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MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]

TO-NIGHT, for the first time in its history, the British Association meets in the ancient city of Leicester; and it now becomes my privilege to convey to you, Mr. Mayor, and to the citizens generally, an expression of our thanks for your kind invitation and for the hospitable reception which you have accorded to us.

Here in Leicester and last year in York the association has followed its usual custom of holding its annual meeting somewhere in the United Kingdom; but in 1905 the meeting was, as you know, held in South Africa. Now, having myself only recently come from the Cape, I wish to take this opportunity of saying that this southern visit of the association has, in my opinion, been productive of much good; wider interest in science has been created amongst colonists, juster estimates of the country and its problems have been formed on the part of the visitors, and personal friendships and interchange of ideas between thinking men in South Africa and at home have arisen which can not fail to have a beneficial influence on the social, political and scientific relations between these colonies and the mother country. We may confidently look for like results from the proposed visit of the association to Canada in 1909.

One is tempted to take advantage of the wide publicity given to words from this

[1] Leicester, 1907.

chair to speak at large in the cause of science, to insist upon the necessity for its wider inclusion in the education of our youth and the devotion of a larger measure of the public funds in aid of scientific research; to point to the supreme value of science as a means for the culture of those faculties which in man promote that knowledge which is power; and to show how dependent is the progress of a nation upon its scientific attainment.

But in recent years these truths have been prominently brought before the association from this chair; they have been exhaustively demonstrated by Sir William Huggins from the chair of the Royal Society, and now a special guild[2] exists for their enforcement upon the mind of the nation.

These considerations appear to warrant me in following the healthy custom of so many previous presidents—viz., of confining their remarks mainly to those departments of science with which the labors of their lives have been chiefly associated.

THE SCIENCE OF MEASUREMENT

Lord Kelvin in 1871 made a statement from the presidential chair of the association at Edinburgh as follows:

> Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than the looking for something new. But nearly all the grandest discoveries of science have been the reward of accurate measurement and patient, long-continued labor in the minute sifting of numerical results.

Besides the instances quoted by Lord Kelvin in support of that statement, we have perhaps as remarkable and typical an exemplification as any in Lord Rayleigh's long-continued work on the density of nitrogen which led him to the discovery of argon. We shall see presently that, true as Lord Kelvin's words are in regard to most fields of science, they are specially applicable as a guide in astronomy.

One of Clerk Maxwell's lectures in the Natural Philosophy Class at Marischall College, Aberdeen, when I was a student under him there, in the year 1859, ran somewhat as follows:

> A standard, as it is at present understood in England, is not a real standard at all; it is a rod of metal with lines ruled upon it to mark the yard, and it is kept somewhere in the House of Commons. If the House of Commons catches fire there may be an end of your standard. A copy of a standard can never be a real standard, because all the work of human hands is liable to error. Besides, will your so-called standard remain of a constant length? It certainly will change by temperature, it probably will change by age (that is, by the rearrangement or settling down of its component molecules), and I am not sure if it does not change according to the azimuth in which it is used. At all events, you must see that it is a very impractical standard—impractical because, if, for example, any one of you went to Mars or Jupiter, and the people there asked you what was your standard of measure, you could not tell them, you could not reproduce it, and you would feel very foolish. Whereas, if you told any capable physicist in Mars or Jupiter that you used some natural invariable standard, such as the wave-length of the D-line of sodium vapor, he would be able to reproduce your yard or your inch, provided that you could tell him how many of such wave-lengths there were in your yard or your inch, and your standard would be available anywhere in the universe where sodium is found.

That was the whimsical way in which Clerk Maxwell used to impress great principles upon us. We all laughed before we understood; then some of us understood and remembered.

Now the scientific world has practically adopted Maxwell's form of natural standard. It is true that it names that standard the meter; but that standard is not one millionth of the earth's quadrant in length, as it was intended to be; it is merely a certain piece of metal approximately of that length.

It is true that the length of that piece

[2] The British Science Guild.

of metal has been reproduced with more precision, and is known with higher accuracy in terms of many secondary standards, than is the length of any other standard in the world; but it is, after all, liable to destruction and to possible secular change of length. For these reasons it can not be scientifically described otherwise than as a piece of metal whose length at 0° C. at the epoch A.D. 1906 is = 1,553,164 times the wave-length of the red line of the spectrum of cadmium when the latter is observed in dry air at the temperature of 15° C. of the normal hydrogen-scale at a pressure of 760 mm. of mercury at 0° C.

This determination, recently made by methods based on the interference of light-waves and carried out by MM. Perot and Fabry at the International Bureau of Weights and Measures, constitutes a real advance in scientific metrology. The result appears to be reliable within one ten-millionth part of the meter.

The length of the meter, in terms of the wave-length of the red line in the spectrum of cadmium, had been determined in 1892 by Michelson's method, with a mean result in almost exact accordance with that just quoted for the comparisons of 1906; but this agreement (within one part in ten millions) is due in some degree to chance, as the uncertainty of the earlier determination was probably ten times greater than the difference between the two independent results of 1892 and 1906.

We owe to M. Guillaume, of the same International Bureau, the discovery of the remarkable properties of the alloys of nickel and steel, and from the point of view of exact measurement the specially valuable discovery of the properties of that alloy which we now call "invar." He has developed methods for treatment of wires made from this alloy which render more permanent the arrangement of their constituent molecules. Thus these wires, with their attached scales, may, for considerable periods of time and under circumstances of careful treatment, be regarded as nearly invariable standards. With proper precautions, we have found at the Cape of Good Hope that these wires can be used for the measurement of base lines of the highest geodetic precision with all the accuracy attainable by the older and most costly forms of apparatus; whilst with the new apparatus a base of 20 kilometers can be measured in less time and for less cost than one of a single kilometer with the older forms of measurement.

THE GREAT AFRICAN ARC OF MERIDIAN

In connection with the progress of geodesy, time only permits me to say a few words about the Great African arc on the thirtieth meridian, which it is a dream of my life to see completed.

The gap in the arc between the Limpopo and the previously executed triangulation in Rhodesia, which I reported to the Association at the Johannesburg meeting in 1905, has now been filled up. My own efforts, at 6,000 miles distance, had failed to obtain the necessary funds, but at Sir George Darwin's instance contributions were obtained from this association, from the Royal Society and others, to the extent of half the estimated cost; the remaining half was met by the British South Africa Company. But for Darwin's happy intervention, which enabled me to secure the services of Captain Gordon and his party before the Transvaal Survey Organization was entirely broken up, this serious gap in the great work would probably have long remained; for it is one thing to add to an existing undertaking of the kind, it is quite another to create a new organization for a limited piece of work.

Since then Colonel (now Sir William)

Morris has brought to a conclusion the reductions of the geodetic survey of the Transvaal and Orange River Colony, and his report is now in my hands for publication.

Dr. Rubin, under my direction, at the cost of the British South Africa Company, has carried the arc of meridian northwards to south latitude 9° 42′, so that we have now continuous triangulation from Cape L'Agulhas to within fifty miles of the southern end of Lake Tanganyika; that is to say, a continuous geodetic survey extending over twenty-five degrees of latitude.

It happens that, for the adjustment of the international boundary between the British Protectorate and the Congo Free State, a topographic survey is at the present moment being executed northward along the thirtieth meridian from the northern border of German East Africa. A proposal on the part of the Royal Society, the Royal Geographical Society, the British Association and the Royal Astronomical Society has been made to strengthen this work by carrying a geodetic triangulation through it along the thirtieth meridian, and thus adding 2½° to the African arc. These societies together guarantee 1,000*l.* towards the cost of the work, and ask for a like sum from government to complete the estimated cost. The topographic survey will serve as the necessary reconnaisance. The topographic work will be completed by the end of January next, and the four following months offer the best season of the year for geodetic operations in these regions.

There is a staff of skilled officers and men on the spot sufficient to complete the work within the period mentioned, and the Intercolonial Council of the Transvaal and Orange River Colony most generously offers to lend the necessary geodetic instruments. The work will have to be done sooner or later, but if another expedition has to be organized for the purpose the work will then cost from twice to three times the present amount. One can not, therefore, doubt that His Majesty's government will take advantage of the present offer and opportunity to vote the small sum required. This done, we can not doubt that the German government will complete the chain along the eastern side of Lake Tanganyika, which lies entirely within their territory. Indeed, it is no secret that the Berlin Academy of Sciences has already prepared the necessary estimates with a view to recommending action on the part of its government.

Captain Lyons, who is at the head of the survey of Egypt, assures me that preliminary operations towards carrying the arc southwards from Alexandria have been begun, and we have perfect confidence that in his energetic hands the work will be prosecuted with vigor. In any case the completion of the African arc will rest largely in his hands. That arc, if ever my dream is realized, will extend from Cape L'Agulhas to Cairo, thence round the eastern shore of the Mediterranean and the islands of Greece, and there meet the triangulation of Greece itself, the latter being already connected with Struve's great arc, which terminates at the North Cape in lat. 70° N. This will constitute an arc of 105° in length—the longest arc of meridian that is measurable on the earth's surface.

THE SOLAR PARALLAX

Much progress has been made in the exact measurement of the great fundamental unit of astronomy—the solar parallax.

Early in 1877 I ventured to predict[3]

[3] "The Determination of the Solar Parallax," *The Observatory*, Vol. I., p. 280.

that we should not arrive at any certainty as to the true value of the solar parallax from observations of transits of Venus, but that the modern heliometer applied to the measurement of angular distances between stars and the star-like images of minor planets would yield results of far higher precision.

The results of the observations of the minor planets Iris, Victoria and Sappho at their favorable oppositions in the years 1888 and 1889, which were made with the cooperation of the chief heliometer and meridian observatories, fully justified this prediction.[4] The sun's distance is now almost certainly known within one thousandth part of its amount. The same series of observations also yielded a very reliable determination of the mass of the moon.

The more recently discovered planet Eros, which in 1900 approached the earth within one third of the mean distance of the sun, afforded a most unexpected and welcome opportunity for redetermining the solar parallax—an opportunity which was largely taken advantage of by the principal observatories of the northern hemisphere. Unfortunately the high northern declination of the planet prevented its observation at the Cape and other southern observatories. So far as the results have been reduced and published[5] they give an almost exact accordance with the value of the solar parallax derived from the heliometer observations of the minor planets, Iris, Victoria and Sappho in 1888 and 1889.

But in 1931 Eros will approach the earth within one sixth part of the sun's mean distance, and the fault will rest with astronomers of that day if they do not succeed in determining the solar parallax within one ten-thousandth part of its amount.

To some of us who struggled so hard to arrive at a tenth part of this accuracy under the less favorable geometrical conditions that were available before the discovery of Eros, how enviable seems the opportunity!

And yet, if we come to think of it rightly, the true opportunity and the chief responsibility is ours, for *now* and not twenty years hence is the time to begin our preparation; *now* is the time to study the origin of those systematic errors which undoubtedly attach to some of our photographic processes; and then we ought to construct telescopes specially designed for the work. These telescopes should be applied to the charting of the stars near the path which Eros will describe at its opposition in 1931, and the resulting star-coordinates derived from the plates photographed by the different telescopes should be rigorously intercompared. Then, if all the telescopes give identical results for the star-places, we can be certain that they will record without systematic error the position of Eros. If they do not give identical results, the source of the errors must be traced.

The planet will describe such a long path in the sky during the opposition of 1931 that it is already time to begin the meridian observations which are necessary to determine the places of the stars that are to be used for determining the constants of the plates. It is desirable, therefore, that some agreement should be come to with respect to selection of these reference-stars, in order that all the principal meridian observatories in the world may take part in observing them.

I venture to suggest that a Congress of

[4] *Annals of the Cape Observatory*, Vol. VI., part 6, p. 29.

[5] *Monthly Notices R.A.S.*, Hinks, Vol. LXIV., p. 725; Christie, Vol. LXVII., p. 382.

Astronomers should assemble in 1908 to consider what steps should be taken with reference to the important opposition of Eros in 1931.

THE STELLAR UNIVERSE

And now to pass from consideration of the dimensions of our solar system to the study of the stars, or other suns, that surround us.

To the lay mind it is difficult to convey a due appreciation of the value and importance of star-catalogues of precision. As a rule such catalogues have nothing whatever to do with discovery in the ordinary sense of the word, for the existence of the stars which they contain is generally well known beforehand; and yet such catalogues are, in reality, by far the most valuable assets of astronomical research.

If it be desired to demarcate a boundary on the earth's surface by astronomical methods, or to fix the position of any object in the heavens, it is to the accurate star-catalogue that we must refer for the necessary data. In that case the stars may be said to resemble the trigonometrical points of a survey, and we are only concerned to know from accurate catalogues their positions in the heavens at the epoch of observation. But in another and grander sense the stars are not mere landmarks, for each has its own apparent motion in the heavens which may be due in part to the absolute motion of the star itself in space, or in part to the motion of the solar system by which our point of view of surrounding stars is changed.

If we desire to determine these motions and to ascertain something of the general conditions which produce them, if we would learn something of the dynamical conditions of the universe and something of the velocity and direction of our own solar system through space, it is to the accurate star-catalogues of widely separated epochs that we must turn for a chief part of the requisite data.

The value of a star-catalogue of precision for present purposes of cosmic research varies as the square of its age and the square of its accuracy. We can not alter the epoch of our observations, but we can increase their value fourfold by doubling their accuracy. Hence it is that many of our greater astronomers have devoted their lives chiefly to the accumulation of meridian observations of high precision, holding the view that to advance such precision is the most valuable service to science they could undertake, and comforted in their unselfish and laborious work only by the consciousness that they are preparing a solid foundation on which future astronomers may safely raise the superstructure of sound knowledge.

But since the extension of our knowledge of the system of the universe depends quite as much on past as on future research, it may be well, before determining upon a program for the future, to consider briefly the record of meridian observation in the past for both hemispheres.

THE COMPARATIVE STATE OF ASTRONOMY IN THE NORTHERN AND SOUTHERN HEMISPHERES

It seems probable that the first express reference to southern constellations in known literature occurs in the Book of Job (ix. 9): "Which maketh Arcturus, Orion and Pleiades, and the chambers of the south." Schiaparelli's strongly supported conjecture is that the expression "chambers of the south," taken with its context, signifies the brilliant stellar region from Canopus to α Centauri, which includes the Southern Cross and coincides with the most brilliant portion of the Milky Way.

About the year 750 B. C. (the probable date of the Book of Job) all these stars culminated at altitudes between 5° and 16° when viewed from the latitude of Judea; but now, owing to precessional change, they can only be seen in a like striking manner from a latitude about 12° further south.

The words of Dante have unquestionably originated the wonderful net of poetic fancy that has been woven about the asterism, which we now call Crux.

> To the right hand I turned, and fixed my mind
> On the other pole attentive, where I saw
> Four stars ne'er seen before save by the ken
> Of our first parents—Heaven of their rays
> Seemed joyous. O thou northern site! bereft
> Indeed, and widowed, since of these deprived.

All the commentators agree that Dante here referred to the stars of the Southern Cross.

Had Dante any imperfect knowledge of the existence of these stars, any tradition of their visibility from European latitudes in remote centuries, so that he might poetically term them the stars of our first parents?

Ptolemy catalogues them as 31, 32, 33 and 34 Centauri, and they are clearly marked on the Borgian globe described by Assemanus in 1790. This globe was constructed by an Arabian in Egypt: it bears the date 622 Hegira, corresponding with A. D. 1225, and it is possible that Dante may have seen it.

Amerigo Vespucci, as he sailed in tropical seas, apparently recognized in what we now call Crux the four luminous stars of Dante; for in 1501 he claimed to be the first European to have looked upon the stars of our first parents. His fellow-voyager, Andrea Corsali, wrote about the same time to Giuliano di Medici describing "the marvelous cross, the most glorious of all the celestial signs."

Thus much mysticism and romance have been woven about this constellation, with the result that exaggerated notions of its brilliancy have been formed, and to most persons its first appearance, when viewed in southern latitudes, is disappointing.

To those, however, who view it at upper culmination for the first time from a latitude a little south of the Canary Islands, and who at the same time make unconsciously a mental allowance for the absorption of light to which one is accustomed in the less clear skies of northern Europe, the sight of the upright cross, standing as if fixed to the horizon, is a most impressive one. I at least found it so on my first voyage to the Cape of Good Hope. But how much more strongly must it have appealed to the mystic and superstitious minds of the early navigators as they entered the unexplored seas of the northern tropic! To them it must have appeared the revered image of the cross pointing the way on their southward course—a symbol and sign of hope and faith on their entry to the unknown.

The first general knowledge of the brighter stars of the southern hemisphere we owe to Frederick de Hautman, who commanded a fleet sent by the Dutch Government in 1595 to the Far East for the purpose of exploring Japan. Hautman was wrecked and taken prisoner at Sumatra, and whilst there he studied the language of the natives and made observations of the positions and magnitudes of the fixed stars of the southern hemisphere.[6]

Our distinguished countryman Halley visited St. Helena in 1677 for the purpose of cataloguing the stars of the southern hemisphere. He selected a station now marked Halley's Mount on the admiralty

[6] The resulting catalogue of 304 stars is printed as an appendix to Hautman's "Vocabulary of the Malay Language," published at Amsterdam in 1603.

chart of the island. I have visited the site, and the foundations of the observatory still remain. Halley's observations were much hindered by cloud. On his return to England, Halley in 1679 published his "Catalogus Stellarum Australium," containing the magnitudes, latitudes and longitudes of 341 stars, which, with the exception of seven, all belonged to the southern hemisphere.

But the first permanently valuable astronomical work in the southern hemisphere was done in 1751–52 by the Abbé de Lacaille. He selected the Cape of Good Hope as the scene of his labors, because it was then perhaps the only spot in the world situated in a considerable southern latitude which an unprotected astronomer could visit in safety, and where the necessary aid of trained artisans to erect his observatory could be obtained. Lacaille received a cordial welcome at the hands of the Dutch governor Tulbagh: he erected his observatory in Cape Town, made a catalogue of nearly 10,000 stars, observed the opposition of Mars, and measured a short arc of meridian all in the course of a single year. Through his labors the Cape of Good Hope became the birthplace of astronomy and geodesy in the southern hemisphere.

Bradley was laying the foundations of exact astronomy in the northern hemisphere at the time when Lacaille labored at the Cape. But Bradley had superior instruments to those of Lacaille and much longer time at his disposal. Bradley's work is now the basis on which the fair superstructure of modern astronomy of precision rests. His labors were continued by his successors at Greenwich and by a long series of illustrious men like Piazzi, Groombridge, Bessel, Struve and Argelander. But in the southern hemisphere the history of astronomy is a blank for seventy years from the days of Lacaille.

We owe to the establishment of the Royal Observatory at the Cape by an Order in Council of 1820 the first successful step towards the foundation of astronomy of high precision in the southern hemisphere.

Time does not permit me to trace in detail the labors of astronomers in the southern hemisphere down to the present day; and this is the less necessary because in a recent presidential address to the South African Philosophical Society[7] I have given in great part that history in considerable detail. But I have not there made adequate reference to the labors of Dr. Gould and Dr. Thome at Cordoba. To their labors, combined with the work done under Stone at the Cape, we owe the fact that for the epoch 1875 the meridian sidereal astronomy of the southern hemisphere is nearly as well provided for as that of the northern. The point I wish to make is that the facts of exact sidereal astronomy in the southern hemisphere may be regarded as dating nearly a hundred years behind those of the northern hemisphere.

THE CONSTITUTION OF THE UNIVERSE

IT was not until 1718, when Edmund Halley, afterwards Astronomer Royal of England, read a paper before the Royal Society,[8] entitled "Considerations on the Change of the Latitudes of Some of the Principal Fixt Stars," that any definite facts were known about the constitution of the universe. In that paper Halley, who had been investigating the precession of the equinoxes, says:

> But while I was upon this enquiry I was surprised to find the latitudes of three of the principal stars in heaven directly to contradict the supposed

[7] *Trans. South African Phil. Soc.*, Vol. XIV., part 2.

[8] *Phil. Trans.*, 1718, p. 738.

greater obliquity of the Ecliptick, which seems confirmed by the latitudes of most of the rest.

This is the first mention in history of an observed change in the relative position of the so-called fixed stars—the first recognition of what we now call "proper motion."

Tobias Mayer, in 1760, seems to have been the first to recognize that if our sun, like other stars, has motion in space, that motion must produce apparent motion amongst the surrounding stars; for in a paper to the Göttingen Academy of Sciences he writes:

If the sun, and with it the planets and the earth which we inhabit, tended to move directly towards some point in the heavens, all the stars scattered in that region would seem to gradually move apart from each other, whilst those in the opposite quarter would mutually approach each other. In the same manner one who walks in the forest sees the trees which are before him separate, and those that he leaves behind approach each other.

No statement of the matter could be more clear; but Mayer, with the meager data at his disposal, came to the conclusion that "the motions of the stars are not governed by the above or any other common law, but belong to the stars themselves."

Sir William Herschel, in 1783, made the first attempt to apply, with any measure of success, Mayer's principle to a determination of the direction and amount of the solar motion in space.[9] He derived, as well as he could from existing data, the proper motions of fourteen stars, and arrived by estimation at the conclusion that the sun's motion in space is nearly in the direction of the star λ Herculis, and that 80 per cent. of the apparent motions of the fourteen stars in question could be assigned to this common origin.

This conclusion rests in reality upon a very slight basis, but the researches of subsequent astronomers show that it was an amazing accidental approach to truth—indeed, a closer approximation than Herschel's subsequent determinations of 1805 and 1806, which rested on wider and better data.[10]

Consider for a moment the conditions of the problem. If all the stars except our sun were at rest in space, then, in accordance with Mayer's statement, just quoted, all the stars would have apparent motions on great circles of the sphere away from the apex and towards the antapex of the solar motion. That is to say, if the position of each star of which the apparent motion is known was plotted on the surface of a sphere and a line with an arrow-head drawn through each star showing the direction of its motion on the sphere, then it should be possible to find a point on the sphere such that a great circle drawn from this point through any star would coincide with the line of direction of that star's proper motion. The arrow-heads would all point to that intersection of the great circles which is the antapex of the solar motion, and the other point of intersection of the great circles would be the apex, that is to say, the direction of the sun's motion in space.

But as the apparent stellar motions are small and only determinable with a considerable percentage of error, it would be impossible to find any point on the sphere such that every great circle passing through it and any particular star, would in every case be coincident with the observed direction of motion of that star.

Such discordances would, on our original assumption, be due to errors of observation, but in reality much larger discordances will occur, which are due to the fact that the other stars (or suns) have independent motions of their own in space.

[9] *Phil. Trans.*, 1783, p. 247.

[10] *Phil. Trans.*, 1805, p. 233; 1806, p. 205.

This at once creates a new difficulty, viz., that of defining an absolute locus in space. The human mind may exhaust itself in the effort, but it can never solve the problem. We can imagine, for example, the position of the sun at any moment to be defined with reference to any number of surrounding stars, but by no effort of imagination can we devise means of defining the *absolute* position of a body in space without reference to surrounding material objects. If, therefore, the referring objects have unknown motions of their own, the rigor of the definition is lost.

What we call the observed proper motion of a star has three possible sources of origin:

1. The *parallactic motion*, or the effect of our sun's motion through space, whereby our point of view of surrounding celestial objects is changed.

2. The *peculiar* or particular motion of the star, *i. e.*, its own *absolute* motion in space.

3. That part of the observed or tabular motion which is due to inevitable error of observation.

In all discussions of the solar motion in space, from that of Herschel down till a recent date, it has been assumed that the peculiar motions of the stars are arranged at random, and may therefore be considered zero in the mean of a considerable number of them. It is then possible to find such a value for the precession, and such a common apex for the solar motion as shall leave the residual peculiar motions of the stars under discussion to be in the mean = zero. That is to say, we refer the motion of the sun in space to the center of gravity of all the stars considered in the discussion, and regard that center of gravity as immovable in space.

In order to proceed rigorously, and especially to determine the amount as well as the direction of the sun's motion in space, we ought to know the parallax of every star employed in the discussion, as well as its proper motion. In the absence of such data it has been usual to start from some such assumption as the following: the stars of a particular magnitude are roughly at the same distance; those of different classes of magnitude may be derived from the hypothesis that on the average they have all equal absolute luminosity.

The assumption is not a legitimate one—

1. Because of the extreme difference in the absolute luminosity of stars.

2. Because it implies that the average absolute luminosity of stars is the same in all regions of space.

The investigation has been carried out by many successive astronomers on these lines with fairly accordant results as to the position of the solar apex, but with very unsatisfactory results as to the distances of the fixed stars.[11] In order to judge how far the magnitude (or brightness) of a star is an index of its probable distance, we must have evidence from direct determinations of stellar parallax.

STELLAR PARALLAX

To extend exact measurement from our own solar system to that of other suns

[11] Argelander, *Mém. présentés à l'Acad. Imp. des Sciences St. Pétersbourg*, tome III.; Lundahl, *Astron. Nachrichten*, **398**, 209; Argelander, *Astron. Nachrichten*, **398**, 210; Otto Struve, *Mém. Acad. des Sciences St. Pétersbourg*, VI^e^ série, Math. et Phys., tome III., p. 17; Galloway, *Phil. Trans.*, 1847, p. 79; Mädler, *Dorpat Observations*, Vol. XIV., and *Ast. Nach.*, **566**, 213; Airy, *Mém. R.A.S.*, Vol. XXVIII., p. 143; Dunkin, *Mem. R.A.S.*, Vol. XXXII., p. 19; Stone, *Monthly Notices R.A.S.*, Vol. XXIV., p. 36; De Ball, inaugural dissertation, Bonn, 1877; Rancken, *Astron. Nachrichten*, **2482**, 149; Bischoff, inaugural dissertation, Bonn, 1884; Ludwig Struve, *Mém. Acad. St. Pétersbourg*, VII^e^ série, tome XXXV., No. 3.

and other systems may be regarded as the supreme achievement of practical astronomy. So great are the difficulties of the problem, so minute the angles involved, that it is but in comparatively recent years that any approximate estimate could be formed of the true parallax of any fixed star. Bradley felt sure that if the star γ Draconis had a parallax of 1″ he would have detected it. Henderson by "the minute sifting of the numerical results" of his own meridian observations of α Centauri, made at the Cape of Good Hope in 1832–33, first obtained certain evidence of the measurable parallax of any fixed star. He was favored in this discovery by the fact that the object he selected happened to be, so far as we yet know, the nearest sun to our own. Shortly afterwards Struve obtained evidence of a measurable parallax for α Lyræ and Bessel for 61 Cygni. Astronomers hailed with delight this bursting of the constraints which our imperfect means imposed on research. But for the great purposes of cosmical astronomy what we are chiefly concerned to know is not what is the parallax of this or that particular star, but rather what is the average parallax of a star having a particular magnitude and proper motion. The prospect of even an ultimate approximate attainment of this knowledge seemed remote. The star α Lyræ is one of the brightest in the heavens; the star 61 Cygni one that had the largest proper motion known at the time; whilst α_2 Centauri is not only a very bright star, but it has also a large proper motion. The parallaxes of these stars must therefore in all probability be large compared with the parallax of the average star; but yet to determine them with approximate accuracy long series of observations by the greatest astronomers and with the finest instruments of the day seemed necessary.

Subsequently various astronomers investigated the parallaxes of other stars having large proper motions, but it was only in 1881, at the Cape of Good Hope, that general research on stellar parallax was instituted.[12] Subsequently at Yale and at the Cape of Good Hope the work was continued on cosmical lines with larger and improved heliometers.[13] By the introduction of the reversing prism and by other practical refinements the possibilities of systematic error were eliminated, and the accidental errors of observation reduced within very small limits.

These researches brought to light the immense diversity in the absolute luminosity and velocity of motion of different stars. Take the following by way of example:

Our nearest neighbor amongst the stars, α_2 Centauri, has a parallax of 0″.76, or is distant about 4⅓ light-years. Its mass is independently known to be almost exactly equal to that of our sun; and its spectrum being also identical with that of our sun, we may reasonably assume that it appears to us of the same magnitude as would our sun if removed to the distance of α_2 Centauri.

But the average star of the same apparent magnitude as α_2 Centauri was found to have a parallax of only 0″.10, so that either α_2 Centauri or our sun, if removed to a distance equal to that of the average fixed star of the first magnitude, would appear to us but little brighter than a star of the fifth magnitude.

Again, there is a star of only 8½ magnitude[14] which has the remarkable annual proper motion of nearly 8¾ seconds of arc —one of those so-called runaway stars—

[12] *Mem. R.A.S.*, Vol. XLVIII.

[13] *Annals of the Cape Observatory*, Vol. VIII., part 2, and *Trans. Astron. Observatory of Yale University*, Vol. I.

[14] Gould's Zones, V^h 243.

which moves with a velocity of 80 miles per second at right angles to the line of sight (we do not know with what velocity in the line of sight). It is at about the same distance from us as Sirius, but it emits but one ten-thousandth part of the light-energy of that brilliant star. Sirius itself emits about thirty times the light-energy of our sun, but it in turn sinks into insignificance when compared with the giant Canopus, which emits at least 10,000 times the light-energy of our sun.

Truly "one star differs from another star in glory." Proper motion rather than apparent brightness is the truer indication of a star's probable proximity to the sun. Every star of considerable proper motion yet examined has proved to have a measurable parallax.

This fact at once suggests the idea, Why should not the apparent parallactic motions of the stars, as produced by the sun's motion in space, be utilized as a means of determining stellar parallax?

SECULAR PARALLACTIC MOTION OF STARS

The strength of such determinations, unlike those made by the method of annual parallax, would grow with time. It is true that the process can not be applied to the determination of the parallax of individual stars, because the peculiar motion of a particular star can not be separated from that part of its apparent motion which is due to parallactic displacement. But what we specially want is not to ascertain the parallax of the individual star, but the mean parallax of a particular group or class of stars, and for this research the method is specially applicable, provided we may assume that the peculiar motions are distributed at random, so that they have no systematic tendency in any direction; in other words, that the center of gravity of any extensive group of stars will remain fixed in space.

This assumption is, of course, but a working hypothesis, and one which from the paper on star-streaming communicated by Professor Kapteyn, of Groningen, to the Johannesburg meeting of the Association two years ago we already know to be inexact.[15] Kapteyn's results were quite recently confirmed in a remarkable way by Eddington,[16] using independent material discussed by a new and elegant method. Both results showed that, at least for extensive parts of space, there are a nearly equal number of stars moving in exactly opposite directions. The assumption, then, that the mean of the peculiar motions is zero may, at least for these parts of space, be still regarded as a good working hypothesis.

Adopting an approximate position of the apex of the solar motion, Kapteyn resolved the observed proper motions of the Bradley stars into two components, viz., one in the plane of the great circle passing through the star and the apex, the other at right angles to that plane.[17] The former component obviously includes the whole of the parallactic motion; the latter is independent of it, and is due entirely to the real motions of the stars themselves. From the former the mean parallactic motion of the group is derived, and from the combination of the two components, the relation of velocity of the sun's motion to that of the mean velocity of the stars of the group.

As the distance of any group of stars found by the parallactic motion is expressed as a unit in terms of the sun's yearly motion through space, the velocity

[15] *Rep. Brit. Assoc.*, 1905, p. 257.

[16] *Monthly Notices R.A.S.*, Vol. LXVII., p. 34.

[17] *Publications Astron. Laboratory Groningen*, Nos. 7 and 9.

of this motion is one of the fundamental quantities to be determined. If the mean parallax of any sufficiently extensive group or class of stars was known we should have at once means for a direct determination of the velocity of the sun's motion in space; or if, on the other hand, we can by independent methods determine the sun's velocity, then the mean parallax of any group of stars can be determined.

DETERMINATION OF STELLAR MOTION IN THE LINE OF SIGHT

Science owes to Sir William Huggins the application of Doppler's principle to the determination of the velocity of star-motion in the line of sight. The method is now so well known, and such an admirable account of its theory and practical development was given by its distinguished inventor from this chair at the Cardiff meeting in 1891, that further mention of that part of the matter seems unnecessary.

THE VELOCITY OF THE SUN'S MOTION IN SPACE

If by this method the velocities in the line of sight of a sufficient number of stars situated near the apex and antapex of the solar motion could be determined, so that in the mean it could be assumed that their peculiar motions would disappear, we have at once a direct determination of the required velocity of the sun's motion.

The material for this determination is gradually accumulating, and indeed much of it, already accumulated, is not yet published. But even with the comparatively scant material available, it now seems almost certain that the true value of the sun's velocity lies between 18 and 20 kilometers per second;[18] or, if we adopt the mean value, 19 kilometers per second, this would correspond almost exactly with a yearly motion of the sun through space equal to four times the distance of the sun from the earth.

Thus the sun's yearly motion being four times the sun's distance, the parallactic motion of stars in which this motion is unforeshortened must be four times their parallax. How this number varies with the amount of foreshortening is of course readily calculated. The point is that from the mean parallactic motion of a group of stars we are now enabled to derive at once its mean parallax.

This research has been carried out by Kapteyn for stars of different magnitudes. It leads to the result that the parallax of stars differing *five* magnitudes does *not* differ in the proportion of one to ten, as would follow from the supposition of equal luminosity of stars throughout the universe, but only in the proportion of about one to five.[19]

The same method can not be applied to groups of stars of different proper motions, and it is only by a somewhat indirect proof, and by calling in the aid of such reliable results of direct parallax determination as we possess, that the variation of parallax with proper motion could be satisfactorily dealt with.

THE MEAN PARALLAXES OF STARS OF DIFFERENT MAGNITUDE AND PROPER MOTION

As a final result Kapteyn derived an empirical formula giving the average parallax for stars of different spectral types, and of any given magnitude and proper motion. This formula was published at Groningen in 1901.[20] Within the

[18] *Kapteyn Ast. Nach.*, No. 3487, p. 108; and Campbell, *Astrophys. Journ.*, XIII., p. 80.

[19] *Astron. Nachrichten*, No. 3487, Table III.; and *Ast. Journ.*, p. 566.

[20] *Publications Astron. Laboratory Groningen*, No. 8, p. 24.

past few months the results of researches on stellar parallax, made under the direction of Dr. Elkin, at the Astronomical Observatory of Yale University, during the past thirteen years,[21] have been published, and they afford a most crucial and entirely independent check on the soundness of Kapteyn's conclusions.

COMPARISON GROUPS ARRANGED IN ORDER OF PROPER MOTION

No. of Stars	Proper Motion	Magnitude	Parallax		Yale-Kapteyn
			Yale	Kapteyn	
21	0.″14	3.8	0.″028	0.″026	+0.″002
39	0.49	6.3	.042	.055	— .013
45	0.59	6.7	.068	.060	+ .008
46	0.77	6.5	.047	.074	— .027
22	1.50	6.2	.118	.124	— .006

GROUPS ARRANGED IN ORDER OF MAGNITUDE

No. of Stars	Proper Motion	Magnitude	Parallax		Yale-Kapteyn
			Yale	Kapteyn	
10	0.″61	0.8	0.″103	0.″110	—0.″007
29	.53	3.8	.076	.075	+ .001
33	.63	5.6	.064	.070	— .006
34	.73	6.7	.055	.070	— .017
31	.68	7.6	.025	.061	— .036
36	.80	8.3	.056	.062	— .006

	No. of Stars	Proper Motion	Magnitude	Parallax		Yale-Kapteyn
				Yale	Kapteyn	
Spectral Type I.	13	0.″42	4.0	0.″076	0.″076	0.″000
Spectral Type II.	81	0.67	5.3	0.067	0.074	—0.007

In considering the comparison between the more or less theoretical results of Kapteyn and the practical determinations of Yale, we have to remember that Kapteyn's tables refer only to the means of groups of a large number of stars having on the average a specified magnitude and proper motion, whilst the latter are direct determinations affected by the accidental errors of the separate determinations and by such uncertainty as attaches to the unknown parallaxes of the comparison stars—parallaxes which we have supplied from Kapteyn's general tables.

The Yale results consist of the determination of the parallax of 173 stars, of which only ten had been previously known to Kapteyn and had been utilized by him. Dividing these results into groups we get the comparison given above.

These results agree in a surprisingly satisfactory way, having regard to the comparatively small number of stars in each group and the great range of parallax which we know to exist amongst individual stars having the same magnitude and proper motion. In the mean perhaps the tabular parallaxes are in a minute degree too large, but we have unquestionable proof from this comparison that our knowledge of stellar distances now rests on a solid foundation.

THE DISTRIBUTION OF VARIETIES OF LUMINOSITY OF STARS

But, besides the mean parallax of stars of a particular magnitude and proper motion, it is essential that we should know approximately what percentage of the stars of such a group have twice, three times, etc., the mean parallax of the group, and what percentage only one half, one third of that parallax, and so on. In principle, at least, this frequency-law may be obtained by means of the directly determined parallaxes. For the stars of which we have reliable determinations we can compare these true parallaxes with the *mean* parallax of stars having corresponding magnitude and proper motion, and this comparison will lead to a knowledge of the frequency-law required. It is true that, owing to the scarcity of material at present available, the determination of the fre-

[21] *Trans. Astron. Observatory of Yale Univ.*, Vol. II., part 1.

quency-law is not so strong as may be desirable, but further improvement is simply a question of time and the augmentation of parallax-determination.

Adopting provisionally the frequency-law found in this way by Kapteyn,[22] we can localize all the stars in space down to about the ninth magnitude.

Take, for example, the stars of magnitude 5.5 to 6.5. There are about 4,800 of these stars in the whole sky. According to Auwers-Bradley, about 9½ per cent. of these stars, or some 460 in all, have proper motions between 0″.04 and 0″.05. Now, according to Kapteyn's empiric formula, whose satisfactory agreement with the Yale results has just been shown, the mean parallax of such stars is almost exactly 0″.01. Further, according to his frequency-law, 29 per cent. of the stars have parallaxes between the *mean* value and double the mean value; 6 per cent. have parallaxes between twice and three times the mean value; 1½ per cent. between three and four times the mean value. Therefore of our 460 stars 133 will have parallaxes between 0″.01 and 0″.02, twenty-eight between 0″.02 and 0″.03, seven between 0″.03 and 0″.04, and so on.

Localizing in the same way the stars of the sixth magnitude having other proper motions, and then treating the stars of the first magnitude, second magnitude, third magnitude, and so on to the ninth magnitude in the same way, we finally locate all these stars in space.[23]

It is true we have not localized the individual stars, but we know approximately and within certain limits of magnitude the number of stars at each distance from the sun.

Thus the apparent brightness and the distance being known we have the means of determining the light-energy or absolute *luminosity* of the stars, *provided it can be assumed that light does not suffer any extinction in its passage through interstellar space.*

On this assumption Kapteyn was led to the following results, viz., that within a sphere the radius of which is 560 light-years (a distance which corresponds with that of the average star of the ninth magnitude) there will be found:

1	star giving	from	100,000	to	10,000	times the	light of	our sun.
26	stars	"	10,000	"	1,000	"	"	"
1,300	"	"	1,000	"	100	"	"	"
22,000	"	"	100	"	10	"	"	"
140,000	"	"	10	"	1	"	"	"
430,000	"	"	1	"	0.1	"	"	"
650,000	"	"	0.1	"	0.01	"	"	"

THE DENSITY OF STELLAR DISTRIBUTION AT DIFFERENT DISTANCES FROM OUR SUN

Consider, lastly, the distribution of stellar density, that is, the number of stars contained in the unit of volume.

We can not determine *absolute* star-density, because, for example, some of the stars which we know from their measured parallaxes to be comparatively near to us are in themselves so little luminous that if removed to even a few light-years greater distance they would appear fainter than the ninth magnitude, and so fall below the magnitude at which our data at present stop.

But if we assume that intrinsically faint and bright stars are distributed in the

[22] *Publications Astron. Lab. Groningen*, No. 8, p. 23.

[23] *Ibid.*, No. 11, Table II.

same proportion in space, it will be evident that the comparative richness of stars in any part of the system will be the same as the comparative richness of the same part of the system in stars of a particular luminosity. Therefore, as we have already found the arrangement in space of the stars of different degrees of luminosity, and consequently their number at different distances from the sun, we must also be able to determine their relative density for these different distances.

Kapteyn finds in this way that, starting from the sun, the star-density (*i. e.*, the number of stars per unit volume of space) is pretty constant till we reach a distance of some 200 light-years. Thence the density gradually diminishes till, at about 2,500 light-years, it is only about *one fifth* of the density in the neighborhood of the sun.[24] This conclusion must, however, be regarded as uncertain until we have by independent means been enabled to estimate the absorption of light in its course through interstellar space, and obtained proof that the ratio of intrinsically faint to bright stars is constant throughout the universe.

Thus far Kapteyn's researches deal with the stellar universe as a whole; the results, therefore, represent only the *mean* conditions of the system. The further development of our knowledge demands a like study applied to the several portions of the universe separately. This will require much more extensive material than we at present possess.

As a first further approximation the investigation will have to be applied separately to the Milky Way and the parts of the sky of higher galactic latitude. The velocity and direction of the sun's motion in space may certainly be treated as constants for many centuries to come, and these constants may be separately determined from groups of stars of various regions, various magnitudes, various proper motions, and various spectral types. If these constants as thus separately determined are different, the differences which are not attributable to errors of observation must be due to a common velocity or direction of motion of the group or class of star to which the sun's velocity or direction is referred. Thus, for example, the sun's velocity as determined by spectroscopic observations of motion in the line of sight, appears to be sensibly smaller than that derived from fainter stars. The explanation appears to be that certain of the brighter stars form part of a cluster or group of which the sun is a member, and these stars tend to some extent to travel together. For these researches the existing material, especially that of the determination of velocities in the line of sight, is far too scanty.

Kapteyn has found that stars whose proper motions exceed $0''.05$ are not more numerous in the Milky Way than in other parts of the sky;[25] in other words, if only the stars having proper motions of $0''.05$ or upwards were mapped there would be no aggregation of stars showing the existence of a Milky Way.

The proper motions of stars of the second spectral type are, as a rule, considerably larger than those of the first type; but Kapteyn comes to the conclusion that this difference does not mean a real difference of velocity, but only that the second-type stars have a smaller luminosity, the mean difference between the two types amounting to $2\frac{1}{2}$ magnitudes.[26]

THE FUTURE COURSE OF RESEARCH

In the last address delivered from this chair on an astronomical subject, Sir Wil-

[24] *Publications Astron. Lab. Groningen*, No. 11.

[25] *Verl. Kn. Akad. Amsterdam*, January, 1893.

[26] *Ibid.*, April, 1892.

liam Huggins, in 1891, dealt so fully with the chemistry of the stars that it seemed fitting on the present occasion to consider more especially the problem of their motion and distribution in space, as it is in this direction that the most striking advances in our knowledge have recently been made. It is true that since 1891 great advances have also been made in our detailed knowledge of the chemistry of the sun and stars. The methods of astro-spectrography have been greatly improved, the precision of the determination of motion in the line of sight greatly enhanced, and many discoveries made of those close double stars, ordinarily termed spectroscopic doubles, the study of which seems destined to throw illustrative light upon the probable history of the development of systems from the original nebular condition to that of more permanent systems.

But the limitations of available time prevent me from entering more fully into this tempting field, more especially as it seems desirable, in the light of what has been said, to indicate the directions in which some of the astronomical work of the future may be most properly systematized. There are two aspects from which this question may be viewed. The first is the more or less immediate extension of knowledge or discovery; the second the fulfilment of our duty, as astronomers, to future generations. These two aspects should never be entirely separated. The first, as it opens out new vistas of research and improved methods of work, must often serve as a guide to the objects of the second. But the second is to the astronomer the supreme duty, viz., to secure for future generations those data the value of which grows by time.

As the result of the Congress of Astronomers held at Paris in 1887 some sixteen of the principal observatories in the world are engaged, as is well known, in the laborious task, not only of photographing the heavens, but of measuring these photographs and publishing the *relative* positions of the stars on the plates down to the eleventh magnitude. A century hence this great work will have to be repeated, and then, if we of the present day have done our duty thoroughly, our successors will have the data for an infinitely more complete and thorough discussion of the motions of the sidereal system than any that can be attempted to-day. But there is still needed the accurate meridian observation of some eight or ten stars on each photographic plate, so as to permit the conversion of the *relative* star-places on the plate into *absolute* star-places in the heavens. It is true that some of the astronomers have already made these observations for the reference stars of the zones which they have undertaken. But this seems to be hardly enough. In order to coordinate these zones, as well as to give an accuracy to the *absolute* positions of the reference stars corresponding with that of the *relative* positions, it is desirable that this should be done for *all* the reference stars in the sky by several observatories. The observations of well-distributed stars by Kustner at Bonn present an admirable instance of the manner in which the work should be done. Several observatories in each hemisphere should devote themselves to this work, employing the same or other equally efficient means for the elimination of sources of systematic error depending on magnitude, etc., and it is of far more importance that we should have, say, two or three observations of each star at three different observatories than two or three times as many observations of each star made at a single observatory.

The southern can not boast of a richness of instrumental and personal equipment comparable with that of the northern hemisphere, and consequently one welcomes with enthusiasm the proposal on the part of the Carnegie Institution to establish a meridian observatory in a suitable situation in the southern hemisphere. Such an observatory, energetically worked, with due attention to all necessary precautions for the exclusion of systematic errors, would conduce more than anything else to remedy in some degree that want of balance of astronomical effort in the two hemispheres to which allusion has already been made. But in designing the program of the work it should be borne in mind that the proper duty of the meridian instrument in the present day is no longer to determine the positions of all stars down to a given order of magnitude, but to determine the positions of stars which are geometrically best situated and of the most suitable magnitude for measurement on photographic plates, and to connect these with the fundamental stars. For this purpose the working list of such an observatory should include only the fundamental stars and the stars which have been used as reference stars for the photographic plates.

Such a task undertaken by the Carnegie Observatory, by the Cape, and if possible by another observatory in the southern hemisphere, and by three observatories in the northern, would be regarded by astronomers of the future as the most valuable contribution that could be made to astronomy of the present day. Taken in conjunction with the astrographic survey of the heavens now so far advanced, it is an opportunity that if lost can never be made good; a work that would grow in value year by year as time rolls on, and one that would ever be remembered with gratitude by the astronomers of the future.

But for the solution of the riddle of the universe much more is required. Besides the proper motions, which would be derived from the data just described, we need for an ideal solution to know the velocity in the line of sight, the parallax, the magnitude, and the spectrum-type of every star.

The broad distinction between these latter data and the determination of proper motion is this, that whereas the observations for proper motion increase in value as the square of their age, those for velocity in the line of sight, parallax, magnitude, and type of spectrum may, for the broader purposes of cosmical research, be made at any time without loss of value. We should therefore be most careful not to sacrifice the interests of the future by immediate neglect of the former for the latter lines of research. The point is that those observatories which undertake this meridian work should set about it with the least possible delay, and prosecute the program to the end with all possible zeal. Three observatories in each hemisphere should be sufficient; the quality of the work should be of the best, and quality should not be sacrificed for speed of work.

But the sole prosecution of routine labor, however high the ultimate object, would hardly be a healthy condition for the astronomy of the immediate future. The sense of progress is essential to healthy growth, the desire to know must in some measure be gratified. We have to test the work that we have done in order to be sure that we are working on the right lines, and new facts, new discoveries, are the best incentives to work.

For these reasons Kapteyn, in consultation with his colleagues in different parts of the world, has proposed a scheme of research which is designed to afford within a comparatively limited time a great augmentation of our knowledge. The principle on which his program is based is that adequate data as to the proper motions, parallaxes, magnitudes, and the type of spectrum of stars situated in limited but symmetrically distributed areas of the sky, will suffice to determine many of the broader facts of the constitution of the universe. His proposals and methods are known to astronomers and need not therefore be here repeated. In all respects save one these proposals are practical and adequate, and the required cooperation may be said to be already secured—the exception is that of the determination of motion in the line of sight.

All present experience goes to show that there is no known satisfactory method of determining radial velocity of stars by wholesale methods, but that such velocities must be determined star by star. For the fainter stars huge telescopes and spectroscopes of comparatively low dispersion must be employed. On this account there is great need in both hemispheres of a huge reflecting telescope—six to eight feet in aperture—devoted almost exclusively to this research. Such a telescope is already in preparation at Mount Wilson, in America, for use in the northern hemisphere. Let us hope that Professor Pickering's appeal for a large reflector to be mounted in the southern hemisphere will meet with an adequate response, and that it will be devoted there to this all-important work.

CONCLUSION

The ancient philosophers were confident in the adequacy of their intellectual powers alone to determine the laws of human thought and regulate the actions of their fellow men, and they did not hesitate to employ the same unsupported means for the solution of the riddle of the universe. Every school of philosophy was agreed that some object which they could see was a fixed center of the universe, and the battle was fought as to what that center was. The absence of facts, their entire ignorance of methods of exact measurement, did not daunt them, and the question furnished them a subject of dispute and fruitless occupation for twenty-five centuries.

But astronomers now recognize that Bradley's meridian observations at Greenwich, made only one hundred and fifty years ago, have contributed more to the advancement of sidereal astronomy than all the speculations of preceding centuries. They have learned the lesson that human knowledge in the slowly developing phenomena of sidereal astronomy must be content to progress by the accumulating labors 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.

By these means we shall learn more and more of the wonders that surround us, and recognize our limitations when measurement and facts fail us.

Huggins's spectroscope has shown that many nebulæ are not stars at all; that many well-condensed nebulæ, as well as vast patches of nebulous light in the sky, are but inchoate masses of luminous gas. Evidence upon evidence has accumulated to show that such nebulæ consist of the matter out of which stars (*i. e.*, suns) have been and are being evolved. The different types of star spectra form such a complete and gradual sequence (from simple spectra resembling those of nebulæ onwards through types of gradually increasing complexity) as to suggest that we have before us, written in the cryptograms of these spectra, the complete story of the evolution of suns from the inchoate nebula onwards to the most active sun (like our own), and then downward to the almost heatless and invisible ball. The period during which human life has existed on our globe is probably too short—even if our first parents had begun the work—to afford observational proof of such a cycle of change in any particular star; but the fact of such evolution, with the evidence before us, can hardly be doubted. I most fully believe that, when the modifications of terrestrial spectra under sufficiently varied conditions of temperature, pressure, and environment have been further studied, this conclusion will be greatly strengthened. But in this study we must have regard also to the spectra of the stars themselves. The stars are the crucibles of the Creator. There we see matter under conditions of temperature and pressure and environment, the variety of which we can not hope to emulate in our laboratories, and on a scale of magnitude beside which the proportion of our greatest experiment is less than that of the drop to the ocean. The spectroscopic astronomer has to thank the physicist and the chemist for the foundation of his science, but the time is coming—we almost see it now—when the astronomer will repay the debt by wide-reaching contributions to the very fundamenta of chemical science.

By patient, long-continued labor in the minute sifting of numerical results, the grand discovery has been made that a great part of space, so far as we have visible knowledge of it, is occupied by two majestic streams of stars traveling in opposite directions. Accurate and minute measurement has given us some certain knowledge as to the distances of the stars within a certain limited portion of space, and in the cryptograms of their spectra has been deciphered the amazing truth that the stars of both streams are alike in design, alike in chemical constitution, and alike in process of development.

But whence have come the two vast streams of matter out of which have been evolved these stars that now move through space in such majestic procession?

The hundreds of millions of stars that comprise these streams, are they the sole ponderable occupants of space? However vast may be the system to which they belong, that system itself is but a speck in illimitable space; may it not be but one of millions of such systems that pervade the infinite?

We do not know.

"Canst thou by searching find out God? canst thou find out the Almighty unto perfection?"

David Gill

SCIENTIFIC BOOKS

A Text-book of Electro-Chemistry. By Max Le Blanc, Professor in the University of Leipzig. Translated from the Fourth German Edition by W. R. Whitney, Ph.D., Director of the Research Laboratory of the General Electric Company, and John W. Brown, Director of the Research and Battery Laboratory of the National Carbon Company. 8vo, pp. xiv + 338. Price, \$2.60 net. New York: The Macmillan Company.

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