The next ten years were the most active of his life, most of the time being spent in the field. In 1856 he published a "Manual of coal and its topography, illustrated by original drawings, chiefly of facts in the geology of the Appalachian region of the United States," and was appointed secretary of the American iron association. In 1859 appeared his "Iron manufacturer's guide to furnaces, forges, and rolling-mills of the United States, with discussions of iron as a chemical element, an American ore, and a manufactured article, in commerce and in history." Jan. 15, 1858, while examining the iron-works of southern Ohio, Mr. Lesley was elected librarian, and Jan. 7, 1859, one of the four secretaries of the American philosophical society, and continues to hold these offices, which, for the first year or two, withdrew him almost entirely from field-work. From 1860 to 1866 Mr. Lesley was busily employed by capitalists to pronounce upon projected mining-plants, and by mine-owners to examine their properties, the call for iron and coal being great on account of the civil war. In 1864 and 1865 serious illnesses, produced by overwork, prepared the way for a complete breakdown of his nervous system in the early summer of 1866, from which he did not recover until the early winter of 1869, this interval of three years and a half being spent mostly in Europe.

Mr. Lesley went from Italy to the Paris exposition of 1867 as one of the ten commissioners appointed by the United States senate; but his illness steadily increased, and he was compelled to abandon his duties. Not until 1872 could he again do six hours of hard work a day; and a new career of usefulness was opened to him by his appointment, in that year, to the professorship of geology in the new department of science of the University of Pennsylvania, and in 1873 to the directorship of the Second geological survey of the state, still in progress. For four years (1873–78) he performed the duties of both offices, finding his only relaxation in a short voyage to Europe every two years; but a threatening recurrence of his former malady induced him to offer his resignation to the trustees of the university, who, however, preferred to grant him an indefinite furlough, until the close of the geological survey.

With what untiring zeal he has devoted himself to the work of that survey is only known to those who have been associated with him in the work. How successfully he has conducted it, is shown to the world through the seventy volumes recording its progress. If his hundreds of papers, scientific and literary, read before the American philosophical society, had never been published, this great work alone would place him in the front rank of American geologists. Of the personal character of a man whose modesty is his most prominent trait, it is difficult to speak as one would wish during his life.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

PENDING PROBLEMS OF ASTRONOMY.[1]

Thirty-six years ago this very month, in this city, and near the place where we are now assembled, the American association for the advancement of science was organized, and held its first meeting. Now, for the first time, it revisits its honored birthplace.

Few of those present this evening were, I suppose, in attendance upon that first meeting. Here and there, among the members of the association, I see, indeed, the venerable faces of one and another, who, at that time in the flush and vigor of early manhood, participated in its proceedings and discussions; and there are others, who, as boys or youths, looked on in silence, and listening to the words of Agassiz and Peirce, of Bache and Henry, and the Rogers brothers and their associates, drank in that inspiring love of truth and science which ever since has guided and impelled their lives. Probably enough, too, there may be among our hosts in the audience a few who remember that occasion, and were present as spectators.

[1] Address to the American association for the advancement of science at Philadelphia, Sept. 5, 1884, by Prof. C. A. Young, professor of astronomy at Princeton, retiring president of the association.

But, substantially, we who meet here to-day are a new generation, more numerous certainly, and in some respects unquestionably better equipped for our work, than our predecessors were; though we might not care to challenge comparisons as regards native ability, or clearness of insight, or lofty purpose.

And the face of science has greatly changed in the mean time; as much, perhaps, as this great city and the nation. One might almost say, that, since 1848, 'all things have become new' in the scientific world. There is a new mathematics and a new astronomy, a new chemistry and a new electricity, a new geology and a new biology. Great voices have spoken, and have transformed the world of thought and research as much as the material products of science have altered the aspects of external life. The telegraph and dynamo-machine have not more changed the conditions of business and industry than the speculations of Darwin and Helmholtz and their compeers have affected those of philosophy and science.

But, although this return to our birthplace suggests retrospections and comparisons which might profitably occupy our attention for even a much longer time than this evening's session, I prefer, on the whole, to take a different course; looking forwards rather than backwards, and confining myself mainly to topics which lie along the pathway of my own line of work.

The voyager upon the inland sea of Japan sees continually rising before him new islands and mountains of that fairyland. Some come out suddenly from behind nearer rocks or islets, which long concealed the greater things beyond; and some are veiled in clouds which give no hint of what they hide, until a breeze rolls back the curtain; some, and the greatest of them all, are first seen as the minutest specks upon the horizon, and grow slowly to their final grandeur. Even before they reach the horizon line, while yet invisible, they sometimes intimate their presence by signs in sky and air; so slight, indeed, that only the practised eye of the skilful sailor can detect them, though quite obvious to him.

Somewhat so, as we look forward into the future of a science, we see new problems and great subjects presenting themselves. Some are imminent and in the way, — they must be dealt with at once, before further progress can be made; others are more remotely interesting in various degrees; and some, as yet, are mere suggestions, almost too misty and indefinite for steady contemplation.

With your permission, I propose this evening to consider some of the pending problems of astronomy, — those which seem to be most pressing, and most urgently require solution as a condition of advance; and those which appear in themselves most interesting, or likely to be fruitful, from a philosophic point of view.

Taking first those that lie nearest, we have the questions which relate to the dimensions and figure of the earth, the uniformity of its diurnal rotation, and the constancy of its poles and axis.

I think the impression prevails, that we already know the earth's dimensions with an accuracy even greater than that required by any *astronomical* demands. I certainly had that impression myself not long ago, and was a little startled on being told by the superintendent of our Nautical almanac that the remaining uncertainty was still sufficient to produce serious embarrassment in the reduction and comparison of certain lunar observations. The length of the line joining, say, the Naval observatory at Washington with the Royal observatory at the Cape of Good Hope, is doubtful; not to the extent of only a few hundred feet, as commonly supposed, but the uncertainty amounts to some thousands of feet, and may possibly be a mile or more, probably not less than a ten-thousandth of the whole distance; and the *direction* of the line is uncertain in about the same degree. Of course, on those portions of either continent which have been directly connected with each other by geodetic triangulations, no corresponding uncertainty obtains; and as time goes on, and these surveys are extended, the form and dimensions of each continuous land-surface will become more and more perfectly determined. But at present we have no satisfactory means of obtaining the desired accuracy in the relative position of places separated by oceans, so that they cannot be connected by chains of triangulation. Astronomical determinations of latitude and longitude do not meet the case; since, in the last analysis, they only give at any selected station the *direction of gravity* relative to the axis of the earth, and some fixed meridian plane, and do not furnish any *linear* measurement or dimension.

Of course, if the surface of the earth were an exact spheroid, and if there were no irregular attractions due to mountains and valleys and the varying density of strata, the difficulty could be easily evaded; but, as the matter stands, it looks as if nothing short of a complete geodetic triangulation of the whole earth would ever answer the purpose, — a triangulation covering Asia and Africa, as well as Europe, and brought into America by way of Siberia and Bering Strait.

It is indeed theoretically possible, and just conceivable, that the problem may some day be reversed, and that the geodesist may come to owe some of his most important data to the observers of the lunar motions. When the relative position of two or more remote observatories shall have been precisely determined by triangulation (for instance, Greenwich, Madras, and the Cape of Good Hope), and when, by improved methods and observations made at these fundamental stations, the moon's position and motion relative to them shall have been determined with an accuracy much exceeding any thing now attainable, then by similar observations, made simultaneously at any station in this hemisphere, it will be theoretically possible to determine the position of this station, and so, by way of the moon, to bridge the ocean, and ascertain how other stations are related to those which were taken as primary. I do not, of course, mean to imply, that, in the *present state* of observational astronomy, any such procedure would lead to results of much value; but, before the Asiatic triangulation meets the American at Bering Strait, it is not unlikely that the accuracy of lunar observations will be greatly increased.

The present uncertainty as to the earth's dimensions is not, however, a sensible embarrassment to astronomers, except in dealing with the moon, especially in attempting to employ observations made at remote and ocean-separated stations for the determination of her parallax.

As to the form of the earth, it seems pretty evident that before long it will be wise to give up further attempts to determine exactly what spheroid or ellipsoid *most nearly corresponds* to the actual figure of the earth; since every new continental survey will require a modification of the elements of this spheroid in order to take account of the new data. It will be better to assume some closely approximate spheroid *as a finality*; its elements to be forever retained unchanged, while the deviations of the actual surface from this ideal standard will be the subject of continued investigation and measurement.

A more important and anxious question of the modern astronomer is, Is the earth's rotation uniform, and, if not, in what way and to what extent does it vary? The importance, of course, lies in the fact that this rotation furnishes our fundamental measure and unit of time.

Up to a comparatively recent date, there has not been reason to suspect this unit of any variation sufficient to be detected by human observation. It has long been perceived, of course, that any changes in the earth's form or dimensions must alter the length of the day. The displacement of the surface or strata by earthquakes or by more gradual elevation and subsidence, the transportation of matter towards or from the equator by rivers or ocean currents, the accumulation or removal of ice in the polar regions or on mountain-tops, — any such causes must necessarily produce a real effect. So, also, must the friction of tides and trade-winds. But it has been supposed that these effects were so minute, and to such an extent mutually compensatory, as to be quite beyond the reach of observation; nor is it yet certain that they are not. All that can be said is, that it is now beginning to be *questionable* whether they are, or are not.

The reason for suspecting perceptible variation in the earth's revolution, lies mainly in certain unexplained irregularities in the apparent motions of the moon. She alone, of all the heavenly bodies, changes her place in the sky so rapidly, that minute inaccuracies of a second or two in the time of observation would lead to sensible discrepancies in the observed position; an error of one second, in the time, corresponding to about half a second in her place, — a quantity minute, certainly, but perfectly observable. No other heavenly body has an apparent movement anywhere nearly as rapid, excepting only the inner satellite of Mars; and this body is so minute that its accurate observation is impracticable, except with the largest telescopes, and at the times when Mars is unusually near the earth.

Now, of late, the motions of the moon have been very carefully investigated, both theoretically and observationally; and, in spite of every thing, there remain discrepancies which defy explanation. We are compelled to admit one of three things, — either the lunar theory is in some degree mathematically incomplete, and fails to represent accurately the gravitational action of the earth and sun, and other known heavenly bodies, upon her movements; or some unknown force other than the gravitational attractions of these bodies is operating in the case; or else, finally, the earth's rotational motion is more or less irregular, and so affects the time-reckoning, and confounds prediction.

If the last is really the case, it is in some sense a most discouraging fact, necessarily putting a limit to the accuracy of all prediction, unless some other unchanging and convenient measure of time shall be found to replace the 'day' and 'second.'

The question at once presents itself, How can the constancy of the day be tested? The lunar motions furnish grounds of suspicion, but nothing more; since it is at least as likely that the mathematical theory is minutely incorrect or incomplete as that the day is sensibly variable.

Up to the present time, the most effective tests suggested are from the transits of Mercury and from the eclipses of Jupiter's satellites. On the whole, the result of Professor Newcomb's elaborate and exhaustive investigation of all the observed transits, together with all the available eclipses and occultations of stars, tends rather to establish the sensible constancy of the day, and to make it pretty certain (to use his own language) that "inequalities in the lunar motions, not accounted for by the theory of gravitation, really exist, and in such a way that the mean motion of the moon between 1800 and 1875 was really less (i.e., slower) than between 1720 and 1800." Until lately, the observations of Jupiter's satellites have not been made with sufficient accuracy to be of any use in settling so delicate a question; but at present the observation of their eclipses is being carried on at Cambridge, Mass., and elsewhere, by methods that promise a great increase of accuracy over any thing preceding. Of course, no *speedy* solution of the problem is possible through such observations, and their result will not be so free from mathematical complications as desirable, — complications arising from the mutual action of the satellites, and the ellipsoidal form of the planet. On account of its freedom from all sensible disturbances, the remote and lonely satellite of Neptune may possibly some time contribute useful data to the problem.

We have not time, and it lies outside my present scope, to discuss whether, and, if so, how, it may be possible to find a unit of time (and length) which shall be independent of the earth's conditions and dimensions (free from all *local considerations*), cosmical, and as applicable in the planetary system of the remotest star as in our own. At present we can postpone its consideration; but the time must unquestionably come, when the accuracy of scientific observation will be so far increased, that the irregularities of the earth's rotation, produced by the causes alluded to a few minutes ago, will protrude, and become intolerable. Then a new unit of time will have to be found for scientific purposes, founded, perhaps, as has been already suggested by many physicists, upon the vibrations or motion of light, or upon some other physical action which pervades the universe.

Another problem of terrestrial astronomy relates to the constancy of the position of the earth's axis in the globe. Just as displacements of matter upon the surface or in the interior of the earth would produce changes in the time of rotation, so also would they cause corresponding alterations in the position of the axis and in the places of the poles, — changes certainly very minute. The only question is, whether they are so minute as to defy detection. It is easy to see that any such displacements of the earth's axis will be indicated by changes in the *latitudes* of our observatories. If, for instance, the pole were moved a hundred feet from its present position, towards the continent of Europe, the latitudes of European observatories would be increased about one second, while in Asia and America the effects would be trifling.

The only observational evidence of such movements of the pole, which thus far amounts to any thing, is found in the results obtained by Nyren in reducing the determinations of the latitude of Pulkowa, made with the great vertical circle, during the last twenty-five years. They seem to show a slow, steady diminution of the latitude of this observatory, amounting to about a second in a century; as if the north pole were drifting away, and increasing its distance from Pulkowa at the rate of about one foot a year.

The Greenwich and Paris observations do not show any such result; but they are not conclusive, on account of the difference of longitude, to say nothing of their inferior precision. The question is certainly a doubtful one; but it is considered of so much importance, that, at the meeting of the International geodetic association in Rome last year, a resolution was adopted recommending observations specially designed to settle it. The plan of Sig. Fergola, who introduced the resolution, is to select pairs of stations, having nearly the same latitude, but differing widely in longitude, and to determine the *difference* of their latitudes by observations of the same set of stars, observed with similar instruments, in the same manner, and reduced by the same methods and formulae. So far as possible, the same observers are to be retained through a series of years, and are frequently to exchange stations when practicable, so as to eliminate personal equations. The main difficulty of the problem lies, of course, in the minuteness of the effect to be detected; and the only hope of success lies in the most scrupulous care and precision in all the operations involved.

Other problems, relating to the rigidity of the earth and its internal constitution and temperature, have, indeed, astronomical bearings, and may be reached to some extent by astronomical methods and considerations; but they lie on the border of our science, and time forbids any thing more than their mere mention here.

If we consider, next, the problems set us by the moon, we find them numerous, important, and difficult. A portion of them are purely mathematical, relating to her orbital motion; while others are physical, and have to do with her surface, atmosphere, heat, etc.

As has been already intimated, the lunar theory is not in a satisfactory state. I do not mean, of course, that the moon's deviations from the predicted path are gross and palpable, — such, for instance, as could be perceived by the unaided eye (this I say for the benefit of those who otherwise might not understand how small a matter sets astronomers to grumbling); but they are large enough to be easily observable, and even obtrusive, amounting to several seconds of arc, or miles of space. As we have seen, the attempt to account for them by the irregularity of the earth's rotation has apparently failed; and we are driven to the conclusion, either that other forces than gravitation are operative upon the lunar motions, or else (what is far more probable, considering the past history of theoretical astronomy) that the mathematical theory is somewhere at fault.

To one looking at the matter a little from the outside, it seems as if that which is most needed just now, in order to secure the advance of science in many directions, is a new, more comprehensive, and more manageable solution of the fundamental equations of motion under attraction. Far be it from me to cry out against those mathematicians who delight themselves in transcendental and n-dimensional space, and revel in the theory of numbers, — we all know how unexpectedly discoveries and new ideas belonging to one field of science find use and application in widely different regions, — but I own, I feel much more interest in the study of the theory of functions and differential equations, and expect more aid for astronomy from it.

The problem of any number of bodies, moving under their mutual attraction, according to the Newtonian laws, stands, from a physical point of view, on precisely the same footing as that of *two* bodies. Given the masses, and the positions and velocities corresponding to any moment of time, then the whole configuration of the system for all time, past and future (abstracting outside forces, of course), is absolutely determinate, and amenable to calculation. But while, in the case of *two* bodies, the calculation is easy and feasible, by methods known for two hundred years, our analysis has not yet mastered the general problem for more than two. In special instances, by computations, tedious, indirect, and approximate, we can, indeed, carry our predictions forward over long periods, or indicate past conditions with any required degree of accuracy; but a general and universally practicable solution is yet wanting. The difficulties in the way are purely mathematical: a step needs to be taken, corresponding in importance to the introduction of the circular functions, into trigonometry, the invention of logarithms, or the discovery of the calculus. The problem confronts the astronomer on a hundred different roads; and, until it is overcome, progress in these directions must be slow and painful. One could not truly say, perhaps, that the lunar theory must, in the mean while, remain quite at a standstill: labor expended in the old ways, upon the extension and development of existing methods, may not be fruitless, and may, perhaps, after a while, effect the reconcilement of prediction and observation far beyond the present limits of accuracy. But if we only had the mathematical powers we long for, then progress would be as by wings: we should fly, where now we crawl.

As to the physical problems presented by the moon, the questions relating to the light and heat — the radiant energy — it sends us, and to its temperature, seem to be the most attractive at present, especially for the reason that the results of the most recent investigators seem partially to contradict those obtained by their predecessors some years ago. It now looks as if we should have to admit that nearly all we receive from the moon is simply *reflected* sun-light and sun-heat, and that the temperature of the lunar surface nowhere rises as high as the freezing-point of water, or even of mercury. At the same time, some astronomers of reputation are not disposed to admit such an upsetting of long-received ideas; and it is quite certain, that, in the course of the next few years, the subject will be carefully and variously investigated.

Closely connected with this is the problem of a lunar atmosphere — if, indeed, she has any.

Then there is the very interesting discussion concerning changes upon the moon's surface. Considering the difference between our modern telescopes and those employed fifty or a hundred years ago, I think it still far from certain that the differences between the representations of earlier and later observers necessarily imply any real alterations. But they, no doubt, render it considerably *probable* that such alterations have occurred, and are still in progress; and they justify a persistent, careful, minute, and thorough study of the details of the lunar surface with powerful instruments: especially do they inculcate the value of large-scale photographs, which can be preserved for future comparison as unimpeachable witnesses.

I will not leave the moon without a word in respect to the remarkable speculations of Professor George Darwin concerning the tidal evolution of our satellite. Without necessarily admitting all the numerical results as to her age and her past and future history, one may certainly say that he has given a most plausible and satisfactory explanation of the manner in which the present state of things might have come about through the operation of causes known and recognized, has opened a new field of research, and shown the way to new dominions. The introduction of the doctrine of the conservation of energy, as a means of establishing the conditions of motion and configuration in an astronomical system, is a very important step.

In the planetary system we meet, in the main, the same problems as those that relate to the moon, with a few cases of special interest.

For the most part, the accordance between theory and observation in the motions of the larger planets is as close as could be expected. The labors of Leverrier, Hill, Newcomb, and others, have so nearly cleared the field, that it seems likely that several decades will be needed to develop discrepancies sufficient to furnish any important corrections to our present tables. Leverrier himself, however, indicated one striking and significant exception to the general tractableness of the planets. Mercury, the nearest to the sun, and the one, therefore, which ought to be the best behaved of all, is rebellious to a certain extent: the perihelion of its orbit moves around the sun more rapidly than can be explained by the action of the other known planets. The evidence to this effect has been continually accumulating ever since Leverrier first announced the fact, some thirty years ago; and the recent investigation by Professor Newcomb, of the whole series of observed transits, puts the thing beyond question. Leverrier's own belief (in which he died) was, that the effect is due to an unknown planet or planets between Mercury and the sun; but, as things now stand, we think that any candid investigator must admit that the probability of the existence of any such body or bodies of considerable dimensions is vanishingly small. We do not forget the numerous instances of round spots seen on the solar disk, nor the eclipse-stars of Watson, Swift, Trouvelot, and others; but the demonstrated possibility of error or mistake in all these cases, and the tremendous array of negative evidence from the most trustworthy observers, with the best equipment and opportunity, makes it little short of certain that there is no Vulcan in the planetary system.

A ring of meteoric matter between the planet and the sun might account for the motion of the perihelion; but, as Newcomb has suggested, such a ring would also disturb the *nodes* of Mercury's orbit.

It has been surmised that the cause may be something in the distribution of matter within the solar globe, or some variation in gravitation from the exact law of the inverse square, or some supplementary electric or magnetic action of the sun, or some special effect of the solar radiation, sensible on account of the planet's proximity, or something peculiar to the region in which the planet moves; but as yet no satisfactory explanation has been established.

Speaking of unknown planets, we are rather reluctantly obliged to admit that it is a part of our scientific duty as astronomers to continue to search for the remaining asteroids; at least, I suppose so, although the family has already become embarrassingly large. Still I think we are likely to learn as much about the constitution, genesis, and history of the solar system from these little flying rocks as from their larger relatives; and the theory of perturbations will be forced to rapid growth in dealing with the effects of Jupiter and Saturn upon their motions.

Nor is it unlikely that some day the searcher for these insignificant little vagabonds may be rewarded by the discovery of some great world, as yet unknown, slow moving in the outer desolation beyond the remotest of the present planetary family. Some configurations in certain cometary orbits, and some almost evanescent peculiarities in Neptune's motions, have been thought to point to the existence of such a world; and there is no evidence, nor even a presumption, against it.

Mercury as yet defies all our attempts to ascertain the length of its day, and the character and condition of its surface. Apparently the instruments and methods now at command are insufficient to cope with the difficulties of the problem; and it is not easy to say how it can be successfully attacked.

With Venus, the earth's twin-sister, the state of things is a little better: we do already know, with some degree of approximation, her period of rotation; and the observations of the last few months bid fair, if followed up, to determine the position of her poles, and possibly to give us some knowledge of her mountains, continents, and seas.

It would be rash to say of Mars that we have reached the limit of possible knowledge as regards a planet's surface; but the main facts are now determined, and we have a rather surprising amount of supposed knowledge regarding his geography. By 'supposed' I mean merely to insinuate a modest doubt whether some of the map-makers have not gone into a little more elaborate detail than the circumstances warrant. At any rate, while the 'areographies' agree very well with each other in respect to the planet's more important features, they differ widely and irreconcilably in minor points.

As regards the physical features of the asteroids, we at present know practically nothing: the field is absolutely open. Whether it is worth any thing may be a question; and yet, if one *could* reach it, I am persuaded that a knowledge of the substance, form, density, rotation, temperature, and other physical characteristics, of one of these little orphans, would throw vivid light on the nature and behavior of interplanetary space, and would be of great use in establishing the physical theory of the solar system.

The planet Jupiter, lordliest of them all, still, as from the first, presents problems of the highest importance and interest. A sort of connecting-link between suns and planets, it seems as if, perhaps, we might find, in the beautiful and varied phenomena he exhibits, a kind of halfway house between familiar terrestrial facts and solar mysteries. It seems quite certain that no analogies drawn from the earth and the earth's atmosphere alone will explain the strange things seen upon his disk, some of which, especially the anomalous differences observed between the rotation periods derived from the observation of markings in different latitudes, are very similar to what we find upon the sun. 'The great red spot' which has just disappeared, after challenging for several years our best endeavors to understand and explain it, still, I think, remains as much a mystery as ever, — a mystery probably hiding within itself the master-key to the constitution of the great orb of whose inmost nature it was an outward and most characteristic expression. The same characteristics are also probably manifested in other less conspicuous but equally curious and interesting markings on the varied and ever-changing countenance of this planet; so that, like the moon, it will well repay the most minute and assiduous study.

Its satellite system also deserves careful observation, especially in respect to the eclipses which occur; since we find in them a measure of the time required for light to cross the orbit of the earth, and so of the solar parallax, and also because, as has been already mentioned, they furnish a test of the constancy of the earth's rotation. The photometric method of observing these eclipses, first instituted by Professor Pickering at Cambridge in 1878, and since re-invented by Cornu in Paris, has already much increased the precision of the results.

With reference to the mathematical theory of the motion of these satellites, the same remarks apply as to the planetary theory. As yet nothing appears in the problem to be beyond the power and scope of existing methods, when carried out with the necessary care and prolixity; but a new and more compendious method is most desirable.

The problems of Saturn are much the same as those of Jupiter, excepting that the surface and atmospheric phenomena are less striking, and more difficult of observation. But we have, in addition, the wonderful rings, unique in the heavens, the loveliest of all telescopic objects, the type and pattern, I suppose, of world-making, in actual progress before our eyes. There seems to be continually accumulating evidence from the observations of Struve, Dawes, Henry, and others, that these whirling clouds are changing in their dimensions and in the density of their different parts; and it is certainly the duty of every one who has a good telescope, a sharp eye, and a chastened imagination, to watch them carefully, and set down exactly what he sees. It may well be that even a few decades will develop most important and instructive phenomena in this gauzy girdle of old Chronos. Great care, however, is needed in order not to mistake fancies and illusions for solid facts. Not a few anomalous appearances have been described and commented on, which failed to be recognized by more cautious observers with less vivid imaginations, more trustworthy eyes, and better telescopes.

The outer planets, Uranus and Neptune, until recently, have defied all attempts to study their surface and physical characteristics. Their own motions and those of their satellites, have been well worked out; but it remains to discuss their rotation, topography, and atmospheric peculiarities. So remote are they, and so faintly illuminated, that the task seems almost hopeless; and yet, within the last year or two, some of our great telescopes have revealed faint and evanescent markings upon Uranus, which may in time lead to some further knowledge of that far-off relative. It may, perhaps, be that some great telescope of the future will give us some such views of Neptune as we now get of Jupiter.

There is a special reason for attempts to determine the rotation periods of the planets, in the fact that there is very possibly some connection between these periods, on the one hand, and, on the other, the planets' distances from the sun, their diameters and masses. More than thirty years ago, Professor Kirkwood supposed that he had discovered the relation in the analogy which bears his name. The materials for testing and establishing it were then, however, insufficient, and still remain so, leaving far too many of the data uncertain and arbitrary. Could such a relation be discovered, it could hardly fail to have a most important significance with respect to theories of the origin and development of the planetary system.

The great problem of the absolute dimensions of our system is, of course, commanded by the special problem of the solar parallax; and this remains a problem still. Constant errors of one kind or another, the origin of which is still obscure, seem to affect the different methods of solution. Thus, while experiments upon the velocity of light and heliometric measurements of the displacements of Mars among the stars agree remarkably in assigning a smaller parallax (and greater distance of the sun) than seems to be indicated by the observations of the late transits of Venus, and by methods founded on the lunar motions, on the other hand, the meridian observations of Mars all point to a larger parallax and smaller distance. While still disposed to put more confidence in the methods first named, I, for one, must admit that the margin of probable error seems to me to have been rather increased than diminished by the latest published results deduced from the transits. I do not feel so confident of the correctness of the value $8''.80$ for the solar parallax as I did three years ago. In its very nature, this problem is one, however, that astronomers can never have done with. So fundamental is it, that the time will never come when they can properly give up the attempt to increase the precision of their determination, and to test the received value by every new method that may be found.

The problems presented by the sun alone might themselves well occupy more than the time at our disposal this evening. Its mass, dimensions, and motions, as a whole, are, indeed, pretty well determined and understood; but when we come to questions relating to its constitution, the cause and nature of the appearances presented upon its surface, the periodicity of its spots, its temperature, and the maintenance of its heat, the extent of its atmosphere, and the nature of the corona, we find the most radical differences of opinion.

The difficulties of all solar problems are, of course, greatly enhanced by the enormous difference between solar conditions and the conditions attainable in our laboratories. We often reach, indeed, similarity sufficient to establish a bond of connection, and to afford a basis for speculation; but the dissimilarity remains so great as to render quantitative calculations unsafe, and make positive conclusions more or less insecure. We can pretty confidently infer the presence of iron and hydrogen and other elements in the sun by appearances which we can reproduce upon the earth; but we cannot safely apply empirical formulae (like that of Dulong and Petit, for instance), deduced from terrestrial experiments, to determine solar temperatures: such a proceeding is an unsound and unwarrantable extrapolation, likely to lead to widely erroneous conclusions.

For my own part, I feel satisfied as to the substantial correctness of the generally received theory of the sun's constitution, which regards this body as a great ball of intensely heated vapors and gases, clothed outwardly with a coat of dazzling clouds formed by the condensation of the less volatile substances into drops and crystals like rain and snow. Yet it must be acknowledged that this hypothesis is called in question by high authorities, who maintain, with Kirchoff and Zöllner, that the visible photosphere is no mere layer of clouds, but either a solid crust, or a liquid ocean of molten metals; and there may be some who continue to hold the view of the elder Herschel (still quoted as authoritative in numerous school-books), that the central core of the sun is a solid and even habitable globe, having the outer surface of its atmosphere covered with a sheet of flame maintained by some action of the matter diffused in the space through which the system is rushing. We must admit that the question of the sun's constitution is not yet beyond debate.

And not only the constitution of the sun itself, but the nature and condition of the matter composing it, is open to question. Have we to do with iron and sodium and hydrogen as we know them on the earth, or are the solar substances in some different and more elemental state?

However confident many of us may be as to the general theory of the constitution of the sun, very few, I imagine, would maintain that the full explanation of sun-spots and their behavoir has yet been reached. We meet continually with phenomena, which, if not really contradictory to prevalent ideas, at least do not find in them an easy explanation.

So far as mere visual appearances are concerned, I think it must be conceded, that the most natural conception is that of a dark chip or scale thrown up from beneath, like scum in a caldron, and floating, partly submerged, in the blazing flames of the photosphere which overhang its edges, and bridge across it, and cover it with filmy veils, until at last it settles down again and disappears. It hardly *looks* like a mere hollow filled with cooler vapor, nor is its appearance that of a cyclone seen from above. But then, on the other hand, its spectrum under high dispersion is very peculiar; not at all that of a solid, heated slag, but it is made up of countless fine dark lines, packed almost in contact, showing, however, here and there, a bright line, or at least an interspace where the rank is broken by an interval wider than that which elsewhere separates the elementary lines,—a spectrum, which, so far as I know, has not yet found an analogue in any laboratory experiment. It seems, however, to belong to the type of absorption spectra, and to indicate, as the accepted theory requires, that the spot is dark in consequence of *loss* of light, and not from any original defect of luminosity. Here, certainly, are problems that require solution.

The problem of the sun's peculiar rotation and equatorial acceleration is a most important one and still unsolved. Probably its solution depends in some way upon a correct understanding of the exchanges of matter going on between the interior and the surface of the fluid, cooling globe. It is a significant fact (already alluded to), that a similar relation appears to hold upon the disk of Jupiter; the bright spots near the equator of the planet completing their rotation about five minutes more quickly than the great red spot which was forty degrees from the equator. It is hardly necessary to say that an astronomer, watching our terrestrial clouds from some external station (on the moon, for instance), would observe just the reverse. Equatorial clouds would complete their revolution more slowly than those in our own latitude. *Our* storms travel toward the east, while the volcanic dust from Krakatoa moved swiftly west. We may at least conjecture that the difference between different planets somehow turns upon the question, whether the body whose atmospheric currents we observe is receiving more heat from without than it is throwing off itself. Whatever may be the true explanation of this peculiarity in the motion of sun-spots, it will, when reached, probably carry with it the solution of many other mysteries, and will arbitrate conclusively between rival hypotheses.

The periodicity of the sun-spots suggests a number of important and interesting problems; relating, on the one hand, to its mysterious cause, and, on the other, to the possible effects of this periodicity upon the earth and its inhabitants. I am no 'sun-spottist' myself, and am more than sceptical whether the terrestrial influence of sun-spots amounts to any thing worth speaking of, except in the direction of magnetism. But all must concede that this is by no means yet demonstrated (it is not easy to prove a negative); and there certainly are facts and presumptions enough tending the other way to warrant more extended investigation of the subject. The investigation is embarrassed by the circumstance, pointed out by Dr. Gould, that the effects of sun-spot periodicity, if they exist at all (as he maintains they do), are likely to be quite different in different portions of the earth. The influence of changes in the amount of the solar radiation will, he says, be first and chiefly felt in alterations and deflections of the prevailing winds, thus varying the *distribution* of heat and rain upon the surface of the earth without necessarily much changing its absolute amount. In some regions it may, therefore, be warmer and dryer during a sun-spot maximum, while in adjoining countries it is the reverse.

There can be no question, that it is now one of the most important and pressing problems of observational astronomy to devise apparatus and methods delicate enough to enable the student to follow promptly and accurately the presumable changes in the daily, even the hourly, amounts of the solar radiation. It might, perhaps, be possible with existing instruments to obtain results of extreme value from observations kept up with persistence and scrupulous care for several years at the top of some rainless mountain, if such can be found; but the undertaking would be a difficult and serious affair, quite beyond any private means.

Related to this subject is the problem of the connection between the activity of the solar surface and magnetic disturbances on the earth, — a connection unquestionable as matter of fact, but at present unexplained as matter of theory. It may have something to do with the remarkable prominence of iron in the list of solar materials; or the explanation may, perhaps, be found in the mechanism by means of which the radiations of light and heat traverse interplanetary space, presenting itself ultimately as a corollary of the perfected electro-magnetic theory of light.

The chromosphere and prominences present several problems of interest. One of the most fruitful of them relates to the spectroscopic phenomena at the base of the chromosphere, and especially to the strange differences in the behavior of different spectrum-lines, which, according to terrestrial observations, are due to the same material. Of two lines (of iron, for instance) side by side in the spectrum, one will glow and blaze, while the other will sulk in imperturbable darkness; one will be distorted and shattered, presumably by the swift motion of the iron vapor to which it is due, while the other stands stiff and straight.

Evidently there is some deep-lying cause for such differences; and as yet no satisfactory explanation appears to me to have been reached, though much ingenious speculation has been expended upon it. Mr. Lockyer's bold and fertile hypothesis, already alluded to, that at solar and stellar temperatures our elements are decomposed into others more elemental yet, seems to have failed of demonstration thus far, and rather to have lost ground of late; and yet one is almost tempted to say, 'It *ought* to be true,' and to add that there is more than a possibility that its essential truth will be established some time in the future.

Probably all that can be safely said at present is, that the spectrum of a metallic vapor (iron, for instance, as before) depends not only upon the chemical element concerned, but also upon its physical conditions; so that, at different levels in the solar atmosphere, the spectrum of the iron will differ greatly as regards the relative conspicuousness of different lines; and so it will happen, that, whenever any mass of iron vapor is suffering disturbance, those lines only which particularly characterize the spectrum of iron in that special state will be distorted or reversed, while all their sisters will remain serene.

The problem of the solar corona is at present receiving much attention. The most recent investigations respecting it — those of Dr. Huggins and Professor Hastings — tend in directions which appear to be diametrically opposite. Dr. Huggins considers that he has succeeded in photographing the corona in full sunshine, and so in establishing its objective reality as an immense solar appendage, sub-permanent in form, and rotating with the globe to which it is attached. One may call it 'an atmosphere,' if the word is not to be too rigidly interpreted. I am bound to say that plates which he has obtained do really show just such appearances as would be produced by such a solar appendage, though they are very faint and ghost-like. I may add further, that, from a letter from Dr. Huggins, recently received, I learn that he has been prevented from obtaining any similar plates in England this summer by the atmospheric haze, but that Dr. Woods, who has been provided with a similar apparatus, and sent to the Riffelberg in Switzerland, writes that he has 'an assured success.'

Our American astronomer, on the other hand, at the last eclipse (in the Pacific Ocean), observed certain phenomena which seem to confirm a theory he had formulated some time ago, and to indicate that the lovely apparition is an apparition only, a purely optical effect due to the *diffraction* (not *refraction*, nor reflection either) of light at the edge of the moon — no more a solar appendage than a rainbow or a mock sun. There are mathematical considerations connected with the theory which may prove decisive when the paper of its ingenious and able proposer comes to be published in full. In the mean time it must be frankly conceded that the observations made by him are very awkward to explain on any other hypothesis.

Whatever may be the result, the investigation of the status and possible extent of a nebulous envelope around a sun or a star is unquestionably a problem of very great interest and importance. We shall be compelled, I believe, as in the case of comets, to recognize other forces than gravity, heat, and ordinary gaseous elasticity, as concerned in the phenomena. As regards the actual existence of an extensive gaseous envelope around the sun, I may add that other appearances than those seen at an eclipse seem to demonstrate it beyond question, — phenomena such as the original formation of clouds of incandescent hydrogen at high elevations, and the forms and motions of the loftiest prominences.

But of all solar problems, the one which excites the deepest and most general interest is that relating to the solar heat, its maintenance and its duration. For my own part, I find no fault with the solution proposed by Helmholtz, who accounts for it mainly by the slow contraction of the solar sphere. The only objection of much force is, that it apparently limits the past duration of the solar system to a period not much exceeding some twenty millions of years; and many of our geological friends protest against so scanty an allowance. The same theory would give us, perhaps, half as much time for our remaining lifetime; but this is no objection, since I perceive no reason to doubt the final cessation of the sun's activity, and the consequent death of the system. But while this hypothesis seems fairly to meet the requirements of the case, and to be a necessary consequence of the best knowledge we can obtain as to the genesis of our system and the constitution of the sun itself, it must, of course, be conceded that it does not yet admit of any observational verification. No measurements within our power can test it, so far as we can see at present.

It may be admitted, too, that much can be said in favor of other theories; such as the one which attributes the solar heat to the impact of meteoric matter, and that other most interesting and ingenious theory of the late Sir William Siemens.

As regards the former, however, I see no escape from the conclusion, that, if it were exclusively true, the earth ought to be receiving, as was pointed out by the late Professor Peirce, as much heat from meteors as from the sun. This would require the fall of a quantity of meteoric matter, — more than sixty million times as much as the best estimates make our present supply, and such as could not escape the most casual observation, since it would amount to more than a hundred and fifty[1] tons a day on every square mile.

As regards the theory of Siemens, the matter has been, of late, so thoroughly discussed, that we probably need spend no time upon it here. To say nothing as to the difficulties connected with the establishment of such a far-reaching vortex as it demands, nor of the fact that the temperature of the sun's surface appears to be above that of the dissociation point of carbon compounds, and hence above the highest heat of their combustion, it seems certainly demonstrated, that matter of the necessary density could not exist in interplanetary space without seriously affecting the planetary motions by its gravitating action as well as by its direct resistance; nor could the stellar radiations reach us, as they do, through a medium capable of taking up and utilizing the rays of the sun in the way this theory supposes.

And yet I imagine that there is a very general sympathy with the feeling that led to the proposal of the theory, — an uncomfortable dissatisfaction with received theories, because they admit that the greater part of the sun's radiant energy is, speaking from a scientific point of view, simply wasted. Nothing like a millionth part of the sky, as seen from the sun, is occupied, so far as we can make out, by objects upon which its rays can fall: the rest is vacancy. If the sun sends out rays in all directions alike, not one of them in a million finds a target, or accomplishes any useful work, unless there is in space some medium to utilize the rays, or unknown worlds of which we have no cognizance beyond the stars.

Now, for my own part, I am very little troubled by accusations of wastefulness against nature, or by demands for theories which will show what the human mind can recognize as 'use' for all energy expended. Where I can perceive such use, I recognize it with reverence and gratitude, I hope; but the

[1] In an article on astronomical collisions, published in the *North-American review* about a year ago, I wrongly stated the amount at fifty tons. There was some fatality connected with my calculations for that article. I gave the amount of heat due to the five hundred tons of meteoric matter which is supposed to fall daily on the earth with an average velocity of fifteen miles per second as fifty-three calories annually per square metre, — a quantity two thousand times too great. Probably the error would have been noticed if even the number given had not been so small, compared with the solar heat, as fully to justify my argument, which is only strengthened by the correction. I owe the correction to Professor LeConte of California, who called my attention to the errors.

failure to recognize it in other cases creates in my mind no presumption against the wisdom of nature, or against the correctness of an hypothesis otherwise satisfactory. It merely suggests human limitations and ignorance. How can one without sight understand what a telescope is good for?

At the same time, perhaps we assume with a little too much confidence, that, in free space, radiation does take place equally in all directions. Of course, if the received views as to the nature and conduct of the hypothetical 'ether' are correct, there is no possibility of questioning the assumption; but, as Sir John Herschel and others have pointed out, the properties which must be ascribed to this 'ether,' to fit it for its various functions, are so surprising and almost inconceivable, that one may be pardoned for some reserve in accepting it as a finality. At any rate, as a fact, the question is continually started (the idea has been brought out repeatedly, in some cases by men of real and recognized scientific and philosophic attainment), whether the constitution of things may not be such that radiation and transfer of energy can take place only between ponderable masses; and that, too, without the expenditure of energy upon the transmitting-agent (if such exist) along the line of transmission, even *in transitu*. If this were the case, then, the sun would send out its energy only to planets and meteors and sister-stars, wasting none in empty space; and so its loss of heat would be enormously diminished, and the time-scale of the life of the planetary system would be correspondingly extended. So far as I know, no one has ever yet been able to indicate any kind of medium or mechanism by which vibrations, such as we know to constitute the radiant energy of light and heat, can be transmitted at all from sun to planet under such restrictions. Still one ought not to be too positive in assertions as to the real condition and occupancy of so-called vacant space. The 'ether' is a good working hypothesis, but hardly more as yet.

I need not add, that a most interesting and as yet inaccessible problem, connected with the preceding, is that of the mechanism of gravitation, and, indeed, of all forces that seem to act at a distance: as for that matter, in the last analysis, *all* forces do. If there really be an 'ether,' then it would seem that somehow all attractions and repulsions of ponderable matter must be due to its action. Challis's investigations and conclusions as to the effect of hydrodynamic actions in such a medium do not seem to have commanded general acceptance; and the field still lies open for one who will show how gravitation and other forces can be correlated with each other through the ether.

Meteors and the comets, seeming to belong neither to the solar system nor to the stellar universe, present a crowd of problems as difficult as they are interesting. Much has undoubtedly been gained during the last few decades, but in some respects that which has been learned has only deepened the mystery.

The problem of the origin of comets has been supposed to be solved to a certain extent by the researches of Schiaparelli, Heis, Professor Newton, and others, who consider them to be strangers coming in from outer space, sometimes 'captured' by planets, and forced into elliptic orbits, so as to become periodic in their motion. Certainly this theory has strong supports and great authority, and probably it meets the conditions better than any other yet proposed. But the objections are really great, if not insuperable, — the fact that we have so few, if any, comets moving in hyperbolic orbits, as comets *met* by the sun would be expected to move; that there seems to be so little relation between the direction of the major axes of cometary orbits, and the direction of the solar motion in space; and especially the fact, pointed out and insisted upon by Mr. Proctor in a recent article, that the alteration of a comet's natural parabolic orbit to the observed elliptic one, by planetary action, implies a reduction of the comet's velocity greater than can be reasonably explained. If, for instance, Brorsen's comet (which has a mean distance from the sun a little more than three times that of the earth) was really once a parabolic comet, and was diverted into its present path by the attraction of Jupiter, as generally admitted, it must have had its velocity reduced from about eleven miles a second to five. Now, it is very difficult, if not out of the question, to imagine any possible configuration of the two bodies and their orbits which could result in so great a change. While I am by no means prepared to indorse as conclusive all the reasoning in the article referred to, and should be very far from ready to accept the author's alternative theory (that the periodic comets have been ejected from the planets, and so are not their captives, but their children), I still feel that the difficulty urged against the received theory is very real, and not to be evaded, though it may possibly be overcome by future research.

Still more problematical is the constitution of these strange objects of such enormous volume and inconceivable tenuity, self-luminous and transparent, yet reflecting light, the seat of forces and phenomena unparalleled in all our other experience. Hardly a topic relating to their appearance and behavior can be named which does not contain an unsolved problem. The varying intensity, polarization, and spectroscopic character of their light; the configurations of the nucleus and its surrounding nebulosity; and especially the phenomena of jets, envelopes, and tail, — all demand careful observation and thorough discussion.

I think it may be regarded as certain, that the explanation of these phenomena when finally reached, if that time ever comes, will carry with it, and be based upon, an enormous increase in our knowledge as to the condition, contents, and temperature of interplanetary space, and the behavior of matter when reduced to lowest terms of density and temperature.

Time forbids any adequate discussion of the numerous problems of stellar astronomy. Our work, in its very nature incessant and interminable, consists, of course, in the continual observation and cataloguing of the places of the stars, with ever-increasing precision. These star-places form the scaffold and framework of all other astronomical investigations involving the motions of the heavenly bodies: they are the reference-points and bench-marks of the universe. Ultimately, too, the comparison of catalogues of different dates will reveal the paths and motions of all the members of the starry host, and bring out the great orbit of the sun and his attendant planets.

Meanwhile, micrometric observations are in order, upon the individual stars in different clusters, to ascertain the motions which occur in such a case; and the mathematician is called upon again to solve the problem of such movement.

Now, too, since the recent work of Gill and Elkin in South Africa, and of Struve, Hall, and others, elsewhere, upon stellar parallax, new hopes arise that we may soon come to some wider knowledge of the subject; that, instead of a dozen or so parallaxes of doubtful precision, we may get a hundred or more relating to stars of widely different brightness and motion, and so be enabled to reach some trustworthy generalizations as to the constitution and dimensions of the stellar universe, and the actual rates of stellar and solar motion in space.

Most interesting, also, are the studies now so vigorously prosecuted by Professor Pickering in this country, and many others elsewhere, upon the brightness of the stars, and the continual variations in this brightness. Since 1875, stellar photometry has become almost a new science.

Then, there are more than a myriad of double and multiple stars to watch, and their orbits to be determined; and the nebulae claim keen attention, since some of them appear to be changing in form and brightness, and are likely to reveal to us some wonderful secrets in the embryology of worlds.

Each star also presents a subject for spectroscopic study; for although, for the most part, the stars may be grouped into a very few classes from the spectroscopic point of view, yet, in detail, the spectra of objects belonging to the same group differ considerably and significantly, almost as much as human faces do.

For such investigations, new instruments are needed, of unexampled powers and accuracy, some for angular measurement, some for mere power of seeing. Photography comes continually more and more to the front; and the idea sometimes suggests itself, that by and by the human eye will hardly be trusted any longer for observations of precision, but will be superseded by an honest, unprejudiced, and unimaginative plate and camera. The time is not yet, however, most certainly. Indeed, it can never come at all, as relates to certain observations; since the human eye and mind together integrate, so to speak, the impressions of many separate and selected moments into one general view, while the camera can only give a brutal copy of an unselected state of things, with all its atmospheric and other imperfections.

New methods are also needed, I think (they are unqestionably possible), for freeing time-observations from the errors of personal equation; and increased precision is demanded, and is being progressively attained, in the prevention, or elimination, of instrumental errors, due to differences of temperature, to mechanical strains, and to inaccuracies of construction. Astronomers are now coming to the investigation of quantities so minute, that they would be completely masked by errors of observation that formerly were usual and tolerable. The science has reached a stage, where, as was indicated at the beginning of this address, it has to confront and deal with the possible unsteadiness of the earth's rotation, and the instability of its axis. The astronomer has now to reverse the old maxim of the courts: for him, and most emphatically at present, *de minimis curat lex*. Residuals and minute discrepancies are the seeds of future knowledge, and the very foundations of new laws.

And now, in closing this hurried and inadequate, but I fear rather tedious, review of the chief problems that are at present occupying the astronomer, what answer can we give to him who insists, *Cui bono?* and requires a reason for the enthusiasm that makes the votaries of our science so ardent and tireless in its pursuit? Evidently very few of the questions which have been presented have much to do directly with the material welfare of the human race. It may possibly turn out, perhaps, that the investigation of the solar radiation, and the behavior of sun-spots, may lead to some better understanding of terrestrial meteorology, and so aid agricultural operations and navigation. I do not say it will be so, — in fact, I hardly expect it, — but I am not sure it will not. Possibly, too, some few other astronomical investigations may facilitate the determination of latitudes and longitudes, and so help exploration and commerce; but, with a few exceptions, it must be admitted that modern astronomical investigations have not the slightest immediate commercial value.

Now, I am not one of those who despise a scientific truth or principle because it admits of an available application to the affairs of what is called 'practical life,' and so is worth something to the community in dollars and cents: its commercial value is — just what it is — to be accepted gratefully.

Indirectly, however, almost all scientific truth has real commercial value, because 'knowledge is power,' and because (I quote it not irreverently) 'the truth shall make you free,' — any truth, and to some extent; that is to say, the intelligent and intellectually cultivated will generally obtain a more comfortable livelihood, and do it more easily, than the stupid and the ignorant. Intelligence and brains are most powerful allies of strength and hands in the struggle for existence; and so, on purely economical grounds, all kinds of science are worthy of cultivation.

But I should be ashamed to rest on this lower ground: the highest value of scientific truth is not economic, but different and more noble; and, to a certain and great degree, its truest worth is more as an object of pursuit than of possession. The 'practical life' — the eating and the drinking, the clothing and the sheltering — comes *first*, of course, and is the necessary foundation of any thing higher; but it is not the whole or the best or the most of life. Apart from all spiritual and religious considerations, which lie one side of our relations in this association, there can be no need, before this audience, to plead the higher rank of the intellectual, aesthetic, and moral life above the material, or to argue that the pabulum of the mind is worth as much as food for the body. Now, it is unquestionable, that, in the investigation and discovery of the secrets and mysteries of the heavens, the human intellect finds most invigorating exercise, and most nourishing and growth-making aliment. What other scientific facts and conceptions are more effective in producing a modest, sober, truthful, and ennobling estimate of man's just place in nature, both of his puny insignificance, regarded as a physical object, and his towering spirit, in some sense comprehending the universe itself, and so akin to the divine?

A nation oppressed by poverty, and near to starving, needs first, most certainly, the trades and occupations that will feed and clothe it. When bodily comfort has been achieved, then higher needs and wants appear; and then science, for truth's own sake, comes to be loved and honored along with poetry and art, leading men into a larger, higher, and nobler life.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

SOME DISTINCTIVE FEATURES OF THE BRITISH ASSOCIATION.

The general plan of organization of the British and American associations for the advancement of science is the same. The English body has a 'council' corresponding closely to our standing committee, and a 'general committee' corresponding to our body of fellows. There are, however, many points of difference which it is well worth while to study with a view of seeing what suggestions of value we may derive for our own guidance. The first of these is, that the British association has long since given up the practice of meeting and transacting business as an organized body. The general meetings are held only to hear such papers as the president's annual address, and not to transact business of any kind. The transaction of all business by the several committees saves much time which the American association spends in the work of organizing the meeting, and electing new members; and we may expect, that, as our numbers increase, this work will become so cumbrous that we shall finally adopt some plan of putting it entirely into the hands of committees.

The organization and conduct of the scientific proceedings are so like those adopted by ourselves as to call for little remark. The division into sections is substantially the same as with us; and the main difference between the sectional programmes is that no estimated length of a paper is given by our neighbors, thus avoiding one source of deception which frequently annoys intended listeners. Perhaps it was owing to the peculiar circumstances of the meeting, that the papers and discussions were of a quality superior to what we are wont to expect in a semi-popular assemblage. Only men who had some serious object could undertake so long a journey; and the result has been, that what such men have had to say was heard without any admixture of the crude and ignorant speculations so often interjected into the discussions, and even forming the subject of the papers. The one subject in which English thought has always been pre-eminent is the theories of physics, and the discussion on the seat of electromotive forces was all the more creditable from the barrenness of the subject. This discussion well illustrated the comparative state of science in the two countries; which may be expressed by saying, that, while a few men of the highest genius stand on the same level, foreign countries are greatly superior to us in the number of trained men, thoroughly grounded in first principles, which they are able to bring forward. The lucid statement by Professor Willard Gibbs of New Haven, of the principles involved, was the feature of the discussion; yet it would have been hard for the speaker to collect in his own country so appreciative an audience as that which greeted him from beyond the ocean.

The most valuable work of the British association has been the reports and investigations undertaken by committees of its appointment. Occasionally these reports have comprised synopses of the progress of science in special branches made by individual members, and presented to the society. The greater number have, however, been accounts of special researches, the funds for prosecuting which have been supplied by the association. A splendid example of such work, which must ever redound to the credit of the body which undertook it, is the system of electrical units now universally adopted, the basis for which was furnished by a committee of the British association. It can hardly be too much to say, that no one work of recent times has done more for the progress and diffusion of electrical