is impossible to place much confidence in arguments based upon the study of such complex phenomena.

As regards the question of chemical *versus* mechanical action, the speaker could only imagine one form of mechanical action attending dissolution, viz. that of the water molecules bombarding the surfaces of the solid, and as it were chipping off particles. All other actions, in so far as they could be regarded as involving the attraction of the molecules of the dissolved substance by those of the solvent, he was inclined to class as chemical. Nothing was more certain than that dissolution depended on the nature both of the solvent and of the substance dissolved. Like dissolves like—water is *the* solvent for bodies containing oxygen; sulphur compounds are dissolved by carbon bisulphide; phosphorus compounds by chloride of phosphorus; shale spirit, which is rich in olefines, and especially rosin spirit, which is rich in acetylenes and benzenes, were far better solvents of hydrocarbons and resinous bodies than petroleum, which consisted of saturated inert hydrocarbons, and was the worst of solvents. Facts such as these spoke strongly in favour of the conclusion that the phenomena of dissolution are largely of a chemical character.

Prof. W. N. Hartley was understood to base the argument in favour of the hydration theory chiefly on the changes of colour observed in the solution of certain salts in various proportions of water. The chlorides, bromides, and iodides of cobalt, nickel, and copper exhibit these phenomena most plainly. Thus the iodide of cobalt in the anhydrous state is black, its dihydrate is green, the hexhydrate a reddish brown. If this last be dissolved in water a pink solution is formed, which probably contains a richer hydrate. The brown saturated solution of the hexhydrate is a very dense liquid, of specific gravity about 3, and when water is added to it the formation of the pink liquid is attended by a large evolution of heat, and this affords evidence that the hydrate exists in the solution. Again, hydrated cupric chloride contains two molecules of water, and when quite dry is of a pale blue colour. Its solution in water has the same colour unless it be heated, and then it turns green. Nickel salts behave similarly. So that the evidence, on the whole, warrants the belief that when a hydrated salt is dissolved in water the water of crystallisation remains a constituent part of the molecule.

Dr. Gladstone commenced his remarks by a discussion of the question, What is a salt in solution? Is the solution of a salt in water a process analogous in any degree to the decomposition which takes place when one salt is mixed with another? Take, for instance, chloride of sodium and water. Many years ago the speaker had endeavoured to determine whether any chemical decomposition of the salt by the water occurred so as to give rise to sodium hydrate and hydrochloric acid, but he had come to the conclusion that this decomposition took place, if at all, only to a very small extent. Many salts, as had already been stated, combine with water to form coloured hydrates, and the hydrate is of a colour different from that of anhydrous salt. But a coloured hydrate, when dissolved in a sufficient quantity of water, is never changed by further dilution. The speaker had endeavoured to ascertain whether the specific refraction of substances was altered by solution. He had found that no alteration could be detected, and this result was afterwards confirmed by the experiments of other chemists. The refraction equivalent of a solution is equal to the sum of the refraction equivalents of the salt and the water present. In an alum solution, the water of crystallisation supposed to be in combination with the salt is not distinguishable by its refractive power from the water of solution outside it. It seems impossible, however, to arrive at a conclusion with regard to the constituents of a solution. The idea of reciprocal decomposition is not supported by experimental evidence, save in some exceptional cases, and the actual condition of a dissolved salt seems beyond expression by formulæ.

TEN YEARS' PROGRESS IN ASTRONOMY[1]

THE *Earth*.—In what may be called the astronomy of the earth there is no very great discovery, nothing extremely new and brilliant to record during the past decade; but there has been considerable and steady progress.

(*a*) As regards the earth's form and dimensions, it has become quite certain that Bessel's ellipticity (1/300) is too small. Clarke's value of 1/294 is now admitted and employed on the U.S. Coast Survey with a decided improvement of accordance. A slightly larger value even is suggested by the most recent pendulum observations, and 1/292 is now adopted in Europe.

One of the most important steps in this branch of investigation is the discovery by Mr. Peirce (of our own Coast Survey), of the large correction required in many former pendulum determinations, on account of the yielding of the stand from which the pendulum is suspended.

During the past ten or fifteen years a great amount of material has been collected towards a complete gravitational survey of the earth, by the work of Lieut.-Col. Herschel in India, and of the officers of the Coast Survey in this country and elsewhere, and a very important part of it has consisted in connecting the older work with the new, by Peirce's operations in Europe, and those of Herschel in this country.

At the same time it has become increasingly evident that very little is now to be gained by endeavouring to find a spheroid fitting the earth's actual form more closely. It will be best simply to adopt some standard (say that of Clarke, but it makes very little difference what), and to investigate hereafter the local deviations from it. These deviations seem to be larger and more extensive than used to be supposed, the station errors in latitude and longitude being at least quantities of the same order as the variations of elevation.

We mention, in passing, the investigations of Fergola, based on observations at Pulkowa and Greenwich, and leading to a suspicion that the axis of the earth is slightly changing its position and shifting the place of the Poles on the earth's surface. Operations have been organised to determine the question by co-operation between different observatories in nearly the same latitude, but widely differing in longitude.

Nor ought we to pass unnoticed an elaborate paper by Kapteyn, of Groningen, on the determination of latitude by a method depending upon time-observation of stars, at equal altitudes, though in widely different parts of the sky; the stars being so selected that all errors of star-places, instrument, and clock, are almost perfectly eliminated. In the same connection we ought to mention also the new equal-altitude instrument, the Almucantar, invented by Chandler, of Cambridge, and his development of the method of determining time by its use. It may possibly supersede the transit instrument for this purpose, as he seems to expect, though we think it hardly likely.

Rapid progress has been made in determining the difference of longitude between all the principal parts of the earth. There now remain very few stations of much importance which have not their longitude from Greenwich telegraphically settled within a small fraction of a second. In Europe Albrecht has combined into a consistent whole all the different data for more than one hundred points. Our American system has been similarly worked out by Schott, and is connected with the European by no less than four different and independent cable-determinations. South America is connected with the United States by the recent operations of our naval officers in the West Indies and along the eastern and western coasts of the continent; and with Europe by a cable connection between Lisbon and Pernambuco, also effected by them. It is worth noting that two large errors in European longitudes owe their detection to American astronomers. The difference of longitude between Greenwich and Paris was corrected by our Coast Survey in 1872 to the extent of nearly half a second of time, and our naval officers in 1878 showed that the then received longitude of Lisbon was 8·54s. too small! It is a less surprising fact that an error of 35s. was found in the longitude of Rio.

Our navy has also determined an important series of telegraphic longitudes along the eastern coast of Asia and through the East Indies. The French have been doing similar work in the same regions, especially in connection with the transits of Venus; and the English have determined a large number of longitudes in India. These Asiatic longitudes have been recently connected with Australia and New Zealand by English astronomers, and a telegraphic longitude connection has been effected down the eastern coast of Africa from Aden to the Cape; so that now it is perfectly practicable, if it is desirable, to have one standard of time in all the civilised world.

A word perhaps is here in place as to this question of standard time and the beginning of the day. The adoption by our railroads of the system of standards differing from Greenwich time only by entire hours has, I think, been admittedly a great step in advance, as regards public convenience and safety in travelling. At a few points, where standard and local time happen

[1] "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young Read May 17, 1886, before the New York Academy of Sciences.

to differ by nearly the maximum possible amount of half an hour, some annoyance is felt, and there is still some opposition; but it seems quite clear that, in this country at least, all resistance will soon die out.

As regards the more purely astronomical question of making the astronomical day coincide with the civil day, by beginning at midnight, instead of noon, as it does at present, there is more difference of opinion. For my own part, I am frankly in favour of the change, because I see no use in perpetuating an anomaly which is sometimes annoying and confusing. At the same time the change would, of course, involve some inconvenience to computers and night-observers, and it must be admitted that at present a large number, and possibly a majority, of the most eminent astronomers, in other countries as well as in this, are opposed to it. Those of us whose work falls about as much in the day as in the night, and those, I think, who take a long look ahead, are in favour of the reform; but those whose work is mainly nocturnal, or is based on observations made chiefly at or near the "witching hour," dread the inconvenience of a change of date in the midst of the record, and the risk of confusion in the interpretation of old observations.

The question, however, seems to me not a very important one. I notice that the visitors of the Royal Observatory have just recommended that the change be introduced into the British *Nautical Almanac* for 1891.

Before passing to the moon, a word should be added as to the outcome of the most recent investigations regarding the steadiness of the earth's rotation. Some irregularities in the lunar motions have appeared to justify a suspicion, at least, that they might be caused by irregularities in the length of the day. The researches of Newcomb upon ancient eclipses and occultations of stars give results not necessarily inconsistent with this hypothesis, perhaps even slightly in its favour, but his careful examination of the past transits of Mercury contra-indicates it pretty decidedly.

The Moon.—During the past ten years there has been no work upon the lunar theory quite on a level with that of Hansen, Delaunay, Plantamour, and Adams in the years preceding; but the labours of Neison, Hill, and Newcomb well deserve mention. The former especially has carried his approximations to a considerably higher point than any of his predecessors, though not without making a few numerical mistakes, which have been detected and corrected by Hill. The investigation of ancient and mediæval observations of the moon by Newcomb is also a very important work, as showing clearly that the lunar theory is still incomplete, and that it is impossible by any tables yet made to represent accurately the whole series of observations. A value of the secular acceleration which suits the observations of the last 200 years will not fit the Arabian observations made 1000 years ago, nor will it satisfy the eclipse observations of still more ancient date, unless at least the received interpretation of those ancient eclipses be admitted to be wrong, as Prof. Newcomb seems to consider rather probable. From his discussion he derives for the secular acceleration a value of 8″·4, as against the value of 12″·1, deduced by Hansen.

It will be remembered probably by every one present that the *theoretical* value of this quantity is about 6″, and that Ferrel, Adams, Delaunay, and others, attributed its apparent increase to 12″ to the action of the tides in retarding the earth's rotation and so lengthening the day; if Newcomb's value is correct, this tidal retardation is cut down from 6″ to about 2″·5.

The study of the moon's surface has been carried on with assiduity, but I do not know that any remarkable results have been reached, though Klein's observation, in 1877, of what he supposed to be a newly-formed crater (Hyginus N.), excited a good deal of interest and discussion for a number of years; and the most eminent selenographers are still divided in opinion on the question.

The publication by the German Government of Schmidt's great map of the moon, in 1878, unquestionably marks an epoch in selenography; and the photographic work of Pritchard, and the heliometric determination of the moon's physical libration by Hartwig, must not pass unnoticed.

Probably, however, the lunar work which has drawn to itself most attention and interest is the investigation of the moon's heat by Lord Rosse and Prof. Langley.

The earliest observations of the kind date back now forty years, when Melloni, in 1846, first detected the moon's heat by means of the then newly-invented thermopile. But the first really scientific *measurements* are only about fifteen years old, due to Lord Rosse, at Parsonstown, and to Marie Davy, at Paris; and they seemed to show that at the time of full moon we receive from our satellite, not merely *reflected* heat, but warmth *radiated* from the moon's surface; as if this surface were raised to a considerable temperature by the long insolation to which it has been exposed during the preceding fortnight. Lord Rosse estimated the probable temperature of this heated rock to be as high as from 300° to 500° F.

But within the past four or five years this conclusion has been called in question. Observations at Parsonstown, of the rapid diminution of radiation during a lunar eclipse, seem to favour the newer view that the moon's surface, like that of a lofty mountain-top on the earth, never gets very hot, since the absence of air enables the solar heat to escape nearly as fast as it is received.

Prof. Langley's recent and still progressing work upon this subject far excels in delicacy and elaborateness anything done before. At first it seemed to show that the temperature of freezing water was never reached even at the hottest parts of the lunar surface; but the later observations throw some doubt on the legitimacy of this inference. It is found that the radiation from the moon unquestionably contains a considerable percentage of rays which have a wave-length *longer* than any of the heat-rays from melting ice; and this fact has been supposed to make it probable that the moon's surface was colder than the ice. But then, within a few weeks, Prof. Langley has found the long-waved rays in the radiation from an *electric arc!* So the question still hangs debatable.

The Sun's Parallax.—I think we may say that, during the past ten years, substantial progress has been made with the problem of the solar parallax. The transit of Venus in 1882 adds whatever value its results may have to those obtained eight years before; but, on the whole, so far as can be judged from the reductions thus far completed and published, it would seem likely that the outcome of the transit observations will be simply to confirm the results obtained by other methods. It may be that the data obtained from the German heliometer measurements will prove more accordant and decisive than those derived from photographs and from the contact observations; there are flying rumours that they will, but it will be necessary to await the official publication for certain knowledge on this point. If they do not, we shall be obliged, hereafter, to relegate transit observations to a secondary rank, as a means of determining the sun's distance. From the various observations of the two transits, different computers have deduced values of the parallax all the way from 8″·6 to 8″·95, corresponding to a distance ranging from 95,000,000 to 91,500,000 miles.

The case is quite different with the heliometer observations of the opposition of Mars, in 1877, made by Mr. Gill at Ascension Island. These give, in a most definite and apparently authoritative manner, a value of 8″·783, and are apparently irreconcilable with any value much greater than 8″·81, or less than 8″·75. So far as can be judged from the number, nature, and accordance of the observations, I believe we must accept this as the most trustworthy of the geometrical methods yet employed; though the weight of the result would certainly be increased if it did not depend to such an extent upon the work of a single individual.

The confidence of astronomers in the correctness of this value is greatly fortified by the fact that the most recent and reliable determinations of the velocity of light, made by Michelson and Newcomb, in 1877, 1880, 1881, and 1882, when combined with the Pulkowa constant of aberration determined by Nyrèn from all the data available up to 1882, give a solar parallax accordant with the preceding almost to the hundredth of a second—8″·794 as against 8″·783. It is true there are possible theoretical objections to the method; as, for instance, that the result may be slighted affected by the motion of the solar system through space. Enough is not known certainly about the constitution of the medium that transmits light through space, to decide all such questions *a priori* and authoritatively; but it is unquestionable that any correction needed on account of such possible causes of error must be very minute.

We believe, therefore, that it is safe to assume pretty confidently that the solar parallax is about 8″·8 (though probably a trifle less), which makes the sun's mean distance 93,000,000 miles, with an error not likely much to exceed 150,000 miles. A larger value of the parallax (about 8″·85) still holds its ground in the nautical almanacs, and undeniably is nearer the *average* of the results given by *all* the known methods. But none of the other methods seem to us to compete at all in precision with the two whose authority we accept.

The Sun and Meteorology.—The study of the solar surface has been carried on very persistently by Spörer, in Germany, as well as by others, and a great amount of material has been collected bearing upon the theory and nature of sunspots, and their periodicity. The extensive series of photographs obtained at Kew, and at Dehra Doon, in India, constitutes almost a continuous record of the solar surface for several years. The relation between this periodicity and terrestrial conditions has been assiduously examined, but on the whole the outcome seems to me to leave this connection as doubtful as it ever was, in most cases at least. While in some parts of the earth it looks as if there were a slight but marked increase of storm and rainfall at the time of sunspot maximum, the reverse seems to be true in other countries. In South America, Dr. Gould thinks that he has demonstrated a very perceptible effect of the condition of the sun's surface in modifying the strength and direction of the winds; but thus far similar investigations elsewhere show no such result. It will evidently be necessary to wait for a longer and more widely extended collection of statistics to settle the question. We do not even know as yet whether we get more or less than the average heat from the sun during the sunspot maximum.

But I think it may be set down as certain that the condition of the sun's surface exerts, if perhaps a real, yet only a very slight effect upon our earthly meteorology. With terrestrial magnetism the case is markedly and singularly different, and one of the most interesting problems now pressing for solution is the nature of the connection between solar disturbances and magnetic storms.

Solar Heat.—A great deal of labour has been expended upon the study of the sun's heat during the last decade. The investigations that strike me on the whole as most worthy of mention are those of our own Langley and of the Italian Rosetti, whose early death a few months ago is a great loss to science. Secchi and Ericsson, on the one side, had contended for a solar temperature of some millions of degrees, basing their results on Newton's law of cooling; while, on the other, Crova and Violle, from their measures of the solar radiation, reduced according to the so-called law of Dulong and Petit, maintained that the temperature does not much exceed that of many terrestrial furnaces, somewhere from 1500° to 2500° C. Rosetti's experiments upon the radiation of the electric arc and other sources of intense heat showed pretty clearly the inapplicability of Dulong and Petit's law to high temperatures, and indicate a solar temperature not far from 10,000° C., or 18,000° F. But they also make it clear that the limits of uncertainty are still very great.

Prof. Langley, by his invention of the bolometer, has been able to investigate separately the amount of energy transmitted to the earth in the solar rays of every possible wave-length, and to determine the effect of our atmosphere in absorbing each kind of ray. He has shown that the older method of investigating this solar radiation, *in a lump* so to speak, gives fallacious results on account of atmospheric absorption; and that the necessary correction compels us to increase our estimate of the sun's energy at least 20 per cent. In my own little book upon the sun, published in 1881, I had set the so-called solar constant at twenty-five calories per square metre per minute. It is now certain that it must be put at least as high as thirty. Prof. Langley's investigations seem also to show another remarkable fact—that we do not receive from the sun any at all of the low-pitched, slowly-pulsing waves, such as we get from surfaces at or below the temperature of boiling water. The solar spectrum appears to be cut off abruptly at the lower end; and this cutting off we know cannot have been effected in the earth's atmosphere, because we receive from the moon in considerable quantity just this very sort of low-pitched rays. Langley finds them also abundant in the radiation of the electric arc, so that we can hardly suppose them to be *originally* wanting in the solar heat. It now looks as if we must admit that they have been suppressed either in the atmosphere of the sun itself, or in interplanetary space. Another striking conclusion first clearly pointed out by Langley is that, if the sun's atmosphere were removed, its light would be strongly blue.

The Solar Surface and Spots.—As regards the general make-up of the solar surface, I do not think there has been any new fact of extreme importance brought out within ten years. Janssen has, however, carried solar photography to higher excellence than ever attained before, and has obtained plates that show the "granules" and their grouping on a scale previously unknown. He thinks that his plates prove a peculiar constitution of the solar surface, consisting in collections of clearly-defined and rounded granules, separated by regions or streaks where they are ill defined and elongated; and he calls the phenomenon the "reseau photosphérique," or photospheric network. According to him the "net" remains approximately constant for some minutes at a time, as shown by plates taken in quick succession, but is subject to rapid and enormous changes in periods exceeding a quarter of an hour or so. I find some scepticism among high authorities as to the trustworthiness of his conclusions. There are suggestions that the appearances presented may be due to currents of air in the telescope tube and at the surface of the sensitive plate; but I am disposed to think he is right, for, on several occasions when the seeing has been exceptionally fine, I have observed with my own eyes something quite analogous, in our large telescope at Princeton.

The spots have been carefully studied by several observers, by Spörer especially, in a statistical way, and by Vogel, Lohse, Tacchini, and others, as to structure and detail. Spörer has brought out very clearly the connection between the number and average latitude of the spots. It appears that, speaking broadly, the disturbance which produces the sunspots begins in two belts on each side of the sun's equator in a latitude of over 30°; these belts or spot-zones then gradually move in towards the equator, the sunspot maximum occurring when their latitude is about 16°, while the disturbance gradually and finally disappears at a latitude of 8° or 10°, some twelve or fourteen years after its first appearance. But two or three years before this disappearance a new zone of disturbance shows itself in the same latitude as its predecessor, so that for a while, about the time of sunspot minimum, there are two well-marked zones of spots on each side of the sun's equator—one pair near the equator, due to the expiring disturbance which began some ten or eleven years ago; the other far from the equator, and due to the newly-arising outburst, which will reach its maximum in three or four years, and then pass away like the former.

There can be no doubt that the phenomenon is a very significant one, but its explanation, like that of the periodicity itself, is still to be found.

Nor is the problem of the spots themselves yet fully solved. Not that there is any reasonable question that they are *hollows* in the solar photosphere; but how they originate, how deep they are, and what are the causes of their darkness, and the condition and temperature of the darkening substance—these are questions to which only uncertain answers can now be given. A long and important series of observations upon the widening of the lines of certain elements in the sunspot spectra has been made by Mr. Lockyer, and establishes clearly the fact that those lines, of *iron* for instance, which are conspicuously black and wide in the sunspots, are often just those which do *not* show themselves conspicuously in the prominences; and moreover both in spots and prominences the iron lines that do show themselves are most frequently those which closely coincide with lines in the spectra of other substances. Singularly, also, and so far quite without explanation, it appears according to his observations that at the sunspot maximum those *iron* lines which at other times are conspicuous in spot-spectra entirely disappear.

Perhaps I may be allowed to mention here a recent observation of my own upon these spot-spectra: with a high dispersion the darkest part of the spot-spectrum is found to be not continuous, but made up of fine lines overlapping or almost touching each other, with here and there a clear space left, like a fine bright line. It means, I think, that the absorbing vapours which darken the interior of the spot are wholly gaseous, and tends to disprove the idea that they are mostly of the nature of smoke or steam. We mention also, in passing, another thing which has been shown by our large instrument at Princeton—that the apparently bulbous, finger-tip-like terminations of the penumbral filaments are often, under the best circumstances of vision, resolved into fine, bright, sharp-pointed hooks which look like the tips of curling flames.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—At the biennial election of members of the Council of the Senate, Prof. Michael Foster and Dr. Donald MacAlister were elected to serve for four years.