendale, who had independently discovered the star on the 15th.

By this evening, the 18th, the star had already fallen in brightness below the third magnitude. The view in the spectroscope was strange, and up to that time unprecedented. Upon a spectrum of the solar order, with its numberless dark lines, shone out brilliantly a few very bright lines. There was little doubt that at least two of these lines belonged to hydrogen. The great brilliancy of these lines, as compared with the parts of the continuous spectrum upon which they fell, suggested a temperature for the gas emitting them higher than that of the star's photosphere.

Few of days, as indeed had been its forbears appearing at long intervals, the new star waned with a rapidity little less remarkable than was the suddenness of its outburst, without visible descent, all armed in a full panoply of light from the moment of its birth. A few hours only before Birmingham saw it blazing with second-magnitude splendour, Schmidt, observing at Athens, could testify that no outburst had taken place. Rapid was the decline of its light, falling in twelve days from the second down to the eighth magnitude.

It was obvious to us that no very considerable mass of matter could cool down from the high temperature indicated by the bright lines in so short a time. At the same time, it was not less clear that the extent of the mass of the fervid gas must be on a very grand scale indeed for a star at its undoubted distance from us to take on so great a splendour. These considerations led us to suggest some sudden and vast convulsion which had taken place in a star so far cooled down as to give but little light, or even to be partially crusted over; by volcanic forces, or by the disturbing approach or partial collision of another dark star. The essential character of the explanation lay in the suggestion of a possible chemical combination of some of the escaping highly heated gases from within, when cooled by the sudden expansion, which might give rise to an outburst of flame at once very brilliant and of very short duration.*

The more precise statement of what occurred during our observations, as made afterwards from the pulpit of one of our cathedrals—"that from afar astronomers had seen a world on fire go out in smoke and ashes"—must be put down to an excess of the theological imagination.

From the beginning of our work upon the spectra of the stars, I saw in vision the application of the new knowledge to the creation of a great method of astronomical observation which could not fail in future to have a powerful influence on the progress of astronomy—indeed, in some respects greater than the more direct one of the investigation of the chemical nature and the relative physical conditions of the stars.

It was the opprobrium of the older astronomy—though, indeed, one which involved no disgrace, for *à l'impossible nul n'est tenu*—that only that part of the motions of the stars which is across the line of sight could be seen and directly measured. The direct observation of the other component in the line of sight, since it caused no change of place and, from the great distance of the stars, no appreciable change of size or of brightness within an observer's lifetime, seemed to lie hopelessly quite outside the limits of man's powers. Still, it was only too clear that, so long as we were unable to ascertain directly those components of the stars' motions which lie in the line of sight, the speed and direction of the solar motion in space, and many of the great problems of the constitution of the heavens, must remain more or less imperfectly known.

Now, as the colour of a given kind of light, and the exact position it would take up in a spectrum, depends directly upon the length of the waves, or, to put it differently, upon the number of waves which would pass into the eye in a second of time, it seemed more than probable that motion between the source of the light and the observer must change the apparent length of the waves to him, and the number reaching his eye in a second. To a swimmer striking out from the shore each wave is shorter, and the number he goes through in a given time is greater, than would be the case if he had stood still in the water. Such a change of wave-length would transform any given kind of light, so that it would take a new place in the spectrum, and from the amount of this change to a higher or to a lower

* *Roy. Soc. Proc.*, xv., 1866, p. 146.

place we could determine the velocity per second of the relative motion between the star and the earth.

The notion that the propagation of light is not instantaneous, though rapid far beyond the appreciation of our senses, is due, not, as is sometimes stated, to Francis, but to Roger Bacon : "Relinquitur ergo," he says, in his *Opus Majus*, "quod lux multiplicatur in tempore . . . sed tamen non in tempore sensibili et perceptibili a visu, sed insensibili. . . ." The discovery of its actual velocity was made by Roemer, in 1675, from observations of the satellites of Jupiter. Now, though the effect of motion in the line of sight upon the apparent velocity of light underlies Roemer's determinations, the idea of a change of colour in light from motion between the source of light and the observer was announced for the first time by Doppler in 1841. Later, various experiments were made in connection with this view by Ballot, Sestini, Klinkerfues, Clerk Maxwell, and Fizeau. But no attempts had been made, nor were indeed possible, to discover by this principle the motions of the heavenly bodies in the line of sight. For, to learn whether any change in the light had taken place from motion in the line of sight, it was clearly necessary to know the original wave-length of the light before it left the star.

As soon as our observations had shown that certain earthly substances were present in the stars, the original wave-lengths of their lines became known, and any small want of coincidence of the stellar lines with the same lines produced upon the earth might safely be interpreted as revealing the velocity of approach or of recession between the star and the earth.

It was not until 1866 that I found time to construct a spectroscope of greater power for this research. It would be scarcely possible, even with greater space, to convey to the reader any true conception of the difficulties which presented themselves in this work, from various instrumental causes, and of the extreme care and caution which were needful to distinguish spurious instrumental shifts of a line from a true shift due to the star's motion.

At last, in 1868, I felt able to announce, in a paper printed in the "Transactions of the Royal Society" for that year, the foundation of this new method of research, which, transcending the wildest dreams of an earlier time, enables the astronomer to measure off directly in terrestrial units the invisible motions in the line of sight of the heavenly bodies.*

To pure astronomers the method came before its time, since they were then unfamiliar with spectrum analysis, which lay completely outside the routine work of an observatory. It would be easy to mention the names of men well known to whom I was "as a very lovely song of one that hath a pleasant voice." They heard my words, but for a time they were very slow to avail themselves of this new power of research. My observations

* *Phil. Trans.*, clviii., 1868, pp. 529-50.

were, however, shortly afterwards confirmed by Vogel in Germany; and by others the principle was soon applied to solar phenomena. By making use of improved methods of photography, Vogel has recently determined the motions of approach and of recession of some fifty stars with an accuracy of about an English mile a second. In the hands of Young, Dunèr, Keeler, and others, the method has been successfully applied to a determination of the rotation of the sun, of Saturn and his rings, and of Jupiter.

It has become fruitful in another direction, for it puts into our hands the power of separating double stars which are beyond the resolving power of any telescope that can ever be constructed. Pickering and Vogel have independently discovered by this method an entirely new class of double stars,

Double stars too close to be separately visible unite in giving a compound spectrum. Now, if the stars are in motion about a common centre of gravity, the lines of one star will shift periodically relatively to similar lines of the other star in the spectrum common to both; and such lines will consequently, at those times, appear double. Even if one of the stars is too dark to give a spectrum which can be seen upon that of the other star, as is actually the case with Algol and Spica, the whirling of the stars about each other may be discovered from the periodical shifting of the lines of the brighter star relatively to terrestrial lines of the same substance. It is clear that, as the stars revolve about their common centre of gravity, the bright star would be sometimes advancing, and at others receding, relatively to an observer on the earth, except it should happen that the stars' orbit were perpendicular to the line of sight.

It would be scarcely possible, without the appearance of great exaggeration, to attempt to sketch out even in broad outline the many glorious achievements which doubtless lie before this method of research in the immediate future.

Comets in the olden time were looked upon as the portents of all kinds of woe:

> There with long bloody haire, a blazing star
> Threatens the World with Famin, Plague, and War.

Though they were no longer, at the time of which I am speaking, a terror to mankind, they were a great mystery. Perhaps of no other phenomenon of nature had so many guesses at truth been made on different, and even on opposing, principles of explanation. It was about this time that a beam of light was thrown in, for the first time, upon the night of mystery in which they moved and had their being, by the researches of Newton of Yale College, by Adams, and by Schiaparelli. The unexpected fact came out of the close relationship of the orbits of certain comets with those of periodic meteor-swarms. Only a year before the observations of which I am about to speak were made, Odling had lighted up the theatre of the Royal Institution with gas brought by a meteorite from celestial space. Two years earlier, Donati showed the light of a small comet to be in part self-emitted, and so not wholly reflected sunshine.

I had myself, in the case of three faint comets, in 1866, in 1867, and January 1868, discovered that part of their light was peculiar to them, and that the light of the last one consisted mainly of three bright flutings. Intense, therefore, was the great expectancy with which I directed the telescope with its attached spectroscope to the much brighter comet which appeared in June 1868.

The comet's light was resolved into a spectrum of three bright bands or flutings, each alike falling off in brightness on the more refrangible side. On the evening of the 22nd I measured the positions in the spectrum of the brighter beginnings of the flutings on the red side. I was not a little surprised the next morning to find that the three cometary flutings agreed in position with three similar flutings in the brightest part of the spectrum of carbon. Some time before, I had mapped down the spectrum of carbon from different sources, chiefly from different hydrocarbons. In some of these spectra the separate lines of which the flutings are built up are individually more distinct than in others. The comet bands, as I had seen them on the previous evening, appeared to be identical in character in this respect, as well as in position in the spectrum, with the flutings as they appeared when I took the spark in a current of olefiant gas. I immediately filled a small holder with this gas, arranged an apparatus in such a manner that the gas could be attached to the end of the telescope, and its spectrum, when a spark was taken in it, seen side by side with that of the comet.

Fortunately, the evening was fine; and, on account of the exceptional interest of confronting for the first time the spectrum of an earthly gas with that of a comet's light, I invited Dr. Miller to come and make the crucial observation with me. The expectation which I had formed from my measures was fully confirmed. The comet's spectrum, when seen together with that from the gas, agreed in all respects precisely with it. The comet, though "subtle as Sphinx," had at last yielded up its secret. The principal part of its light was emitted by luminous vapour of carbon.*

This result was in harmony with the nature of the gas found occluded in meteorites. Odling had found carbonic oxide as well as hydrogen in his meteorite. Wright, experimenting with another type of meteorite, found that carbon dioxide was chiefly given off. Many meteorites contain a large percentage of hydrocarbons; from one of such sky-stones a little later I

* *Phil. Trans.*, clviii., 1868, pp. 555-64.

observed a spectrum similar to that of the comet. The three bands may be seen in the base of a candle flame.

Since these early observations the spectra of many comets have been examined by many observers. The close general agreement as to the three bright flutings which form the main feature of the cometary spectrum, confirms beyond doubt the view that the greater part of the light of comets is due to the fluted spectrum of carbon. Some additional knowledge of the spectra of comets, obtained by means of photography, will have its proper place later on.

About this time I devoted some attention to spectroscopic observations of the sun, and especially to the modifications of the spectrum which take place under the influence of the solar spots.

The aerial ocean around and above us, in which finely divided matter is always more or less floating, becomes itself illuminated, and a source of light, when the sun shines upon it, and so conceals, like a luminous veil, any object less brilliant than itself in the heavens beyond. From this cause the stars are invisible at midday. This curtain of light above us, at all ordinary times, shuts out from our view the magnificent spectacle of red flames flashing upon a coronal glory of bright beams and streamers, which suddenly bursts upon the sight, for a few minutes only, when at rare intervals the light-curtain is lifted by the screening of the sun's light by the moon, at a total eclipse.

As yet the spectrum of the red flames had not been seen. If, as seemed probable, it should be found to be that of a gas, consisting of bright lines only, it was conceivable that the spectroscope might enable us so to weaken by dispersion the air-glare, relatively to the bright lines which would remain undispersed, that the bright lines of the flames might become visible through the atmospheric glare.

The historic sequence of events is as follows :—In November 1866 Mr. Lockyer asked the question : "May not the spectroscope afford us evidence of the existence of the red flames, which total eclipses have revealed to us in the sun's atmosphere, though they escape all other methods of observation at other times?"

In the Report of the Council of the Royal Astronomical Society, read in February 1868, occurs the following statement, furnished by me, in which the explanation is fully given of the principle on which I had been working to obtain the spectrum of the red flames without an eclipse :—

During the last two years Mr. Huggins has made numerous observations for the purpose of obtaining a view, if possible, of the red prominences seen during an eclipse. The invisibility of these objects at ordinary times is supposed to arise from the illumination of our atmosphere. If these bodies are gaseous, their spectra would consist of bright lines. With a powerful spectroscope the light reflected from our atmosphere near the sun's limb edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy. This principle has been carried out by various forms of prismatic apparatus, and also by other contrivances, but hitherto without success.*

At the total eclipse of the sun, August 18th, 1868, several observers saw the light of the red flames to be resolved in their spectroscopes into bright lines, among which lines of hydrogen were recognised. The distinguished astronomer, Janssen, one of the observers in India, saw some of the bright lines again the next day, by means of the principle described above, when there was no eclipse.

On October 29th Mr. Lockyer sent a note to the Royal Society to say that on that day he had succeeded in observing three bright lines, of a fine prominence.

About the time that the news of the discovery of the bright lines at the eclipse reached this country, in September, I was altogether incapacitated for work for some little time through the death of my beloved mother. We had been all in all to each other for many years. The first day I was sufficiently recovered to resume work (December 19th), on looking at the sun's limb with the same spectroscope I had often used before, now that I knew exactly at what part of the spectrum to search for the lines, I saw them at the first moment of putting my eye to the instrument.

As yet, by all observers the lines only of the prominences had been seen, and therefore, to learn their forms, it was necessary to combine in one design the lengths of the lines as they varied, when the slit was made to pass over a prominence. In February of the following year it occurred to me that, by widening the opening of the slit, the form of a prominence, and not its lines only, might be directly observed.† This method of using a wide slit has been since universally employed.

It does not fall within the scope of this article to describe an ingenious photographic method by which Hale has been able to take daily records of the constantly varying phenomena of the red flames and the bright faculæ, upon and around the solar disk.

The purpose of this article is to sketch, in very broad outline only, the principal events, in the order of their succession in time, *quorum pars magna fui*, which contributed in an important degree to the rise of the new astronomy. As a science advances it follows naturally that its further progress will consist more and more in matters of detail, and in points which are of technical rather than of general interest.

It would, therefore, be altogether out of place here to carry on in detail the narrative of the work of my observatory, when, as was inevitable, it began to take on the character of a development only, along lines of which I have

* *Month. Not. R. Ast. Soc.*, vol. xxviii., p. 88.

† *Roy. Soc. Proc.*, vol. xvii., 1849, p. 302.

already spoken: namely, the observation of more stars, and of other nebulæ, and other comets. I pass on at once, therefore, to the year 1876, in which, by the aid of the new dry plates, with gelatine films, introduced by Mr. Kennett, I was able to take up again, and this time with success, the photography of the spectra of the stars, of my early attempts at which I have already spoken.

I was now better prepared for work. My observatory had been enlarged from a dome of 12 feet in diameter to a drum having a diameter of 18 feet. This alteration had been made for the reception of a larger telescope made by Sir Howard Grubb, at the expense of a legacy to the Royal Society, and which was placed in my hands on loan by that Society. This instrument was furnished with two telescopes: an achromatic of 15 inches aperture and a Cassegrain of 18 inches aperture, with mirrors of speculum metal. At this time, one only of these telescopes could be in use at a time. Later on, in 1882, by a device which occurred to me, of giving each telescope an independent polar axis, the one working within the other, both telescopes could remain together on the equatorial mounting, and be equally ready for use.

By this time I had the great happiness of having secured an able and enthusiastic assistant, by my marriage in 1875.

The great and notable advances in astronomical methods and discoveries by means of photography, since 1875, are due almost entirely to the great advantages which the gelatine dry plate possesses for use in the observatory over the process of Daguerre, and even over that of wet collodion. The silver-bromide gelatine plate, which I was the first, I believe, to use for photographing the spectra of stars, except for its grained texture, meets the need of the astronomer at all points. This plate possesses extreme sensitiveness; it is always ready for use; it can be placed in any position; it can be exposed for hours; lastly, immediate development is not necessary, and for this reason, as I soon found to be necessary in this climate, it can be exposed again to the same object on succeeding nights, and so make up by successive instalments, as the weather may permit, the total long exposure which may be needful.

The power of the eye falls off as the spectrum extends beyond the blue, and soon fails altogether. There is therefore no drawback to the use of glass for the prisms and lenses of a visual spectroscope. But while the sensitiveness of a photographic plate is not similarly limited, glass, like the eye, is imperfectly transparent, and soon becomes partially opaque, to the parts of the spectrum at a short distance beyond the limit of the visible spectrum. To obtain, therefore, upon the plate a spectrum complete at the blue end of stellar light, it was necessary to avoid glass, and to employ instead Iceland spar and rock crystal, which are transparent up to the limit of the ultra-violet light which can reach us through our atmosphere. Such a spectroscope was constructed and fixed with its slit at the focus of the great speculum of the Cassegrain telescope.

How was the image of a star to be easily brought, and then kept, for an hour, or even for many hours, precisely at one place on a slit so narrow as about one two-hundredth of an inch? For this purpose the very convenient device was adopted of making the slit-plates of highly polished metal, so as to form a divided mirror, in which the reflected image of a star could be observed from the eye-end of the telescope by means of a small telescope fixed within the central hole of the great mirror. A photograph of the spectrum of *a* Lyræ, taken with this instrument, was shown at the Royal Society in 1876.*

In the spectra of such stars as Sirius and Vega, there came out in the ultra-violet region, which up to that time had remained unexplored, the completion of a grand rhythmical group of strong dark lines, of which the well known hydrogen lines in the visible region form the lower members. Terrestrial chemistry became enriched with a more complete knowledge of the spectrum of hydrogen from the stars. Shortly afterwards, Cornu succeeded in photographing a similar spectrum in his laboratory from earthly hydrogen.

I presented in 1879 a paper, with maps, to the Royal Society, on the photographic spectra of the stars, which was printed in their Transactions for 1880.† In this paper, besides descriptions of the photographs and tables of the measures of the positions of the lines, I made a first attempt to arrange the stars in a possible evolutional series, from the relative behaviour of the hydrogen and the metallic lines. In this series, Sirius and Vega are placed at the earlier end; Capella and the sun, at about the same evolutional stage, somewhere in the middle of the series; while at the most advanced and oldest stage of the stars which I had then photographed, came Betelgeux, in the spectrum of which the ultra-violet region, though not wanting, is very greatly enfeebled.

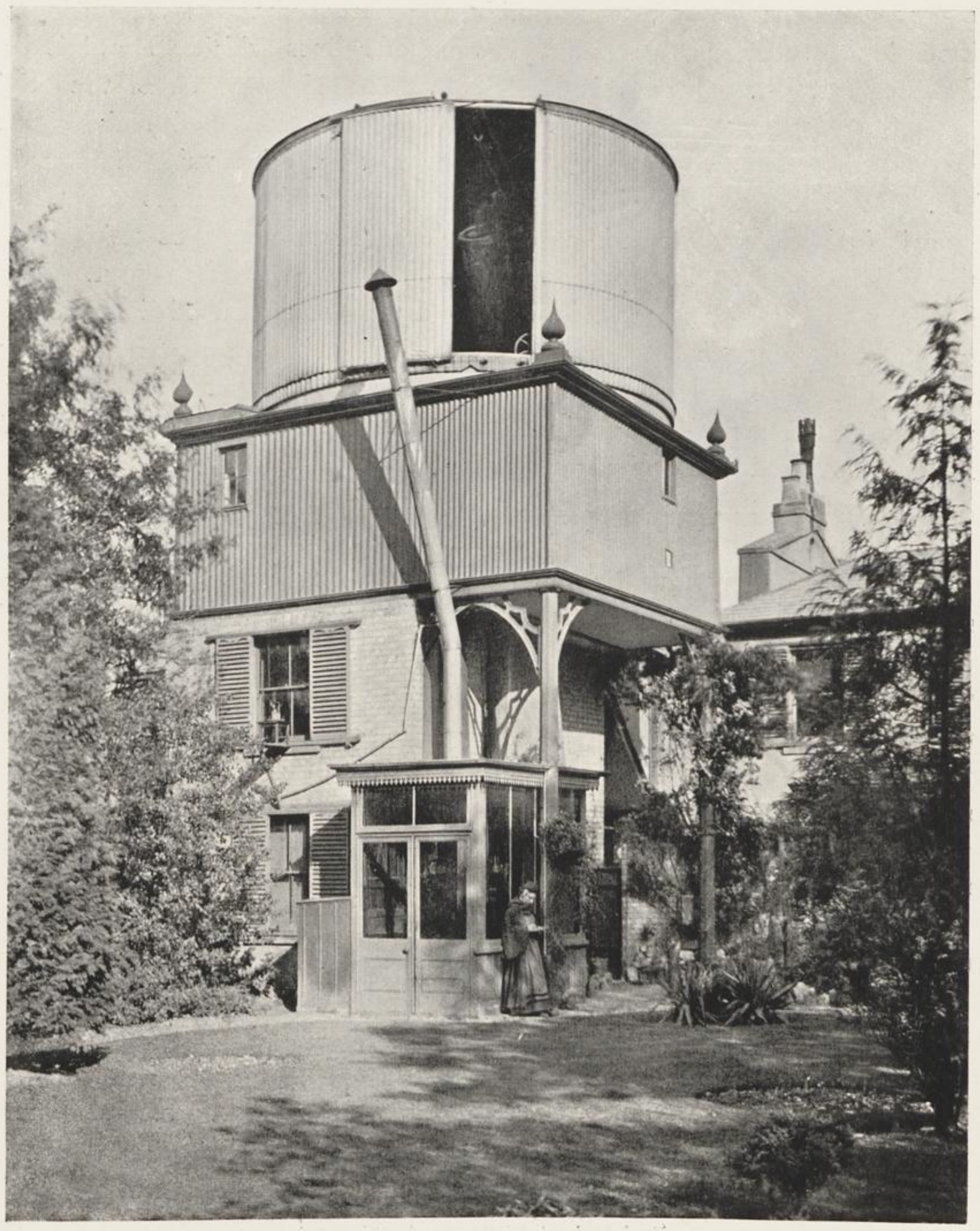
Shortly afterwards, I directed the photographic arrangement of combined spectroscope and telescope to the nebula in Orion, and obtained for the first time information of the nature of its spectrum beyond the visible region. One line a little distance on in the ultra-violet region came out very strongly on the plate. If this kind of light came within the range of our vision, it would no doubt give the dominant colour to the nebula, in place of its present blue-greenish hue. Other lines of the hydrogen series, as might be expected, were seen in the photograph, together with a number of other bright lines.‡

* *Roy. Soc. Proc.*, vol. xxv., 1877, p. 445.

† *Phil. Trans.*, vol. clxxi., 1880, pp. 669–90.

‡ *Roy. Soc. Proc.*, xxxiii., 1882, p. 425; xlvi., 1889, p. 40; xlviii., 1890, p. 213.

In 1881, for the first time since the spectroscope and also suitable photographic plates had been in the hands of astronomers, the coming of a



THE OBSERVATORY. PHOTOGRAPHED FROM THE GARDEN.

bright comet made it possible to extend the examination of its light into the invisible region of the spectrum at the blue end. On the 22nd of June I was able to obtain, with an exposure of one hour, a good photograph of the head of the comet. It was under a great tension of expectancy that the plate was developed, so that I might be able to look for the first time into a virgin region of nature, as yet unexplored by the eye of man.*

The plate contained an extension and confirmation of my earlier observations by eye. There were the combined spectra of two kinds of light—a faint continuous spectrum, crossed by Fraunhofer lines which showed it to be reflected solar light. Upon this was seen a second spectrum of the original light emitted by the comet itself. This spectrum consisted mainly of two groups of bright lines, characteristic of the spectra of certain compounds of carbon. It will be remembered that my earlier observations revealed the three principal flutings of carbon as the main feature of a comet's spectrum in the visible region. The photograph brought a new fact to light. Liveing and Dewar had shown that one of these bands consisted of lines belonging to a nitrogen compound of carbon. We gained the new knowledge that nitrogen, as well as carbon and hydrogen, exists in comets. Now, nitrogen is present in the gas found occluded in some meteorites. At a later date, Dr. Flight showed that nitrogen formed as much as 17 per cent. of the occluded gas from the meteorite of Cranbourne, Australia.

I have now advanced to the extreme limit of time within which the rise of the new astronomy can be regarded as taking place. At this time, in respect of the broad lines of its methods and the wide scope of the directions in which it was already applied, it had become well established. Already it possessed a literature of its own, and many observatories were becoming, in part at least, devoted to its methods.

In my own observatory work has gone on whenever our unfavourable climate has permitted observations to be made. At the present moment more than one research is in progress. It would be altogether beyond the intention and limited scope of the present article to follow this later work.

We found the new astronomy newly born in a laboratory at Heidelberg; to astronomers she was

> . . . a stranger,
> Born out of their dominions.

We take leave of her in the full beauty of a vigorous youth, receiving homage in nearly all the observatories of the world.

* *Roy. Soc. Proc.*, xxxiii., 1882, pp. 1-3.