have taken place near coasts, some species have been gradually carried down to deep water, have accommodated themselves to the new conditions, and have gradually migrated to the regions far from land. A few species may thus have migrated to the deep sea during each geological period. In this way the origin and distribution of the deep-sea fauna in the present oceans may in some measure be explained. In like manner, the pelagic fauna and flora of the ocean is most probably derived originally from the shore and shallow water. During each period of the earth's history a few animals and plants have been carried to sea, and have ultimately adopted a pelagic mode of life.

Without insisting strongly on the correctness of some of these deductions and conclusions, we present them for the consideration of naturalists and geologists, as the result of a long, careful, but as yet incomplete, investigation.

THE FIXED STARS[1]

THERE is no science which has so long and so continuously occupied the thoughtful minds of successive generations of men as has astronomy; and of its various branches there is one which has for all ages possessed a special fascination, viz. that of sidereal astronomy.

There has ever been a desire to burst aside the constraints imposed upon our research by the distances of space, to pass from the study of the planets of our solar system to that of the suns and galaxies that surround us, to determine the position and relative importance of our own system in the scheme of the universe and the whence we have come and the whither we are drifting through the realms of space.

Questions without number crowd upon the mind. The galaxy or Milky Way—what is it? Is our sun one of its members? What is the shape of that galaxy? What are its dimensions? What is the position of our sun in it?

The star-clusters—what are they, these wondrous aggregations where hundreds and even thousands of suns may be seen in the limited field of view of a powerful telescope? Are these clusters galaxies? Have these suns real dimensions comparable with those of our sun, and is it distance alone that renders their light and dimension so insignificant to the naked eye? Or are the real dimensions of the clusters small as compared with our galaxy? Are their component suns but the fragments of some great sun that has been shattered by forces unknown to us, or have they originated from chaotic matter, which, instead of forming one great whirlpool and condensing by vortex action into one great sun, has been disturbed into numerous minor vortices, and so become rolled up into numerous small suns?

The nebulæ—what are they? Are they too condensing into clusters or stars, or will their ghost-like forms remain for ever unchanged amongst the stars? or do they play some part in the scheme of nature of which we have as yet no conception?

These and many others are the questions which press on the ardent mind that contemplates the subject; and there arises the intense desire to answer such questions, and where facts are wanting to supply facts by fancy. The history of deep and profound thought in some of these subjects goes back through 2000 years, but the history of real progress is but as of yesterday. The foundation of sidereal astronomy may be said to have begun with the art of accurate observation. Bradley's meridian observations at Greenwich about 1750, his previous discovery of the aberration of light in 1727, and Herschel's discovery of the binary nature of double stars, his surveys of the heavens, and his catalogues of double stars—these are solid facts, facts that have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They point to us the lesson that "art is long and life is short," that human knowledge, in the slow developing phenomena of sidereal astronomy, must be content to progress by the accumulating labours of successive generations of men, that progress will be measured for generations yet to come more by the amount of honest, well-directed and systematically-discussed observation than by the most brilliant speculation, and that in observation concentrated systematic effort on a special thoughtfully-selected problem will be of more avail than the most brilliant but disconnected work.

I hope that no one present thinks from what I have said that I undervalue the imaginative fervid mind that longs for the truth, and whose fancy delights to speculate on these great subjects. On the contrary, I think and I believe that without that fervid mind, without that longing for the truth, no man is fitted for the work required of him in such a field—for it is such a mind and such desires that alone can sweeten the long watches of the night, and transform such work from drudgery into a noble labour of love.

It is for like reasons that I ask you to leave with me the captivating realms of fancy this evening, and to enter the more substantial realms of fact. And if at any time I should become too technical or dry I beg that you too will remember the noble problems for the solution of which such dry work is undertaken.

We suppose ourselves then face to face with all the problems of sidereal astronomy to which I have hastily referred—the human mind is lost in speculation, and we are anxious to establish a solid groundwork of fact.

Now what in such circumstances would be the instinct of the scientific mind?

The answer is unquestionable—viz. to measure—and no sooner were astronomical instruments made of reasonable exactness than astronomers did begin to measure, and to ask, are the distances of the fixed stars measurable?

I should like to have given a short history of the early attempts of astronomers to measure the distance of a fixed star. I had indeed prepared such an account, but I remembered that there is in this theatre a relentless clock that has curbed the exuberant verbosity of many a lecturer before me, and I found that if the real subject-matter of this evening's lecture were to be reached and dealt with before 10 o'clock, I must pass over this earlier history, instructive and interesting though it is, and come at once to the time when the long baffled labours of astronomers began to be crowned with success.

Perhaps I cannot summarise it better than in the words of Sir John Herschel. In one of his presidential addresses he says:—"The distance of every individual body in the universe from us is necessarily admitted to be finite. But though the distance of each particular star be not in strictness infinite, it is yet a real and immense accession to our knowledge to have *measured* it in any one case. To accomplish this has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct apprehension, and so leading him on in hopeless, endless, and exhausting pursuit."

Those who have read the history of exact astronomy from the days of Flamsteed—*i.e.* from 1689—down to 1832, will understand how exactly these words of Sir John Herschel describe the position of the problem.

But these laborious pursuits, like all honest researches in quest of truth, were not without reward, even though the immediate object in view was not attained. Bradley was rewarded by his great discovery of aberration, and Sir William Herschel by the greatest of his great discoveries, the binary nature of double stars, when engaged in vain attempts to measure the distance of a fixed star. Time forbids that I should tell more of this instructive story—for the story of failure is often fully as instructive as that of success—and I must begin the history of our problem between 1832 and 1842, when success was first attained.

But before I begin it will save both time and circumlocution if I define a word that we must frequently use—viz. the word parallax.

Here on the table is a large ball representing the sun, and here, travelling on a circular railway round the larger ball, is a smaller ball which we shall suppose to represent the earth. The larger ball is suspended from the ceiling by a white string, the small ball is suspended from the same point by a red string. At the far end of the white string you can suppose a star whose true direction is represented by this white string, and whose apparent direction as looked at from the earth is represented by the red string. Now if the star is within a measurable distance, the red string which indicates the star's apparent direction as seen from the earth will always be displaced inwards towards the sun. This displacement is called "parallax." It may be defined as the change in the apparent place of a star produced by viewing it from a point other than that of reference. Our point of reference for stars is the sun, and as we view the stars now from one side of the sun, and six months afterwards from a point on the

[1] Lecture on Friday evening, May 23, at the Royal Institution, "On Recent Researches on the Distances of the Fixed Stars, and on some Future Problems in Sidereal Astronomy," by David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

opposite side of the sun—that is, from two points 186 millions of miles apart—we might expect to find a considerable change in their apparent places.

But previous to 1832 astronomers could not discover with any certainty that such changes were sensible—that, in other words, the red and the white strings met at a point so distant that, as far as they were able to measure, the two strings were practically parallel—or, putting it another way, the stars were so distant that the diameter of the earth's orbit viewed from the nearest star subtended a smaller angle than their instruments could measure. Bradley felt sure that if the star γ Draconis were so near that its parallax amounted to 1″ of arc he would have detected it—that is, if the earth's orbit viewed from γ Draconis measured 2″ in diameter, that is, if it looked as big as a globe one foot in diameter would look if viewed at forty miles distant, he would have detected it. But the real distances of the stars were greater than that.

The time at last arrived when the two great masters of modern practical astronomy, Bessel and Struve, were preparing by elaborate experiment and study for the researches which led to ultimate success. After vain attempts to obtain conclusive results by endeavours to determine the apparent changes in the absolute direction of a star at different seasons of the year, both astronomers had recourse to a method which, originally proposed by Galileo in 1632, was carried out first on a large scale by Sir William Herschel. I shall refer in the first place to the researches of the great Russian astronomer Struve.

Astronomers had sufficiently demonstrated that the distances of the stars were very great, and it was reasonable to argue that as a rule the brighter stars would be those nearest to us. If, therefore, two stars are apparently near each other—the one bright, the other faint—the chances are that in reality they are far apart, though accidentally nearly in a line.

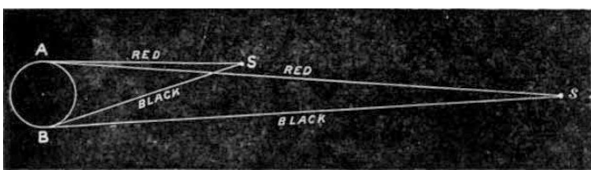


FIG. 1.

If two such stars are represented by S *s* in Diagram I., they would appear near each other viewed from one side of the earth's orbit at A, but not so near each other viewed from B, the opposite side of the earth's orbit, the red lines obviously indicating the apparent angle between the stars when they are viewed from A, and the black lines the apparent angle when they are viewed from B. Struve selected for the star S the bright star Vega (α Lyræ). From its brilliancy he considered it probably one of our nearest neighbours amongst the stars, and a faint star apparently near it seemed to afford a suitable representative of the really distant star *s*. Struve was careful to ascertain that this comparison star was not physically connected with α Lyræ, and he was able to prove this from the fact that whilst α Lyræ has a small annual motion relative to all neighbouring stars, this motion is not shared by the faint comparison star. Struve was provided with a telescope driven by clockwork to follow the diurnal motion of a star, and thus the hands of the observer were free to make the necessary measures. These were accomplished by an instrument such as I hold in my hands applied to the telescope. This micrometer contains two parallel spider webs each attached to a slide, one slide being moved by one screw, the other by the other screw. The screws are provided with drum-heads divided into 100 parts. One web was placed on the image of α Lyræ, the other upon that of the faint comparison star, and the angle between the stars was thus read off in terms of the number of revolutions and decimals of a revolution of the screws. A number of such observations was made on each night, and the result for each night depended on the mean of the numerous observations made each night.

By observations on ninety-six nights between November 1835 and August 1838, he showed that the distance between α Lyræ and the faint comparison star changed systematically with a regular annual period, and that the maxima and minima of those distances corresponded with the times of the year at which these maxima and minima should occur if the brighter star were really much nearer than the fainter one.

Assuming that the fainter star is at a practically immeasurable distance, Struve showed that α Lyræ had a parallax that amounted to about a quarter of a second of arc, which is equivalent to the statement that a globe whose diameter is equal to that of the earth's orbit—that is, to 186,000,000 of miles—would at the distance of α Lyræ present an apparent diameter of half a second of arc. If you wish to realise this angle, place a globe one foot in diameter at a distance of eighty miles, or look at a coin half the diameter of a silver threepenny piece at a distance of one mile from the eye, and try to measure it.

The great German astronomer, Bessel, was simultaneously engaged in like work at Königsberg. He selected as the object of his researches a very remarkable double star—61 Cygni.

This star had already been the subject of similar researches on his part with much inferior means. He now attacked the problem with the splendid heliometer which had been made for him by Frauenhofer for the purpose. The principle of this instrument I shall presently explain. His reasons for choosing 61 Cygni were that the two components of this star, though not remarkable for brightness—they are just visible to the naked eye—yet have this peculiarity, that they have a remarkably large proper motion, the largest then known, though now surpassed by that of two other stars which I shall afterwards mention. They have an apparent angular motion relative to other stars of more than five seconds of arc per annum.

Struve had argued that if the stars were on the average of similar brightness, those stars which were brightest would probably be those nearest to us, and Bessel, in like manner, argued that if the absolute motions of the stars were similar on the average, those motions which appeared the largest belonged to stars which on the average were nearest to us—just as the motion of a snail could be easily watched at the distance of two or three feet from the eye, but could not be detected except after a long interval, if the animal were a good many yards distant.

Bessel employed two faint comparison stars at right angles to each other with respect to 61 Cygni, and he made two separate series of observations, the first extending from August 1837 to October 1838, the second from October 1838 to March 1840.

Both series confirm each other, and the results deduced separately from the measures of the two comparison stars also agree within very narrow limits. From all the observations combined Bessel found the parallax of 61 Cygni to be 35/100 of a second—a quantity which has been shown by the modern researches of Prof. Auwers and Dr. Ball to be more nearly half a second of arc. Thus at 61 Cygni the diameter of the earth's orbit round the sun would appear of the same size as a globe a foot in diameter viewed at forty miles distance, or of a silver threepenny piece a mile off. But whilst these great masters of astronomy—Struve and Bessel—had been exhausting the resources of their skill in observation, and that of the astronomical workshops of Europe in supplying them with the most refined instruments, a quiet and earnest man had been at work at the Cape of Good Hope, and, without knowing it at the time, had *really made the* FIRST *observations* which afforded strong presumptive evidence of the existence of the parallax of any fixed star.

Henderson occupied the post of Her Majesty's Astronomer at the Cape of Good Hope in 1832 and 1833, and during his brief and brilliant tenure of office there he made, amongst many others, a fine series of meridian observations of α Centauri—a bright and otherwise remarkable double star. When, after his return to England, Henderson reduced these observations, and compared them with the earlier observations of other astronomers, he found that α Centauri had a large proper motion; he was therefore led to examine and see whether his observations gave any indication of an annual parallax. He found that they did so, and not of a small parallax but of one amounting to nearly a second of arc. But it was not till this was confirmed, not only by the observations with the mural circle but by those of the transit instrument also, not only by his own observations but by those of Lieut. Meadows, his assistant, that Henderson ventured to publish his remarkable result.

In the year 1842 it was felt by the astronomical world at large that the problem which hitherto had baffled astronomers had begun to yield, that some approximation to the truth had at last been arrived at with regard to the distance of a fixed star, and it was fit and proper that the Royal Astronomical Society of London should acknowledge the labours of him who had most effectually contributed to this end.

Henderson's results seemed sufficiently convincing, but they depended upon determinations of the absolute place of α Centauri. The experiences of the skilful astronomer Brinkley at Dublin were still fresh in the minds of astronomers. He had arrived by similar though less perfect means at results like those of Henderson; but his results had been proved to be fallacious, though the causes of their being so still remain somewhat inexplicable. In the case of Struve's observations the weight of evidence which he produced and the excellence of his method were admitted, but men were not prepared by experience for accepting as accurate the minute changes of angle which Struve had to measure—nor, I am bound to admit, was the proof afforded by Struve's series of observations so entirely convincing as that afforded by the series of Bessel. Therefore to Bessel the well-earned medal was given, but the labours of Struve and Henderson received high and honourable mention. I quote from the speech of Sir John Herschel in awarding that medal. He says of Henderson's researches on α Centauri:—

"Should a different eye and a different circle continue to give the same result, we must of course acquiesce in the conclusion; and the distinct and entire merit of the *first* discovery of the parallax of *a* fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in anticipating a decision which time alone can stamp with the seal of absolute authority."

So much for Sir John Herschel's officially expressed opinion. I can state now, and as Henderson's successor I do so with pride and pleasure, that a different eye (that of his able and sympathetic successor, Sir Thomas Maclear) fully confirmed Henderson's result with another circle; and further, that Henderson's result has been still further confirmed by additional researches of which I shall presently speak.

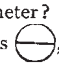
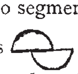
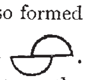
I must now pass over briefly the history of succeeding researches, and indeed it has been so admirably and so recently told within these walls by Dr. Ball that it is quite unnecessary I should enter upon it in detail. The most reliable values arrived at for the parallaxes of the stars of the northern hemisphere are given in the following table, and to these results I shall afterwards refer:—

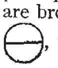
TABLE I.—*Parallaxes of Stars which have been determined in the Northern Heavens with considerable Accuracy*

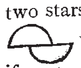
	Magnitude	Proper motion	Parallax
		″	″
61 Cygni	6	5·14	0·50
Lalande 21185 ...	7¼	4·75	0·50
α Tauri	1	0·19	0·52
34 Groombridge ...	8	2·81	0·29
Lalande 21258 ...	8½	4·40	0·26
O.Mg. 17415 ...	9	1·27	0·25
σ Draconis	—	1·87	0·25
α Lyræ	1	0·31	0·20
p Ophiuchi	4½	1·0	0·17
α Bootis...	1	2·43	0·13 ?
Groombridge 1830.	7	7·05	0·09
Bradley 3077 ...	6	2·09	0·07
85 Pegasi	6	1·38	0·05

The recent researches referred to in the title of this evening's lecture are some investigations which, in conjunction with a young American friend, Dr. Elkin, who was my guest for two years, I have recently carried out at the Cape of Good Hope.

The instrument employed was a heliometer—my own property—the good qualities of which I had previously tested at Mauritius in 1874 and at the Island of Ascension in 1877.

Now what is a heliometer? It is a telescope of which the object-glass is divided thus , and the two segments so formed can be moved with respect to each other, thus  and .

Here is a model which has been constructed to illustrate the principle of the instrument. You see that when the two segments are brought into what we may call their natural position, thus , that a heliometer differs in no way from an ordinary telescope—its divided lens produces a single image of a point of light, as will be evident from the image of the single artificial disk now on the screen. In optical language, the optical centres of the two segments are in coincidence, and so the images produced by each segment of the lens are in coincidence. But now, if the segments are separated, either segment produces a separate image of the artificial star, and the separation of the images is proportional to the separation of the segments.

Now, to illustrate how this instrument is used in observation, let there be two artificial stars—*a* and *b*. When the optical centres of the segments are in coincidence, we have on the screen—or in the field of view of the telescope—the images of these two stars. By separating the optical centres of the segments thus  we obtain double images of each of the stars *a* and *b*. Now if we turn the direction of the line of motion of the divided segments parallel to the direction of the stars *a* and *b*, and if we separate the lenses sufficiently we can make one of the images of the star *a* coincide with one of the images of star *b*. Similarly if we cross the segments we can bring the second image of star *b* into coincidence with the second image of star *a*, and if we have finely divided scales attached to the slides by which the segments are separated we can read off, in terms of these scales, the amount of this separation, and this separation is obviously twice the angle between the stars *a b*.

There is now upon the screen a photograph from a drawing illustrating the arrangements by which the segments of my heliometer are moved, and showing the scales by which the amount of the movement is measured; and these scales are read off by a powerful microscope from the eye end of the telescope, as in the photograph of the instrument now on the screen.

There is now on the screen a photograph of a drawing of the most perfect heliometer in the world, recently made by Messrs. Repsold of Hamburg for the Observatory of Yale College, New Haven, U.S. That instrument is now under the charge of my young friend, Dr. Elkin, of whom I have already spoken. If then we wish to observe the angle between two stars, it is only necessary to separate the segments of the object-glass by the required amount, to rotate the tube till the line of section of the object-glass is in the line joining the stars, to direct the axis of the telescope to a point in the heavens midway between the two stars under observation, and then we shall find in the field of view the two stars the angle between which we wish to measure. Then by slow and delicate changes in the distance of the optical centres of the segments, whilst the images of the stars are made to pass and repass through each other—thus—we are able to exactly adjust the angular distance of the segments to correspond truly with angular distance of the stars.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Museums and Lecture-Rooms Syndicate have recommended the immediate erection of a new lecture-room for physiology, with large additions to the rooms for practical physiology and to the work-rooms adjoining the Comparative Anatomy Museum, at an estimated cost of 7500*l.* These are to be carried along Corn Exchange Street. A work-room for the large class of Elementary Biology is also recommended to be built as an additional story above the Museum of Mineralogy, at a cost of about 2500*l.* It is also recommended that 1000*l.* be laid out in the purchase of microscopes.

The Board of Biology and Geology have modified their report respecting demonstrators and lecturers in Animal Morphology, on learning that the General Board of Studies cannot support their former proposals owing to the financial state of the University. They now ask for a lecturer on Vertebrates at 100*l.* and one on Invertebrates at 50*l.*, together with a demonstrator at 150*l.*, to be appointed by the Senior Lecturer in Animal Morphology.

The Rev. J. Venn, of Gonville and Caius College, has been approved for the degree of D.Sc.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 15.—"Some Experiments on Metallic Reflection. No. V. On the Amount of Light Reflected by Metallic Surfaces. III." By Sir John Conroy, Bart., M.A. Communicated by Prof. G. G. Stokes, Sec.R.S.