For the sake of clearness in our narration, we have brought together in one view the principal facts relating to Sir John Lawes' career as a chemical manufacturer; we have next to regard him as the founder of the Agricultural Experiment Station at Rothamsted, and as its presiding genius during a period of nearly sixty years.

We must go back again to 1842. When Sir John Lawes had taken out his patent, and had determined to start a factory in London, it would have seemed natural if his entire energies had been transferred to this new and promising sphere of labour; but now the truly scientific character of the man was made manifest. The agricultural investigations in the fields at Rothamsted, which had become so fascinating, were not to be given up, but extended. This could only be done by engaging scientific assistance. A young chemist, Dr. J. H. Gilbert, was engaged to superintend the Rothamsted experiments; and thus, in 1843, began that partnership in labour which has yielded such a rich harvest of results.

The station thus founded at Rothamsted began its work long before any of the agricultural stations now existing in other countries, of which there are at present several hundreds. The investigations carried out have proceeded to a considerable extent on lines peculiar to the place, and have generally been of a very laborious character. The most striking characteristic of Rothamsted is its experimental fields, covering nearly forty acres. Here the various crops of a four-course rotation are grown, both separately and in their usual order of succession; the influence of different manures upon the quantity and composition of the crops is studied; the alterations in the composition of the soil brought about by different treatment are determined, and in some cases the composition of the drainage water from the different plots is ascertained. The Rothamsted investigations have also included many important and laborious experiments on farm animals.

It is quite impossible to attempt to enumerate the various investigations made at Rothamsted. The first formal report appeared in 1847. The collected reports now occupy nine volumes. We may, however, note some stages in the development of this great enterprise. By 1848 most of the experimental fields had commenced their work; by 1856 the whole of the present series was in operation. The chemical work required soon exceeded the capacity of the old barn first used, and in 1854-5 a handsome new laboratory was built and presented to Sir John Lawes as a testimonial from the agriculturists of England for his services to agriculture. Large additional buildings for preparing and storing samples have since been added. In 1889 Sir John Lawes transferred the whole of the laboratories and experimental fields to trustees, with an endowment of 100,000*l*., so that the agricultural investigations might be permanently continued. The management is now vested in a committee, nominated by the Royal, the Royal Agricultural, the Chemical and the Linnean Societies. The jubilee of the Rothamsted Station was celebrated in 1893, and on this occasion Dr. J. H. Gilbert received the honour of knighthood. A full account of the celebration will be found in NATURE of July 27 and August 3, 1893.

But we must turn once more to the man himself. Sir John Lawes received many honours. The Queen created him a baronet in 1882. Universities gave him their degrees; societies bestowed upon him their medals. Prosperity could not spoil him. Quite free from personal ambition, he was always ready to give the credit of success to his fellow-workers. Visitors to the Rothamsted experiments—and they were many—were delighted when Sir John himself was the pilot of the party; the two hours' talk to which they listened was a treat to be remembered. In terse, vigorous sentences the practical results of each trial were brought before them, while the whole was illuminated by many a flash of humour. In his middle life Sir John Lawes wrote a great number of short articles for the agricultural press. In these he excelled. His thorough knowledge of the details of farming, and his practical mind, prevented him from ever writing as a mere doctrinaire; the facts ascertained by investigation were presented by him in their concrete aspect as things to be reckoned with by the farmer in his daily life on the farm.

Our notice would be incomplete without some reference to his local beneficence. As Lord of the Manor he did much to maintain and increase the charms of a pretty village now rapidly transforming itself into a town. He was the agricultural labourers' best friend. He provided them with an ample supply of allotment gardens, and in 1857 built a club room for their benefit; this was visited and described by Charles Dickens in 1859. Sir John Lawes also tried to introduce several co-operative schemes for the labourers' benefit. He was always seen to great advantage on the occasion of the annual allotment club dinner at which he presided, when he carved a huge piece of beef provided by himself, and afterwards made a humorous speech to the labourers. As a generous donor to public and private charities he will be long remembered.

As he passed into old age his powers seemed to suffer little diminution. A few days before his last illness he went as usual to London, and thence down to the factory at Millwall. He died on August 31, in his eighty-sixth year, full of days and full of honours, and venerated by all who knew him. R. WARINGTON.

THE BRADFORD MEETING OF THE BRITISH ASSOCIATION.

IN the midst of the turmoil of the Association week it is difficult to give any careful compression of the results of the meeting; but, from the point of view of the Local Committee, the visit has been an unqualified success. Apprehensions were felt, in making the preliminary arrangements, lest there might be some incongruity in certain cases between the guests and the hosts: but, judging from the absence of rumours to that effect, the fears were groundless.

One point upon which at first some individual soreness arose was due to a slight misunderstanding in the matter of the excursions. These had long presented an exceedingly difficult problem to the local organisers of Association meetings; so much so, that the entire abandonment of all excursions has at times been contemplated. It is probably inevitable that, in any scheme put forward to ensure the success of the excursionists as a whole, some individual hardship must be occasioned, leading to a certain amount of unpleasantness. In the circumstances, it is scarcely surprising that, at first, a few sufferers should have complained that the excursion arrangements had been planned in a new and inferior manner; but, eventually, they fell in readily with the innovations. At former meetings it has been found impossible to organise excursions upon any exact system, owing to the fact that persons applied for tickets in a vague and undecided spirit, and often failed to take them up when they were allotted to them, to the consequent deprivation of other persons. Accordingly, in the Bradford scheme, the following regulations were issued:—

(1) That out of the seven excursions for the day three should be selected in the order of preference. (2) That the fee (made practically uniform) should be handed in with the application form; and (3) that persons complying with these requirements should in due course receive a ticket for one of the trips selected, or have their money returned. Those applicants who stated that they would only name a single excursion were

advised not to hand in any application or fee, as no guarantee could be given that they would receive one particular ticket. Despite the individual irritation which has been occasioned by an imperfect comprehension of the scheme, and by occasional unwillingness to comply with the conditions, the new arrangement has received a widespread approval, and has formed another instance of the willing and courteous acceptance of the Bradford arrangements, which has so conspicuously characterised the present meeting of the Association.

RAMSDEN BACCHUS.

Meetings of the General Committee.

At the first meeting of the General Committee, Prof. Schäfer reported that the following resolutions had been considered and acted upon by the Council of the Association.

I. That in view of the opportunities of ethnographical inquiry which will be presented by the Indian census, the Council of the Association be requested to urge the Government of India to make use of the census officers for the purposes enumerated below, and to place photographers at the service of the census officers :—(1) To establish a survey of the jungle races, Bhils, Gonds, and other tribes of the central mountain districts ; (2) to establish a further survey of the Naga, Kuki, and other cognate races of the Assam and Burmese frontiers ; (3) to collect further information about the vagrant and criminal tribes, Haburas, Beriyas, Sansiyas, &c., in North and Central India ; (4) to collect physical measurements, particularly of the various Dravidian tribes, in order to determine their origin ; and of the Rajputs and Jats of Rajputana and the Eastern Punjab, to determine their relation with the Yu-echi and other Indo-Scythian races ; (5) to furnish a series of photographs of typical specimens of the various races, of views of archaic industries, and of other facts interesting to ethnologists.

II. That the Council be requested to represent to Her Majesty's Government the importance of giving more prominence to botany in the training of Indian forest officers.

A resolution was adopted, that in future women should be eligible as members of the Sectional Committees.

At the second meeting of the General Committee it was decided that the meeting of the Association in 1902 should be held in Belfast. The meeting next year will be held in Glasgow, with Prof. A. W. Rücker as president, and will commence on Wednesday, September 11, 1901. The vice-presidents for the meeting will be : The Earl of Glasgow, Lord Blythswood, Lord Kelvin, the Lord Provost of Glasgow, the Principal of the University of Glasgow, Sir John Stirling-Maxwell, M.P., Sir Andrew Noble, Sir Archibald Geikie, Sir W. T. Thiselton-Dyer, Mr. James Parker Smith, M.P., Mr. John Inglis, and Mr. Andrew Stewart. Alluding to the arrangements for the Glasgow meeting, the Lord Provost of Glasgow said the University had been placed at the disposal of the Association, and all the Sections will probably be accommodated under one roof. There will also be an exhibition in Glasgow next year, and the 450th anniversary of the University will be celebrated.

Prof. Schäfer has retired from his position as one of the general secretaries, and Dr. D. H. Scott has been elected his successor.

The following is a synopsis of the grants of money appropriated to scientific purposes by the General Committee :—

Mathematics and Physics.

	£
*Rayleigh, Lord—Electrical Standards	45
*Judd, Prof. J. W.—Seismological Observations	75
*Rücker, Prof. A. W.—Magnetic Force on board Ship (renewed)	10

* Re-appointed.

Chemistry.

	£
Hartley, Prof. W. N.—Relation between Absorption Spectra and Constitution of Organic Substances (balance, £6 8*s.* 9*d.* in hand)	—
*Roscoe, Sir H. E.—Wave-length Tables	5
*Miers, Prof. H. A.—Isomorphous Sulphonic Derivatives of Benzene	35

Geology.

*Hull, Prof. E.—Erratic Blocks (£6 in hand)	—
*Geikie, Prof. J.—Photographs of Geological Interest (balance, £10 in hand)	—
*Lloyd-Morgan, Prof. C.—Ossiferous Caves at Uphill (renewed)	5
*Watts, Prof. W. W.—Underground Water of North-West Yorkshire	50
*Scharff, Dr.—Exploration of Irish Caves (renewed) ...	15
*Marr, Mr. J. E.—Life-zones in British Carboniferous Rocks	2

Zoology.

*Herdman, Prof. W. A.—Table at the Zoological Station, Naples	100
*Bourne, Mr. G. C.—Table at the Biological Laboratory, Plymouth	20
*Woodward, Dr. H.—Index Generum et Specierum Animalium...	75
*Newton, Prof. A.—Migration of Birds	10
Poulton, Prof. E. B.—Life-history of the Marble Gall-fly	—

Geography.

Keltie, Dr. J. Scott—Terrestrial Surface Waves ...	5
Mill, Dr. H. R.—Changes of Land-level in the Phlegræan Fields	50

Economic Science and Statistics.

*Giffen, Sir R.—State Monopolies in other Countries (£13 13s. 6d. in hand)	—
Brabrook, E. W.—Legislation regulating Women's Labour	15

Mechanical Science.

*Preece, Mr. W. H.—Small Screw Gauge (balance in hand) and...	45
Binnie, Sir A.—Resistance of Road Vehicles to Traction	75

Anthropology.

*Evans, Mr. A. J.—Silchester Excavation	10
*Penhallow, Prof. D. P.—Ethnological Survey of Canada	30
Garson, Dr. J. G.—Age of Stone Circles (balance in hand)	—
*Read, Mr. C. H.—Photographs of Anthropological Interest (balance of £10 in hand)	—
*Tylor, Prof. G. B.—Anthropological Teaching	5
Evans, Sir John.—Exploration in Crete	145

Physiology.

*Schäfer, Prof. E. A.—Physiological Effects of Peptone...	30
Schäfer, Prof. E. A.—Chemistry of Bone Marrow ...	15
Starling, Prof. E. H.—Suprarenal Capsules in the Rabbit	5

Botany.

*Farmer, Prof. J. B.—Fertilisation in Phæophyceæ ...	15
Marshall Ward, Prof.—Morphology, Ecology, and Taxonomy of Podostemaceæ	20

Corresponding Societies.

*Whitaker, Mr. W.—Preparation of Report	15
	£945

* Re-appointed.

SECTION A.

DEPARTMENT OF ASTRONOMY.

OPENING ADDRESS BY DR. A. A. COMMON, F.R.S., F.R.A.S., CHAIRMAN OF THE DEPARTMENT.

IT has been decided to form a Department of Astronomy under Section A, and I have been requested to give an address on the occasion. In looking up the records of the British Association to see what position Astronomy has occupied, I was delighted to find, in the very first volume, "A Report on the Progress of Astronomy during the Present Century," made by the late Sir George Airy, so many years our Astronomer Royal, and at that time Plumian Professor of Astronomy at Cambridge. This report, made at the second meeting of the Association, describes, in a most interesting manner, the progress that was made during the first third of the century, and we can gather from it the state of astronomical matters at that time. The thought naturally occurred to me to give a report, on the same lines, to the end of this century, but a little consideration showed that it was impossible in the limited time at my disposal to give more than a bare outline of the progress made.

At the time this report was written we may say, in a general way, that the astronomy of that day concerned itself with the position of the heavenly bodies only, and, except for the greater precision of observation resulting from better instruments and the larger number of observatories at work, this, the gravitational side of astronomy, remains much as it was in Airy's time.

What has been aptly called the New or Physical Astronomy did not then exist. I propose to briefly compare the state of things then existing with the present state of the science, without dealing very particularly with the various causes operating to produce the change; to allude briefly to the new astronomy; and to speak rather fully about astronomical instruments generally, and of the lines on which it is most probable future developments will be made.

In this report (*Brit. Assoc. Report*, 1831–32, p. 125) we find that at the beginning of the century the Greenwich Observatory was the only one in which observations were made on a regular system. The thirty-six stars selected by Dr. Maskelyne, and the sun and moon, were observed on the meridian with great regularity, the planets very rarely and only at particular parts of their orbits; small stars, or stars not included in the thirty-six, were seldom observed.

This state of affairs was no doubt greatly improved at the epoch of the report, but it contrasts strongly with the present work at Greenwich, where 5000 stars were observed in 1899, in addition to the astrographic, spectroscopic, magnetic, meteorological, and other work.

Many observatories, of great importance since, were about that time founded, those at Cambridge, Cape of Good Hope and Paramatta having just been started. A list is given of the public observatories then existing, with the remark that the author is "unaware that there is any public observatory in America, though there are," he says, "some able observers."

The progress made since then is truly remarkable The first public observatory in America was founded about the middle of the century, and now public and private observatories number about 150, while the instrumental equipment is in many cases superior to that of any other country. The prophetic opinion of Airy about American observers has been fully borne out. The discovery of two satellites to Mars by Hall in 1877, of a fifth satellite to Jupiter by Barnard in 1892, and the discovery of Hyperion by Bond, simultaneously with Lassell, in 1848, are notable achievements.

The enormous amount of work turned out by the Harvard Observatory and its branches in South America, all the photographic and spectroscopic work carried out by many different astronomers, and the new lines of research initiated show an amount of enthusiasm not excelled by any other country. A greater portion of the astronomical work in America has been on the lines of the new astronomy, but the old astronomy has not been at all neglected. In this branch pace has been kept with other countries.

From this report we gather that the mural quadrant at most of the observatories was about to be replaced by the divided circle. Troughton had perfected a method of dividing circles, which, as the author says, "may be considered as the greatest improvement ever made in the art of instrument making."

Two refractors of 11 and 12 inches aperture had just been imported into this country; clockwork for driving had been applied to the Dorpat and Paris equatorials, but the author had not seen either in a state of action.

The method of mounting instruments adopted by the Germans was rather severely criticised by the author, the general principle of their mounting being "telescopes are always supported at the middle, not at the ends."

"Every part is, if possible, supported by counterpoises."

"To these principles everything is sacrificed. For instance, in an equatorial the polar axis is to be supported in the middle by a counterpoise. This not only makes the instrument weak (as the axis must be single), but also introduces some inconvenience into the use of it. The telescope is on one side of the axis; on the other side is a counterpoise Each end of the telescope has a counterpoise. A telescope thus mounted must, I should think, be very liable to tremor. If a person who is no mechanic and who has not used one of these instruments may presume to give an opinion, I should say that the Germans have made no improvement in instruments except in the excellence of the workmanship."

I have no doubt that this question had often occupied Airy's mind, for in the Northumberland Equatorial Telescope which he designed shortly after for Cambridge he adopted what has been called the English form of mounting, where the telescope is supported by a pivot at each side, and a long polar axis is supported at each end. This telescope is in working order at the present time at Cambridge.

When he became Astronomer Royal he used the same design for what was for many years the great equatorial at Greenwich, though the wooden uprights forming the polar axis were in the Greenwich telescope replaced by iron. It says much for the excellence of the design and workmanship of this mounting, designed as it was for an object-glass of about 13 inches diameter, when we find the present Astronomer Royal, Mr. Christie, has used it to carry a telescope of 28 inches aperture, and that it does this perfectly.

Notwithstanding the greater steadiness of the English form of mounting, the German form has been adopted generally for the mounting of the large refractors recently made.

There is much interesting matter in this report of an historical character.

As I have already said, the new astronomy, as we know it, did not exist; but in a report (*Brit. Assoc. Report*, 1831–32, p. 308) on optics, in the same volume, by Sir David Brewster, we find that spectrum analysis was then occupying attention, and the last paragraph of this report is well worth quoting: "But whatever hypothesis be destined to embrace and explain this class of phenomena, the fact which I have mentioned opens an extensive field of inquiry. By the aid of the gaseous absorbent we may study with the minutest accuracy the action of the elements of material bodies in all their variety of combinations, upon definite and easily recognised rays of light, and we may discover curious analogies between their affinities and those which produce the fixed lines in the spectra of the stars. The apparatus, however, which is requisite to carry on such inquiries with success cannot be procured by individuals, and cannot even be used in ordinary apartments. Lenses of large diameter, accurate heliostats, and telescopes of large aperture are absolutely necessary for this purpose; but with such auxiliaries it would be easy to construct optical combinations, by which the defective rays in the spectra of all the fixed stars down to the *tenth* magnitude might be observed, and by which we might study the effects of the very combustion which lights up the suns of other systems."

Brewster's words are almost prophetic, and it would almost appear as if he unknowingly held the key to the elucidation of the spectrum lines, for it was not until 1859 that Kirchhoff's discovery of the true origin of the dark lines was made.

Fraunhofer was the first to observe the spectra of the planets and the stars, and to notice the different types of stellar spectra. In 1817 he recorded the spectrum of Venus and Sirius, and later, in 1823, he described the spectrum of Mars; also Castor, Pollux, Capella, Betelgeux and Procyon.

Fraunhofer, Lamont, Donati, Brewster, Stokes, Gladstone and others carried on their researches at a time when the principles of spectrum analysis were unknown, but immediately upon Kirchhoff's discovery great interest was awakened.

With spectrum analysis thus established, aided as it was later by the greater development of photography, the new astronomy was firmly established.

The memorable results arrived at by Kirchhoff were no sooner published than they were accepted without dissent. The works of Stokes, Foucault and Ångström at that period were all suggestive of the truth, but do not mark an epoch of discovery.

Astronomical spectroscopy divided itself naturally into two main branches, the one of the sun, the other of the stars, each having its many offshoots. I shall just mention a few points relating to each. The dark lines in the solar spectrum had already been mapped by Fraunhofer, and now it only needed better instruments and the application of laboratory spectra with Kirchhoff's principle to advance this work still further.

Fraunhofer had already pointed out the way in using gratings, and these were further improved by Nobert and Rutherfurd.

Kirchhoff's Map of the Solar Spectrum, published in 1861–62, was the most complete up to that time; but the scale of reference adopted by him was an arbitrary one, so that it was not long before this was improved upon. Ångström published in 1868 his map of the "Normal Solar Spectrum," adopting the natural scale of wave-lengths for reference, and this remained in use until quite recent times.

The increased accuracy in the ruling of gratings by Rutherfurd materially improved the efficiency of the solar spectroscope, but it was not until Prof. Rowland's invention of the concave grating that this work gained any decisive impetus. The maps (first published in 1885) and tables (published in the years 1896–98) of the lines of the solar spectrum are now almost universally accepted and adopted as a standard of reference. These tables alone record about 10,000 lines in the spectrum of the sun, which is in marked contrast to the number 7 recorded by Wollaston at the beginning of the century (1802). Good work in the production of maps has also been done in this country by Higgs.

Michelson has also recently invented a new form of spectroscope called the "Echelon" (*Ast. Phys. Journ.*, vol. viii. 1898, p. 37), in which a grating with a relatively small number of lines is employed, the dispersion necessary for modern work being obtained by using a high order (say the hundredth) into which most of the light has been concentrated.

Besides lines recorded in the visual and ultra-violet portions of the solar spectrum, maps have been made of the lines in the infra-red, the most important being that of Langley's, published in 1894, prepared by the use of his "bolometer." Good work had, however, been done in this direction previously by Becquerel, Lamansky and Abney; the last, indeed, succeeded even in photographing a part of it.

The recording of the Fraunhofer lines in the solar spectrum is not all, however. The application of the spectroscope to the sun has several epoch-marking events attached to it, notably those of proving the solar character of the prominences and corona, the rendering visible of the prominences without the aid of an eclipse by the discovery of Lockyer and Janssen in 1868, the photography of the prominences both round the limb and those projected on the solar disc by the invention of the spectraheliograph by Hale and Deslandres in 1890.

Success has not yet favoured the many attempts to photograph the corona without an eclipse by spectroscopic means; but even now this problem is being attacked by Deslandres with the employment of the calorific rays.

Spectroscopic work on the sun has led to the discovery of many hundreds of dark lines, the counterparts of which it has not yet been possible to produce on the earth.

But besides those unknown substances which reveal their presence by dark lines, there were two others discovered, which showed themselves only by bright lines, the one in the chromosphere, to which the name of Helium was given, and the other in the corona, to which the name of Coronium was applied.

The former was, however, identified terrestrially by Ramsay in 1895, though the latter is still undetermined. The revision of its wave-length, brought about by the observations of the eclipse of 1898, may, however, result in this element being transferred from the unknown to the known in the near future.

The study of stellar spectra was taken up by Huggins, Rutherfurd and Secchi. Rutherfurd (*Am. Journ.*, vol. xxxv. 1862, p. 77) published in 1862 his results upon a number of stars, and suggested a rough classification of the white and yellow stars; but Secchi deserves the high credit of introducing the first systematic differentiation of the stars according to their spectra, he having begun a spectroscopic survey of the heavens for the purposes of classification (*Comptes rendus*, t. lvii. 1853), whilst Huggins devoted himself to the thorough analysis of the spectra of a few stars.

The introduction of photography marks another epoch in the study of stellar spectra. Sir William Huggins applied photography as early as 1863 (*Phil. Trans.*, 1864, p. 428), and secured an impression of the spectrum of Sirius, but nearly another decade elapsed before Prof. H. Draper (*Am. Journ. of Soc. and Arts*, vol. xviii. 1879, p. 421) took a photograph of the spectrum of Vega in 1872, which was the first to record any lines. With the introduction of dry plates this branch of the new astronomy received another impetus, and the catalogues of stellar spectra have now become numerous. Among them may be mentioned those of Harvard College, Potsdam, Lockyer, McClean and Huggins. The Draper Catalogue (*Annals Harvard Coll.*, vol. xxvii. 1890) of the Harvard College, which is a spectroscopic Durchmusterung, alone contains the spectra of 10,351 stars down to the 7–8 magnitudes, and this has further been extended by work at Arequipa, whilst Vogel and Müller of Potsdam (*Astro-Phys. Obs. zu Potsdam*, vol. iii. 1882–83) made a spectroscopic survey of the stars down to the 7·5 magnitude between $-1°$ and $+20°$ declination. This has again been supplemented by Scheiner (*ibid.*, vol. vii. 1895: "Untersuchungen über die Spectra der helleren Sterne"), and by Vogel and Wilsing (*ibid.*, vol. xii. 1899: "Untersuchungen über die Spectra von 528 Sternen"). Lockyer (*Phil. Trans.*, vol. clxxxiv. A, 1893) in 1892 published a series of large-scale photographs of the brighter stars, and more recently McClean (*Phil. Trans.*, vol. cxci. A, 1898) has completed a spectroscopic survey of the stars of both hemispheres down to the $3\frac{1}{2}$ magnitude. For the study and investigation of special types of stars, the researches of Dunér on the red stars, made at Upsala, and those of Keeler and Campbell on the bright-line stars, made at the Lick Observatory, deserve mention. For the study of stellar spectra the use of prisms in slit or objective-prism spectroscopes has predominated, though more recently the use of specially ruled gratings has been attended by some degree of success at the Yerkes Observatory.

Several new stars have also been discovered by their spectra by Pickering in his routine work of charting the spectra of the stars in different portions of the sky. The photographic plate containing their peculiar spectra was, however, not examined in many cases until the star had died down again.

Spectrum analysis also opened up another field of inquiry, viz. that of the motion of the stars in the line of sight, based on the process of reasoning due to Doppler, and accordingly named Doppler's Principle ("Ueber das farbige Licht der Doppelsterne," . . . *Abhandl. der K. Böhmischen Ges. d. Wiss.* V. Folge, 2 Bd. 1843.)

The observatories of Greenwich and Potsdam were among the first to apply this to the stars, and more recently Campbell at Lick, Newall at Cambridge, and Belopolsky at Pulkowa have made use of the same principle with enormous success.

It was also discovered that there are certain classes of stars having a large component velocity in the line of sight, which changes its direction from time to time, and in many such cases orbital motion has been proven, as in the case of Algol.

Another class of binary stars has also been discovered spectroscopically and explained by Doppler's principle. I refer to the stars known as spectroscopic binaries, in which the spectrum lines of one luminous source reciprocate over those from the other source of light, according as one is moving towards or away from the earth. This displacement of the spectrum lines led to the discovery of the duplicity of β Aurigæ, and ζ Ursæ Majoris by Pickering (*Am. Jour.* [3], 39, p. 46, 1890).

Several other such stars have now been detected, notably β Lyræ, and lastly Capella, discovered independently by Campbell (*Astro-Phys. Jour.*, vol. x. p. 177) at Lick, and Newall (*Monthly Notices*, vol. lx. p. 2, 1899) at Cambridge.

The progress of the new astronomy is so closely bound up with that of photography that I shall briefly call to mind some of the many achievements in which photography has aided the astronomer.

Daguerre's invention in 1839 was almost immediately tried with the sun and moon, J. W. Draper and the two Bonds in America, Warren de la Rue in this country, and Foucault and Fizeau in France, being among the pioneers of celestial photography; but no real progress seems to have been made until after the introduction of the collodion process. Sir John Herschel in 1847 suggested the daily self-registration of the sun-spots to supersede drawings; and in 1857 the De la Rue photo-heliograph was installed at Kew. From 1858–72 a daily record was maintained by the Kew photo-heliograph, when the work was discontinued. Since 1873 the Kew series has been continued at Greenwich, and is supplemented by pictures from Dehra Dûn in India and from Mauritius. The standard size of the sun's disc on these photographs has now been for many years 8 inches, though for some time a 12-inch series was kept up.

The first recorded endeavour to employ photography for eclipse work dates back to 1851, when Berowsky obtained a daguerreotype of the solar prominences during the total eclipse. From that date nearly every total eclipse of the sun has been studied by the aid of photography.

In 1860 the first regularly planned attack on the problem by means of photography was made, when De la Rue and Secchi successfully photographed the prominences and traces of the corona, but it was not until 1869 that Prof. Stephen Alexander obtained the first good photograph of the corona.

In recent years, from 1893 until the total eclipse which occurred last May, photography has been employed to secure large-scale pictures of the corona. These were inaugurated in 1893 by Prof. Schaeberle, who secured a 4-inch picture of the eclipsed sun in Chili: these have been exceeded by Prof. Langley, who obtained a 15-inch picture of the corona in North Carolina during the eclipse of May 1900.

Photography also supplied the key to the question of the prominences and corona being solar appendages, for pictures of the eclipse sun taken in Spain in 1860 terminated this dispute with regard to the prominences, and finally to the corona in 1871.

In 1875, in addition to photographing the corona, attempts were made to photograph its spectrum, and at every eclipse since then the sensitised plate has been used to record both the spectrum of the chromosphere and the corona. The spectrum of the lower layers of the chromosphere were first successfully photographed during the total eclipse of 1896 in Nova Zembla by Mr. Shackleton, though seen by Young as early as 1870, and a new value was given to the wave-length of the coronal line (wrongly mapped by Young in 1869) from photographs taken by Mr. Fowler during the eclipse of 1898 (India).

Lunar photography has occupied the attention of various physicists from time to time, and when Daguerre's process was first enunciated, Arago proposed that the lunar surface should be studied by means of the photographically produced images. In 1840 Dr. Draper succeeded in impressing a daguerreotype plate with a lunar image by the aid of a 5-inch refractor. The earliest lunar photographs, however, shown in England were due to Prof. Bond, of the United States. These he exhibited at the Great Exhibition in 1851. Dancer, the optician, of Manchester, was, perhaps, the first Englishman who secured lunar images, but they were of small size (Abney, "Photography").

Another skilful observer was Crookes, who obtained images of 2 inches diameter, with an 8-inch refractor of the Liverpool Observatory. In 1852 De la Rue began experimenting in lunar photography. He employed a reflector of some 10 feet focal length and about 13 inches diameter. A very complete account of his methods is given in a paper read before the British Association. Mr. Rutherfurd at a later date having tried an 11½-inch refractor, and also a 13-inch reflector, finally constructed a photographic refracting telescope, and produced some of the finest pictures of the moon that were ever taken until recent years. Also Henry Draper's picture of the moon taken Sept. 3, 1863, remained unsurpassed for a quarter of a century.

Admirable photographs of the lunar surface have been published in recent years by the Lick Observatory and others. I myself devoted considerable attention to this subject at one time; but only those surpassing anything before attempted have been published in 1896–99 by MM. Loëwy and Puiseux, taken with the Equatorial Coudé of the Paris Observatory.

Star prints were first secured at Harvard College, under the direction of W. C. Bond, in 1850; and his son, G. P. Bond, made in 1857 a most promising start with double-star measurements on sensitive plates, his subject being the well-known pair in the tail of the Great Bear. The competence of the photographic method to meet the stringent requirements of exact astronomy was still more decisively shown in 1866 by Dr. Gould's determination from his plates of nearly fifty stars in the Pleiades. Their comparison with Bessel's places for the same objects proved that the lapse of a score of years had made no difference in the configuration of that immemorial cluster; and Prof. Jacoby's recent measures of Rutherfurd's photographs taken in 1872 and 1874 enforce the same conclusion.

The above facts are so forcible that no wonder that at the Astrophotographic Congress held in Paris in 1887 it was decided to make a photographic survey of the heavens, and now eighteen photographic telescopes of 13 inches aperture are in operation in various parts of the world, for the purpose of preparing the international astrographic chart, and it was hoped that the catalogue plates would be completed by 1900.

Photography has been applied so assiduously to the discovery of minor planets that something like 450 are now known, the most noteworthy, perhaps, as regards utility being the discovery of Eros (433) in 1898 by Herr Witt at the Observatory Urania, near Berlin.

With regard to the application of photography to recording the form of various nebulæ, it is interesting to quote a passage from Dick's "Practical Astronomer," published in 1845, as opposed to Herschel's opinion that the photography of a nebula would never be possible.

"It might, perhaps, be considered as beyond the bounds of probability to expect that even the distant nebulæ might thus be fixed, and a delineation of their objects produced, which shall be capable of being magnified by microscopes. But we ought to consider that the art is only in its infancy, and that plates of a more delicate nature than those hitherto used may yet be prepared, and that other properties of light may yet be discovered, which shall facilitate such designs. For we ought now to set no boundaries to the discoveries of science, and to the practical applications of scientific discovery, which genius and art may accomplish."

It was not, however, until 1880 that Draper first photographed the Orion Nebula, and later by three years I succeeded in doing the same thing with an exposure of only thirty-seven minutes. In December 1885 the brothers Henry by the aid of photography found that the Pleiades were involved in a nebula, part of which, however, had been seen by myself (*Monthly Notices*, vol. xl. p. 376) with my 3-foot reflector in February 1880, and later, February 1886; it was also partly discerned at Pulkowa with the 30-inch refractor then newly erected.

Still more nebulosity was shown by Dr. Roberts's photographs (*ibid.*, vol. xlvii. p. 24), taken with his 20-inch reflector in October and December 1886, when the whole western side of the group was shown to be involved in a vast nebula, whilst a later photograph taken by MM. Henry early in 1888 showed that practically the whole of the group was a shoal of nebulous matter.

In 1881 Draper and Janssen recorded the comet of that year by photography.

Huggins (*Proc. Roy. Soc.*, vol. xxxii. No. 213) succeeded in photographing a part of the spectrum of the same object (Tebbutt's Comet 1881, II.) on June 24, and the Fraunhofer lines were amongst the photographic impressions, thus demonstrating that at least a part of the continuous spectrum is due to reflected sunlight. He also secured a similar result from Comet Wells (*Brit. Assoc. Report*, 1882, p. 442).

I propose to consider the question of the telescope on the following lines: (1) The refractor and reflector from their inception to their present state. (2) The various modifications and improvements that have been made in mounting these instruments, and (3) the instrument that has lately been introduced by a combination of the two, refractor and reflector, a striking example of which exists now at the Paris Exhibition.

At a meeting of the British Association held nearly half a century ago (1852) (Belfast) Sir David Brewster showed a plate of rock crystal worked in the form of a lens which had been recently found in Nineveh. Sir David Brewster asserted that this lens had been destined for optical purposes, and that it never was a dress ornament.

That the ancients were acquainted with the powers of a magnifying lens may be inferred from the delicacy and minuteness of the incised work on their seals and intaglios, which could only have been done by an eye aided by a lens of some sort.

There is, however, no direct evidence that the ancients were really acquainted with the refracting telescope, though Aristotle speaks of the tubes through which the ancients observed distant objects, and compares their effect to that of a well from the bottom of which the stars may be seen in daylight ("De Gen. Animalium," lib. v.) As an historical fact without any equivocations, however, there is no serious doubt that the telescope was invented in Holland.

The honour of being the originator has been claimed for three men, each of whom has had his partisans. Their names are Metius, Lippershey and Janssen.

Galileo himself says that it was through hearing that some one in France or Holland had made an instrument which magnified distant objects that he was led to inquire how such a result could be obtained.

The first publisher of a result or discovery, supposing such discovery to be honestly his own, ranks as the first inventor, and there is little doubt that Galileo was the first to show the world how to make a telescope (Newcomb's "Astronomy," p. 108). His first telescope was made whilst on a visit to Venice, and he there exhibited a telescope *magnifying three times*: this was in May 1609. Later telescopes which emanated from the hands of Galileo magnified successively four, seven and thirty times. This latter number he never exceeded.

Greater magnifying power was not attained until Kepler explained the theory and some of the advantages of a telescope made of two convex lenses in his "Catoptrics" (1611). The first person to actually apply this to the telescope was Father Scheiner, who describes it in his "Rosa Ursina" (1630), and Wm. Gascoigne was the first to appreciate practically the chief advantages by his invention of the micrometer and application of telescopic sights to instruments of precision.

It was, however, not until about the middle of the seventeenth century that Kepler's telescope came to be nearly universal, and then chiefly because its field of view exceeded that of the Galilean.

The first powerful telescopes were made by Huyghens, and with one of these he discovered Titan (Saturn's brightest satellite): his telescopes magnified from forty-eight to ninety-two times, were about $2\frac{1}{3}$ inches aperture, with focal lengths ranging from 12 to 23 feet. By the aid of these he gave the first explanation of Saturn's ring, which he published in 1659.

Huyghens also states that he made object-glasses of 170 feet and 210 feet focal length; also one 300 feet long, but which magnified only 600 times; he also presented one of 123 feet to the Royal Society of London.

Auzout states that the best telescopes of Campani at Rome magnified 150 times, and were of 17 feet focal length. He himself is said to have made telescopes of from 300 to 600 feet focus, but it is improbable that they were ever put to practical use. Cassini discovered Saturn's fifth satellite (Rhea) in 1672, with a telescope made by Campani, magnifying about 150 times, whilst later, in 1684, he added the third and fourth satellites of the same planet to the list of his discoveries.

Although these telescopes were unwieldy, Bradley, with his usual persistency, actually determined the diameter of Venus in 1722 with a telescope of 212 feet focal length.

With such cumbersome instruments many devices were invented of pointing these *aërial telescopes*, as they were termed, to various parts of the sky. Huyghens contrived some ingenious arrangements for this purpose, and also for adjusting and centreing the eye-piece, the object-glass and eye-piece being connected by a long braced rod.

It was not, however, until Dolland's invention of the achromatic object-glass in 1757–58 that the refracting telescope was materially improved, and even then the difficulty of obtaining large blocks of glass free from striæ limited the telescope as regards aperture, for even at the date of Airy's report we have seen that 12 inches was about the maximum aperture for an object-glass.

The work of improving glass dates back to 1784, when Guinand began experimenting with the manufacture of optical flint glass.

He conveyed his secrets to the firm of Fraunhofer and Utzschneider, whom he joined in 1805, and during the period he was there they made the 9·6 inches object-glass for the Dorpat telescope.

Merz and Mädler, the successors of Fraunhofer, carried out successfully the methods handed down to them by Guinand and Fraunhofer.

Guinand communicated his secrets to his family before his death in 1823, and they entered into partnership with Bontemps. The latter afterwards joined the firm of Chance Bros., of Birmingham, and so some of Guinand's work came to England.

At the present day MM. Feil, of Paris, who are direct descendants of Guinand and Messrs. Chance Bros., of Birmingham, are the best known manufacturers of large discs of optical glass.

It is related in history that Ptolemy Euergetes had caused to be erected on a lighthouse at Alexandria a piece of apparatus for discovering vessels a long way off; it has also been maintained that the instrument cited was a concave reflecting mirror, and it is possible to observe with the naked eye images formed by a concave mirror, and that such images are very bright.

Also the Romans were well acquainted with the concentrating power of concave mirrors, using them as burning mirrors, as they were called. The first application of an eye lens to the image formed by reflection from a concave mirror appears to have been made by Father Zucchi, an Italian Jesuit. His work was published in 1652, though it appears he employed such an instrument as early as 1616. The priority, however, of describing, if not making, a practical reflecting telescope belongs to Gregory, who, in his "Optica Promota," 1663, discusses the forms of images of objects produced by mirrors. He was well aware of the failure of all attempts to perfect telescopes by using lenses of various curvature, and proposed the form of reflecting telescope which bears his name.

Newton, however, was the first to construct a reflecting telescope, and with it he could see Jupiter's satellites, &c. Encouraged by this, he made another of $6\frac{1}{2}$ inches focal length, which magnified thirty-eight times, and this he presented to the Royal Society on the day of his election to the Society in 1671.

To Newton we owe also the idea of employing pitch, used in the working of the surfaces.

A third form of telescope was invented by Cassegrain in 1672. He substituted a small convex mirror for the concave mirror in Gregory's form, and thus rendered the telescope a little shorter.

Short also, from 1730–68, displayed uncommon ability in the manufacture of reflecting telescopes, and succeeded in giving true parabolic and elliptic figures to his specula, besides obtaining a high degree of polish upon them. In Short's first telescopes the specula were of glass, as suggested by Gregory; but it was not until after Liebig's discovery of the process of depositing a film of metallic silver upon a glass surface from a salt in solution that glass specula became almost universal, and thus replaced the metallic ones of earlier times.

Shortly after the announcement of Liebig's discovery Steinheil (*Gaz. Univ. d'Augsburg*, March 24, 1856)—and later, independently, Foucault (*Comptes rend.*, vol. xliv. February 1857)—proposed to employ glass for the specula of telescopes. and, as is well known, this is done in all the large reflectors of to-day.

I now propose to deal with the various steps in the development of the telescope, which have resulted in the three forms that I take as examples of the highest development at the present time. These are the Yerkes telescope at Chicago, my own 5-foot reflector, and the telescope recently erected at the Paris Exhibition, dealing not only with the mountings, but with the principles of construction of each. When the telescope was first used all could be seen by holding it in the hand. As the magnifying power increased, some kind of support would become absolutely necessary, and this would take the form of the altitude and azimuth stand, and the motion of the heavenly bodies would doubtless suggest the parallactic or equatorial movement, by which the telescope followed the object by one movement of an axis placed parallel to the pole. This did not come, however, immediately. The long focus telescopes of which I have spoken were sometimes used with a tube, but more often the object-glass was mounted in a long cell and suspended from the top of a pole, at the right height to be in a line between the observer and the object to be looked at; and it was so arranged that by means of a cord it could be brought into a fairly correct position. Notwithstanding the extreme awkwardness of this arrangement, most excellent observations were made in the seventeenth century by the users of these telescopes. Then the achromatic telescope was invented and mechanical mountings were used, with circles for finding positions, much as we have them now. I have already mentioned the rivalry between the English and German forms of mountings, and Airy's preference for the English form. The general feeling amongst astronomers has, however, been largely in favour of the German mounting for refractors, due, no doubt, to a great extent, to the enormous advance in engineering skill. We have many examples of this form of mounting. A list of the principal large refracting and reflecting telescopes now existing is given at the end of this paper. All the refractors in this list, with the exception of the Paris telescope of 50 inches, and the Greenwich telescope of 28 inches, are mounted on the German form. Some of these carry a reflector as well, as, for instance, the telescope lately presented to the Greenwich Observatory by Sir Henry Thompson, which, in addition to a 26-inch refractor, carries a 30-inch reflector at the other end of the declination axis, such as had been previously used by Sir William Huggins and Dr. Roberts; the last, and perhaps the finest, example of the German form being the Yerkes telescope at Chicago.

The small reflector made by Sir Isaac Newton, probably the first ever made, and now at the Royal Society, is mounted on a ball, gripped by two curved pieces, attached to the body of the telescope, which allows the telescope to be pointed in any direction We have not much information as to the mounting of early reflectors. Sir William Herschel mounted his 4-foot telescope on a rough but admirably-planned open work mounting, capable of being turned round, and with means to tilt the telescope to any required angle. This form was not very suitable for picking up objects or determining their position, except indirectly; but for the way it was used by Sir William Herschel it was most admirably adapted: the telescope being elevated to the required angle, it was left in that position, and became practically a transit instrument. All the objects passing through the field of view (which was of considerable extent, as the eyepiece could be moved in declination) were observed, and their places in time and declination noted, so that the positions of all these objects in the zone observed were obtained with a considerable degree of accuracy. It was on this plan that Sir John Herschel made his general catalogue of nebulæ, embracing all the nebulæ he could see in both hemispheres; a complete work by one man that is almost unique in the history of astronomy.

Sir William Herschel's mounting of his 4-foot reflector differs in almost every particular from the mountings of the long focus telescopes we have just spoken of. The object-glass was at a height, the reflector was close to the ground. There was a tube to one telescope, but not to the other. The observer in one case stood on the ground, in the other he was on a stage at a considerable elevation. One pole sufficed with a cord for one; a whole mass of poles, wheels, pulleys and ropes surrounded the other. In one respect only were they alike—they both did fine work.

Lassell seems to have been the first to mount a reflector equatorially. He, like Herschel, made a 4-foot telescope, and this he mounted in this way. Lord Rosse mounted his telescopes somewhat after the manner of Sir William Herschel. The present Earl has mounted a 3-foot equatorially.

A 4-foot telescope was made by Thomas Grubb for Melbourne, and this he mounted on the German plan. The telescope being a Cassegrain, the observer is practically on the ground level. A somewhat similar instrument exists at the Paris Observatory. Lassell's 4-foot was mounted in what is called a fork mounting, as is also my own 5-foot reflector, and this in some ways seems well adapted for reflectors of the Newtonian kind.

We now come to the Paris telescope. This is really the result of the combination of a reflector and a refractor. I cannot say when a plane mirror was first used to direct the light into a telescope for astronomical purposes. It seems first to have been suggested by Hooke, who, at a meeting of the Royal Society, when the difficulty of mounting the long focus lenses of Huyghens was under discussion, pointed out that all difficulties would be done away with if, instead of giving movement to the huge telescope itself, a plane mirror were made to move in front of it (Lockyer, "Star-gazing," p. 453).

The Earl of Crawford, then Lord Lindsay, used a heliostat to direct the rays from the sun, on the occasion of the transit of Venus, through a lens of 40 feet focal length, in order to obtain photographs, and it was also largely used by the American observers on the same occasion.

Monsieur Loëwy at Paris proposed in 1871 a most ingenious telescope made by a combination of two plane mirrors and an achromatic object-glass, which he calls a Coudé telescope, which has some most important advantages. Chief amongst these are that the observer sits in perfect comfort at the upper end of the polar axis, whence he need not move, and by suitable arrangements he can direct the telescope to any part of the visible heavens. Several have been made in France, including a large one of 24 inches aperture, erected at the Paris Observatory, and which has already made its mark by the production of perhaps the best photographs of the moon yet obtained. I have already spoken of Lord Lindsay and his 40-foot telescope, fed, as it were, with light from a heliostat. This is exactly the plan that has been followed in the design of the large telescope in the Paris Exhibition. But in place of a lens of 4 inches aperture and a heliostat a few inches larger, the Paris telescope has a plane mirror of 6 feet and a lens exceeding 4 feet in diameter, with a focal length of 186 feet. The cost of a mounting on the German plan and of a dome to shelter such an instrument would have been enormous. The form chosen is at once the best and cheapest. One of the great disadvantages is that from the nature of things it cannot take in the whole of the heavens. The heliostat form of mounting of the plane mirror causes a rotation of the image in the field of view which in many lines of research is a strong objection. There is much to be said on the other side. The dome is dispensed with, the tube, the equatorial mounting and the rising floor are not wanted. The mechanical arrangements of importance are confined to the mounting of the necessary machinery to carry the large plane mirror and move it round at the proper rate. The telescope need not have any tube (that to the Paris telescope is, of course, only placed there for effect), as the flimsiest covering is enough if it excludes false light falling on the eye-end; and, more important than all, the observer sits at his ease in the dark chamber. This question of the observer, and the conditions under which he observes, is a most important one as regards both the quality and quantity of the work done.

We have watched the astronomer, first observing from the floor level, then mounted on a high scaffold like Sir William Herschel, Lassell and Lord Rosse; then, starting again from the floor level and using the early achromatic telescope; then, as these grew in size, climbing up on observing chairs to suit the various positions of the eye-end of the telescope, as we see in Mr. Newall's great telescope; then brought to the floor again by that excellent device of Sir Howard Grubb, the rising floor. This is in use with the Lick and the Yerkes telescopes, where the observer is practically always on the floor level, though constant attention is needed, and the circular motion has to be provided for by constant movement, to say nothing of the danger of the floor going wrong. Then we have the ideal condition, as in the Equatorial Coudé at the Paris Observatory, where the observer sits comfortably sheltered and looks down the telescope, and from this position can survey the whole of the visible heavens. The comfort of the observer is a most important matter, especially for the long exposures that are given to photographic plates, as well as for continued visual work. In such a form of telescope as that at Paris the heliostat form of mounting the plane mirror is most suitable, notwithstanding the rotation of the image. But there is another way in which a plane mirror can be mounted, and that is on the plan first proposed by Auguste many years ago, and lately brought forward again by Mons. Lippman, of Paris, and that is by simply mounting the plane mirror on a polar axis and parallel therewith, and causing this mirror to rotate at half the speed of the earth's rotation Any part of the heavens seen by any person reflected from this mirror will appear to be fixed in space, and not partake of the apparent movement of the earth, so long as the mirror is kept moving at this rate. A telescope, therefore, directed to such a mirror can observe any heavenly body as if it were in an absolutely fixed position so long as the angle of the mirror shall not be such as to make the reflected beam less than will fill the object-glass. There is one disadvantage in the cœlostat, as this instrument is called, and that is its suitability only for regions near the equator. The range above and below, however, is large enough to include the greater portion of the heavens, and that portion in which the solar system is included. Here the telescope must be moved in azimuth for different portions of the sky, as is fully explained by Prof. Turner in vol. lvi. of the *Monthly Notices*, and it therefore becomes necessary to provide for moving the telescope in azimuth from time to time as different zones above or below the equator are observed. No instrument yet devised is suitable for all kinds of work, but this form, notwithstanding its defects, has so many and such important advantages that I think it will obviate the necessity of building any larger refractors on the usual models. The cost of producing a telescope much larger than the Yerkes on that model, in comparison with what could be done on the plan I now advocate, renders it most improbable that further money will be spent in that way. It may be asked, What are the lines of research which could be taken up by a telescope of this construction, and on what lines should the telescope be built? I will endeavour to answer this. All the work that is usually done by an astronomical telescope, excepting very long-continued observations, can be equally wel done by the fixed telescope. But there are some special lines for which this form of research is admirably suited, such as photographs of the moon, which would be possible with a reflecting mirror of, say, 200 feet focal length, giving an image of some 2 feet diameter in a primary focus, or a larger image might be obtained either by a longer focus mirror or by a combination. It might even be worth while to build a special cœlostat for lunar photography, provided with an adjustment to the polar axis and a method of regulating the rate of clock to correct the irregular motion of the moon, and thus obtain absolutely fixed images on the photographic plate.

The advantage of large primary images in photography is now fully recognised. For all other kinds of astronomical photography a fixed telescope is admirably adapted; and so with all spectroscopic investigations, a little consideration will show that the conditions under which these investigations can be pursued are almost ideal. As to the actual form such a construction would take, we can easily imagine it. The large mirror mounted as a cœlostat in the centre; circular tracts round this centre, on which a fan-shaped house can be travelled round to any azimuth, containing all the necessary apparatus for utilising the light from the large plane mirror, so as to be easily moved round to the required position in azimuth for observation. In place of a fan-shaped house movable round the plane mirror, a permanent house might encircle the greater portion round the mirror, and in this house the telescope or whatever optical combination is used might be arranged on an open framework, supported on similar rails, so as to run round to any azimuth required. The simplicity of the arrangement and the enormous saving in cost would allow in any well-equipped observatory the use of a special instrument for special work. The French telescope has a mirror about 6 feet in diameter and a lens of about 4 feet. This is a great step in advance over the Yerkes telescope, and it may be some time before the glass for a lens greater than 50 inches diameter will be made, as the difficulty in making optical glass is undoubtedly very great. But with the plane mirror there will be no such difficulty, as 6 feet has already been made; and so with a concave mirror there would be little difficulty in beginning with 6 feet or 7 feet. The way in which the mirror would be used, always hanging in a band, is the most favourable condition for good work, and the absence of motion during an observation, except of course that of the plane mirror (which could be given by floating the polar axis and suitable mechanical arrangements, a motion of almost perfect regularity).

One extremely important thing in using silver or glass mirrors is the matter of resilvering from time to time. Up to quite recently the silvering of my 5-foot mirror was a long, uncertain, and expensive process. Now we have a method of silvering mirrors that is certain, quick, and cheap. This takes away the one great disability from the silver or glass reflecting telescope, as the surface of silver can now be renewed with greater ease and in less time than the lenses of a large refracting telescope could be taken out and cleaned. It may be that we shall revert to speculum metal for our mirrors, or use some other deposited metal on glass; but even as it is we have the silvered glass reflector, which at once allows an enormous advance in power. To do justice to any large telescope it should be erected in a position, as regards climate, where the conditions are as favourable as possible.

The invention of the telescope is to me the most beautiful ever made. Familiarity both in making and in using has only increased my admiration. With the exception of the microphone of the late Prof. Hughes, which enabled one to hear otherwise inaudible sounds, sight is the only sense that we have been able to enormously increase in range. The telescope enables one to see distant objects as if they were at, say, one-five-thousandth part of their distance, while the microscope renders visible objects so small as to be almost incredible. In order to appreciate better what optical aid does for the sense of sight, we can imagine the size of an eye, and therefore of a man, capable of seeing in a natural way what the ordinary eye sees by the aid of a large telescope, and, on the other hand, the size of a man and his eye that could see plainly small objects as we see them under a powerful microscope. The man in the first case would be several miles in height, and in the latter he would not exceed a very small fraction of an inch in height.

Photography also comes in as a further aid to the telescope, as it may possibly be to the microscope. For a certain amount of light is necessary to produce sensation in the eye. If this light is insufficient nothing is seen; but owing to the accumulative effect of light on the photographic plate, photographs can be taken of objects otherwise invisible, as I pointed out years ago, for in photographs I took in 1883 stars were shown on photographic plates that I could not see in the telescope. All photographs, when closely examined, are made up of a certain number of little dots, as it were, in the nature of stippling, and it is a very interesting point to consider the relation of the size and separation of these dots that form the image, and the rods and cones of the reckoner which determines the power of the eye.

Many years ago I tried to determine this question. I first took a photograph of the moon with a telescope of very short focus (as near as I could get it to the focus of the eye itself, which is about half an inch). The resulting photograph measured one two-hundredth of an inch in diameter, and when examined again with a microscope showed a fair amount of detail, in fact, very much as we see the moon with the naked eye; making a picture of the moon by hand on such a scale that each separate dot of which it was made corresponded with each separate sensitive point of the retina employed when viewing the moon without optical aid, I found, on looking at this picture at the proper distance, that it looked exactly like a real moon. In this case the distance of the dots was constant, making them larger or smaller forming the light or shade of the picture.

I did not complete these experiments, but as far as I went I thought that there was good reason to believe that we could in this way increase the defining power of the eye. It is a subject well worthy of further consideration.

I know that in this imperfect and necessarily brief address I have been obliged to omit the names of many workers, but I cannot conclude without alluding to the part that this Association has played in fostering and aiding Astronomy. A glance through the list of money grants shows that the help has been most liberal. In my youth I recollect the great value that was put on the British Association Catalogue of Stars; we know the help that was given in its early days to the Kew Observatory; and the Reports of the Association show the great interest that has always been taken in our work. The formation of a separate Department of Astronomy is, I hope, a pledge that this interest will be continued, to the advantage of our science.

List of Large Telescopes in existence in 1900.

Refractors 15 inches and upwards	Inches	Refractors 15 inches and upwards	Inches
Paris (Exhibition)	50	Mount Etna	21·8
Yerkes	40	Strassburg	19·1
Lick	36	Milan	19·1
Pulkowa	30	(Dearborn) Chicago	18·5
Nice	29·9	Warner Observatory, Rochester, U.S.	16·0
Paris	28·9	Washburn Observatory, Madison, Wisconsin	15·5
Greenwich	28·0	Edinburgh	15·1
Vienna	27·0	Brussels	15·1
Washington, U.S.	26·0	Madrid	15·0
Leander, McCormick Observatory, Virginia	26·0	Rio Janeiro	15·0
Greenwich	26·0	Paris	15·0
Newall's, Cambridge.	25·0	Sir William Huggins.	15·0
Cape of Good Hope	24·0	Paris	15·0
Harvard	24·0		
Princeton, N.J., U.S.	23·0		

Reflectors 2 feet 6 inches and upwards	Ft.	In.	Reflectors 2 feet 6 inches and upwards	Ft.	In.
Lord Rosse	6	0	South Kensington	3	0
Dr. Common	5	0	Crossley (Lick).	3	0
Melbourne	4	0	Greenwich	2	6
Paris	4	0	South Kensington	2	6
Meudon	3	3			

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. W. H. PERKIN, JUN., PH.D., F.R.S., PRESIDENT OF THE SECTION.

The Modern System of Teaching Practical Inorganic Chemistry and its Development.

IN choosing for the subject of my Address to-day the development of the teaching of practical inorganic chemistry I do so, not only on account of the great importance of the subject, but also because it does not appear that this matter has been brought before this Section, in the President's Address at all events, during the last few years.

In dealing generally with the subject of the teaching of chemistry as a branch of science it may be well in the first place to consider the value of such teaching as a means of general education, and to turn our attention for a few minutes to the development of the teaching of science in schools.

There can be no doubt that there has been great progress in the teaching of science in schools during the last forty years, and this is very evident from the perusal of the essay, entitled "Education: Intellectual, Moral, and Physical," which Herbert Spencer wrote in 1859. After giving his reasons for considering the study of science of primary importance in education, Herbert Spencer continues: "While what we call civilisation could never have arisen had it not been for science, science forms scarcely an appreciable element in our so-called civilised training."

From this it is apparent that science was not taught to any appreciable extent in schools at that date, though doubtless in some few schools occasional lectures were given on such scientific subjects as physiology, anatomy, astronomy, and mechanics.

Herbert Spencer's pamphlet appears to have had only a very gradual effect towards the introduction of science into schemes of education. For many years chemical instruction was only given in schools at the schoolroom desk, or at the best from the lecture table, and many of the most modern of schools had no laboratories.

The first school to give any practical instruction in chemistry was apparently the City of London School, at which, in the year 1847, Mr. Hall was appointed teacher of chemistry, and there he continued to teach until 1869.[1] Besides the lecture theatre and a room for storing apparatus, Mr. Hall's department contained a long room, or rather passage, leading into the lecture theatre, and closed at each end with glass doors. In this room, which was fitted up as a laboratory, and used principally as a preparation room for the lectures, Mr. Hall performed experiments with the few boys who assisted him with his lectures. As accommodation was at that time strictly limited, he used to suggest simple experiments and encourage the boys to carry them out at home, and afterwards he himself would examine the substances which they had made.

From this small beginning the teaching of chemistry in the City of London School rapidly developed, and this school now possesses laboratories which compare favourably with those of any school in the country.

The Manchester Grammar School appears to have been one of the first to teach practical chemistry. In connection with this school a small laboratory was built in 1868; this was replaced by a larger one in 1872, and the present large laboratories, under the charge of Mr. Francis Jones, were opened in 1880.

Dr. Marshall Watts, who was the first science master in this school, taught practical chemistry along with the theoretical work from the commencement in 1868.

As laboratories were gradually multiplied it might be supposed that boys were given the opportunity to carry out experiments which had a close connection with their lecture-room courses. But the programme of laboratory work which became all but universal was the preparation of a few gases, followed by the practice of qualitative analysis. The course adopted seems to have been largely built up on the best books of practical chemistry in use in the colleges at that time; but it was also, no doubt, largely influenced by the requirements of the syllabus of the Science and Art Department, which contained a scheme for teaching practical chemistry.[1] Even down to quite recent times it was in many schools still not considered essential that boys should have practical instruction in connection with lectures in chemistry.

[1] Mr. A. T. Pollard, M.A., Head Master of the City of London School, has kindly instituted a search among the bound copies of the boys' terminal reports, and informs me that in the School form of Terminal Report a heading for Chemistry was introduced in the year 1847, the year of Mr. Hall's appointment.

A Report issued in 1897 by a special Committee appointed by the Technical Education Board of the London County Council adduces evidence of this from twenty-five secondary schools in London, in which there were 3960 boys learning chemistry. Of these 1698 boys, or 43 per cent., did no practical work whatever; 955 boys, or 24 per cent., did practical work, consisting of a certain amount of preparation of gases, together with qualitative analysis; but of these latter 743, or 77 per cent., had not reached the study of the metals in their theoretical work, so that their testing work can have been of little educational value. It was also found that in the case of 655, or 68 per cent. of the total number of boys taking practical work, the first introduction to practical chemistry was through qualitative analysis.

But some years before this Report was issued a movement had begun which was destined to have a far-reaching effect. A Report "on the best means for promoting Scientific Education in Schools" having been presented to the Dundee Meeting of this Association in 1867, and published in 1868, a Committee of the British Association was appointed in 1887 "for the purpose of inquiring and reporting upon the present methods of teaching chemistry." The well-known Report which this Committee presented to the Newcastle Meeting in 1889 insisted that it was worth while to teach chemistry in schools, not so much for the usefulness of the information imparted as for the special mental discipline it afforded if the scientific method of investigating nature were employed. It was argued that "learners should be put in the attitude of discoverers, and led to make observations, experiments, and inferences for themselves." And since there can be little progress without measurement, it was pointed out that the experimental work would necessarily be largely of a quantitative character.

Prof. H. E. Armstrong, in a paper read at a conference at the Health Exhibition five years before this, had foreshadowed much that was in this Report. He also drew up a detailed scheme for "a course of elementary instruction in physical science," which was included in the Report of the Committee, and it cannot be doubted that this scheme and the labours of the Committee have had a very marked influence on the development of the teaching of practical chemistry in schools. That this influence has been great will be admitted when it is understood that schemes based on the recommendation of the Committee are now included in the codes for both Elementary Day Schools and Evening Continuation Schools. The recent syllabuses for elementary and advanced courses issued by the Incorporated Association of Headmasters and by the Oxford and Cambridge local boards and others are evidently directly inspired by the ideas set forth by the Committee.

The Department of Science and Art has also adopted some of the suggestions of the Committee, and a revised syllabus was issued by the Department in 1895, in which qualitative analysis is replaced by quantitative experiments of a simple form, and by other exercises so framed "as to prevent answers being given by students who have obtained their information from books or oral instruction." This was a very considerable advance but it must be admitted that there is nothing in the syllabus which encourages, or even suggests, placing the learners in the attitude of discoverers, and this, in the opinion of the Committee of this Association, is vital if the teaching is to have educational value.

Many criticisms have been passed upon the 1889 Report. It has been said that life is much too short to allow of each individual advancing from the known to the unknown, according to scientific methods, and that even were this not so too severe a tax is made upon the powers of boys and girls. In answer to the second point it will be conceded that while it is doubtless futile to try to teach chemistry to young children, on the other hand experience has abundantly shown that the average schoolboy of fourteen or fifteen can, with much success, investigate such problems as were studied in the researches of Black and Scheele, of Priestley and Cavendish and Lavoisier, and it is quite remarkable with what interest such young students carry out this class of work.

It may be well to quote the words which Sir Michael Foster

[1] I find, on inquiry, that examinations in the Advanced Stage and Honours of Practical Chemistry were first held by the Science and Art Department in 1878, the practical examination being extended to the Elementary Stage in 1882.

used in this connection in his admirable Presidential Address to this Association in 1899. He said: "The learner may be led to old truths, even the oldest, in more ways than one. He may be brought abruptly to a truth in its finished form, coming straight to it like a thief climbing over a wall; and the hurry and press of modern life tempt many to adopt this quicker way. Or he may be more slowly guided along the path by which the truth was reached by him who first laid hold of it. It is by this latter way of learning the truth, and by this alone, that the learner may hope to catch something at least of the spirit of the scientific inquirer."

I believe that in the determination of a suitable school course in experimental science this principle of historical development is a very valuable guide, although it is not laid down in the 1889 Report of the British Association.

The application of this principle will lead to the study of the solvent action of water, of crystallisation, and of the separation of mixtures of solids before the investigation of the composition of water, and also before the investigation of the phenomena of combustion. It will lead to the investigation of hydrochloric acid before chlorine, and especially to the postponement of atomic and molecular theories, chemical equations, and the laws of chemical combination, until the student has really sufficient knowledge to understand how these theories came to be necessary.

There can be no doubt that this new system of teaching chemistry in schools has been most successful. Teachers are delighted with the results which have already been obtained, and those whom I have had the opportunity of consulting, directly and indirectly, cannot speak too highly of their satisfaction at the disappearance of the old system of qualitative analysis, and the institution of the new order of things. Especially I may mention in this connection the excellent work which is being carried on under the supervision of Dr. Bevan Lean at the Friends' School in Ackworth, where the boys have attained results which are far in advance of anything which would have been thought possible a few years since.

It is, of course, obvious that if a schoolboy is made to take the attitude of a discoverer, his progress may appear to be slow. But does this matter? Most boys will not become professional chemists; but if while at school a boy learns how to learn, and how to "make knowledge"[1] by working out for himself a few problems, a habit of mind will be formed which will enable him in future years to look in a scientific spirit at any new problems which may face him. When school-days are past the details of the preparation of hydrogen may have been forgotten; but if it was really understood at the time that it could not be decided at once whether the gas was derived from the acid or from the metal, or from the water, or in part from the one and in part from the other, an attitude of scepticism and of suspended judgment will have been formed, which will continue to guard from error.

In the new system of teaching chemistry in schools much attention must necessarily be given to weights and measurements; indeed, the work must be largely of a quantitative kind, and it is in this connection that an important note of warning has been sounded by several teachers.[2] They consider, very rightly, that it is important to point out clearly to the scholar that science does not consist of measurement, but that measurement is only a tool in the hand of the inquirer, and that when once sufficient skill has been developed in its use it should be employed only with a distinct object. Measurements should, in fact, be made only in reference to some actual problem which appears to be really worth solving, not in the accumulation of aimless details.

And, of course, all research carried out must be genuine and not sham, and all assumption of the "obvious" must be most carefully guarded against. But the young scholar must, at the same time, not forget that although the scientific method is necessary to enable him to arrive at a result, in real life it is the answer to the problem which is of the most importance.[3]

Although, then, there has been so much discussion, during the last ten years, on the subject of teaching chemistry in schools, and such steady progress has been made towards devising a really satisfactory system of teaching the subject to young boys and girls, it is certainly very remarkable that practically nothing has been said or written bearing on the training which a student who wishes to become a chemist is to undertake at the close of his school-days at the college or university in which his education is continued.

One of the most remarkable points, to my mind, in connection with the teaching of chemistry, is the fact that although the science has been advancing year by year with such unexampled rapidity, the course of training which the student goes through during his first two years at most colleges is still practically the same as it was thirty or forty years ago. Then, as now, after preparing a few of the principal gases, the student devotes the bulk of his first year to qualitative analysis in the dry and wet way, and his second year to quantitative analysis, and, although the methods employed in teaching the latter may possibly have undergone some slight modification, there is certainly no great difference between the routine of simple salt and mixture followed by quantitative analysis practised at the present day and that which was in vogue in the days of our fathers and grandfathers.

Since, then, the present system has held the field for so long, not only in this country but also on the Continent, it is worth while considering whether it affords the best training which a student who wishes to become a chemist can undergo in the short time during which he can attend at a college or university. In considering this matter I was led in the first place to carefully examine old books and other records, with the object of finding out how the present system originated, and I think that valuable and interesting information bearing on the subject may be obtained from a very brief sketch of the rise and development of the present system of teaching chemistry, and especially in so far as it bears on the inclusion of qualitative analysis. Unfortunately, it is not so easy to gain a good historical acquaintance with the matter as I at first imagined would be the case, and this is due in a large measure to the fact that so few of the laboratories which took an active part in the development of the present system of chemical training have left any record of the methods which they employed. In this connection I may, perhaps, be allowed to suggest that it would be a valuable help to the future historian if all prominent teachers of chemistry would leave behind them a brief record of the system of teaching adopted in their laboratories, showing the changes which they had instituted, the object of these changes, and the results which followed their adoption.

There is no doubt that the progress of practical chemistry went largely hand in hand with the progress of theoretical chemistry, for as the latter gradually developed, so the necessity for the determination of the composition, first of the best known, and then of the rarer minerals and other substances, became more and more marked.

The analytical examination of substances in the dry way was employed in very early times in connection with metallurgical operations, and especially in the determination of the presence of valuable constituents in samples of minerals. Cupellation was used by the Greeks in the separation of gold and silver from their ores and in the purification of these metals. Geber knew that the addition of nitre to the ore facilitated the separation of gold and silver, and subsequently Glauber (1604–1668) called attention to the fact that many commoner metals could easily be separated from their ores with the aid of nitre.

But it was not till the eighteenth century that any marked progress was made in analysis in the dry way, and the progress which then became rapid was undoubtedly due to the discovery of the blowpipe, and to the introduction of its use into analytical operations. The blowpipe is mentioned for the first time in 1660, in the transactions of the Accademia del Cimento of Florence, but the first to recommend its use in chemical operations was Johann Andreas Cramer in 1739. The progress of blowpipe analysis was largely due to Gahn (1745–1818), who spent much time in perfecting its use in the examination of minerals, and it was he who first used platinum wire and cobalt solution in connection with blowpipe analysis. The methods employed by Gahn were further developed by his friend Berzelius (1779–1848), who gave much attention to the matter, and who with great skill and patience gradually worked out a complete scheme of blowpipe analysis, and published it in a pamphlet, entitled "Ueber die Anwendung des Löthrohrs," which appeared in 1820. After the publication of this work blowpipe analysis rapidly came into general use in England France and Germany, and the scheme devised by Berzelius is essentially that employed at the present day.

[1] *Cf.* Prof. J. G. Macgregor in NATURE, September 1899.
[2] *Cf.* H. Picton in *The School World*, November 1899; Bevan Lean, *ibid.*, February 1900.
[3] *Cf.* Mrs. Bryant, "Special Reports on Educational Subjects," vol. ii. p. 113.

Indeed, the only notable additions to the methods of analysis in the dry way since the time of Berzelius are the development of flame reactions, which Bunsen worked out with such characteristic skill and ingenuity, and the introduction of the spectroscope.

The necessity for some process other than that of analysis in the dry way seems, in the first instance, to have arisen in quite early times in connection with the examination of drugs, not only on account of the necessity for discovering their constituents, but also as a means of determining whether they were adulterated. In such cases analysis in the dry way was obviously unsuitable, and experience soon showed that the only way to arrive at the desired result was to treat the substance under examination with aqueous solutions of definite substances, the first reagent apparently being a decoction of gallnuts, which is described by Pliny as being employed in detecting adulteration with green vitriol.

The progress made in connection with wet analysis was, however, exceedingly slow, largely owing to the lack of reagents; but as these were gradually discovered wet analysis rapidly developed, especially in the hands of Tachenius, Scheele, Boyle, Hoffman, Margraf, and Bergmann. Boyle (1626–1691) especially had an extensive knowledge of reagents and their application; and, indeed, it was Boyle who first introduced the word "analysis" for those operations by which substances may be recognised in the presence of one another. Boyle knew how to test for silver with hydrochloric acid, for calcium salts with sulphuric acid, and for copper by the blue solution produced by ammonia.

Margraf (1709–1782) introduced prussiate of potash for the detection of iron, and Bergmann (1735–1784) not only introduced new reagents and new methods for decomposing minerals and refractory substances, such as fusion with potash, digestion with nitric acid or hydrochloric acid, but he also was the first to suggest the application of tests in a systematic way, and, indeed, the method of analysis which he developed is on much the same lines as that in use at the present day. He paid special attention to the qualitative analysis of minerals, and gave careful instructions for the analysis of gold, platinum, silver, lead, copper, zinc, and other ores. The work of Scheele (1742–1786) had indirectly a great influence on qualitative analysis, as, although he did not give a general systematic method of procedure in the analysis of substances of unknown composition, yet the methods which he employed in the examination of new substances were so original and exact as to remain models of how qualitative analysis should be conducted.

Great strides in analytical chemistry in the wet way were made through the work of Berzelius, who, by the discovery of new methods, such as the decomposition of silicates by hydrofluoric acid and the introduction of new tests, greatly advanced the art. He paid special attention to perfecting the methods of analysis of mineral waters, and these researches, as well as his work on ores, and particularly his investigation of platinum ores, stamp Berzelius as one of the great pioneers in qualitative and quantitative analytical chemistry.

By the labours of the great experimenters whom I have mentioned qualitative analysis gradually acquired the familiar appearance of to-day, and many books were written with the object of arranging the mass of information which had accumulated, and of thus rendering it available for the student in his efforts to investigate the composition of new minerals and other substances. Among these books may be mentioned the "Handbuch der analytischen Chemie," by H. Rose, and especially the well-known analytical text-books of Fresenius, which have had an extraordinarily wide circulation and passed through many editions.

The work of the great pioneers in analytical chemistry was work done often under circumstances of great difficulty, as before the end of the seventeenth century there were no public institutions of any sort in which a practical knowledge of chemistry could be acquired. Lectures were, of course, given from very early times, but it was not until the time of Guillaume François Rouelle (1703–1770), at the beginning of the eighteenth century, that lectures began to be illustrated by experiments. Rouelle, who was very active as a teacher, numbered among his pupils many men of eminence, such as Lavoisier and Proust, and it was largely owing to his influence that France took such a lead in practical teaching. In Germany progress was much slower, and in our country the introduction of lectures illustrated by experiments seems to have been mainly due to Davy.

When it is considered how slowly experimental work came to be recognised as a means of illustration and education, even in connection with lectures, it is not surprising that in early times practical teaching in laboratories should have been thought quite unnecessary.

The few laboratories which existed in the sixteenth century were built mainly for the practice of alchemy by the reigning princes of the time, and, indeed, up to the beginning of the nineteenth entury, the private laboratories of the great masters were the only schools in which a favoured few might study, but which were not open to the public. Thus we find that Berzelius received in his laboratory a limited number of students who worked mostly at research; these were not usually young men, and his school cannot thus be considered as a teaching institution in the ordinary sense of the word.

The earliest laboratory open for general instruction in Great Britain was that of Thomas Thomson, who, after graduating in Edinburgh in 1799, began lecturing in that city in 1800, and opened a laboratory for the practical instruction of his pupils. Thomson was appointed lecturer in Chemistry in Glasgow University in 1807, and Regius Professor in 1818, and in Glasgow he also opened a general laboratory.

The first really great advance in laboratory teaching is due to Liebig, who, after working for some years in Paris under Gay-Lussac, was appointed in 1824 to be Professor of Chemistry in Giessen. Liebig was strongly impressed with the necessity for public institutions where any student could study chemistry, and to him fell the honour of founding the world-famed Giessen Laboratory, the first public institution in Germany which brought practical chemistry within the reach of all students.

Giessen rapidly became the centre of chemical interest in Germany, and students flocked to the laboratory in such numbers as to necessitate the development of a systematic course of practical chemistry, and in this way a scheme of teaching was devised which, as we shall see later, has served as the foundation for the system of practical chemistry in use at the present day.

When the success of this laboratory had been clearly established, many other towns discovered the necessity for similar institutions, and in a comparatively short time every university in Germany possessed a chemical laboratory. The teaching of practical chemistry in other countries was, however, of very slow growth; in France, for example, Wurtz in 1869 drew attention to the fact that there was at that time only one laboratory which could compare with the German laboratories, namely, that of the École normale supérieure.

In this country the provision of suitable laboratories for the study of chemistry seems to date from the year 1845, when the College of Chemistry was founded in London, an institution which under A. W. Hofmann's guidance rapidly rose to such a prominent position.

In 1851 Frankland was appointed to the chair of chemistry in the new college founded in Manchester by the trustees of John Owens, and here he equipped a laboratory for the teaching of practical chemistry. Under Sir Henry Roscoe this laboratory soon became too small for the growing number of chemical students, a defect which was removed when the new buildings of the college were opened in 1873. In 1849 Alexander Williamson was appointed Professor of Practical Chemistry at University College, London, where he introduced the practical methods of Liebig.

Following these examples, the older universities gradually came to see the necessity for providing accommodation for the practical teaching of chemistry, with the result that well-equipped laboratories have been erected in all the centres of learning in this country.

Since Liebig, by the establishment of the Giessen Laboratory, must be looked upon as the pioneer in the development of practical laboratory teaching, it will be interesting to endeavour to obtain some idea of the methods which he used in the training of the students who attended his laboratory in Giessen. From small beginnings he gradually introduced a systematic course of practical chemistry, and a careful comparison shows that this was similar in many ways to that in use at the present day. The student at Giessen, after preparing the more important gases, was carefully trained in qualitative and quantitative analysis: he was then required to make a large number of preparations, after which he engaged in original research.

Although there is, as far as I have been able to ascertain, no printed record of the nature of the quantitative work and the preparations which Liebig required from his students, the course of qualitative analysis is easily followed, owing to the existence of a most interesting book published for the use of the Giessen students.

In 1846, at Liebig's request, Henry Will, Ph.D., Extraordinary Professor of Chemistry in the University of Giessen, wrote a small book, for use at Giessen, called "Giessen Outlines of Analysis," which shows clearly the kind of instruction given in that laboratory at the time in so far as qualitative analysis is concerned. This book, which contains a preface by Liebig, is particularly interesting on account of the fact that it is evidently the first Introduction to Analysis intended for the training of elementary students which was ever published. In the preface Liebig writes: "The want of an introduction to chemical analysis adapted for the use of a laboratory has given rise to the present work, which contains an accurate description of the course I have followed in my laboratory with great advantage for twenty-five years It has been prepared at my request by Prof. Will, who has been my assistant during a great part of this period."

This book undoubtedly had a considerable circulation, and was used in most of the laboratories which were in existence at that time, and thus we find, for example, that the English translation which Liebig "hopes and believes will be acceptable to the English public" was the book used by Hofmann for his students at the College of Chemistry. In this book the metals are first divided into groups much in the same way as is done now; each group is then separately dealt with, the principal characteristics of the metals of the group are noted, and their reactions studied. Those tests which are useful in the detection of each metal are particularly emphasised, and the reasons given for selecting certain of them as of special value for the purposes of separating one metal from another.

Throughout this section of the book there are frequent discussions as to the possible methods of the separation, not only of the metals of one group, but of those belonging to different groups; and the whole subject is treated in a manner which shows clearly that Liebig's great object was to make the student think for himself. After studying in a similar manner the behaviour of the principal acids with reagents, the student is introduced to a course of qualitative analysis comprising (1), preliminary examination of solids (2), qualitative analysis of the substance in solution.

Both sections are evidently written with the object, not only of constructing a system of qualitative analysis, but more particularly of clearly leading the student to argue out for himself the methods of separation which he will ultimately adopt. The book concludes with a few tables which differ considerably in design from those in use at the present day, and which are so meagre that the student could not possibly have used them mechanically.

The system introduced in this book, no doubt owing to the excellent results obtained by its use, was rapidly recognised as the standard method of teaching analysis in most of the institutions existing at that time. Soon the course began to be further developed, book after book was published on the subject, and gradually the teaching of qualitative analysis assumed the shape and form with which we are all so well acquainted. But the present-day book on qualitative analysis differs widely from "Giessen Outlines" in this respect, that whereas in the latter the tables introduced are mere indications of the methods of separation to be employed, and are of such a nature that the student who did not think for himself must have been constantly in difficulties, in the book of the present day these tables have been worked out to the minutest detail. Every contingency is provided for; nothing is left to the originality of the student; and that which, no doubt, was once an excellent course has now become so hopelessly mechanical as to make it doubtful whether it retains anything of its former educational value.

The question which I now wish to consider more particularly is whether the system of training chemists which is at present adopted, with little variation, in our colleges and universities is a really satisfactory one, and whether it supplies the student with the kind of knowledge which will be of the most value to him in his future career.

Those who study chemistry may be roughly divided as to their future careers into two groups—those who become teachers and those who become technical chemists. Now, whether the student takes up either one or the other career, I think that it is clear that the objects to be aimed at in training him are to give him a sound knowledge of his subject, and especially to so arrange his studies as to bring out in every possible way his capacity for original thought.

A teacher who has no originality will hardly be successful, even though he may possess a very wide knowledge of what has already been done in the past. He will have little enthusiasm for his subject, and will continue to teach on the lines laid down by the text-books of the day, without himself materially improving the existing methods, and, above all, he will be unable, and will have no desire, to add to our store of knowledge by original investigation.

It is in the power of almost every teacher to do some research work, and it seems probable that the reason why more is not done by teachers is because the importance of research work was not sufficiently insisted on, and their original faculty was not sufficiently trained, at the schools and colleges where they received their education.

And these remarks apply with equal force to the student who subsequently becomes a technical chemist.

In the chemical works of to-day sound knowledge is essential, but originality is an even more important matter. A technical chemist without originality can scarcely rise to a responsible position in a large works; whereas a chemist who is capable of constantly improving the processes in operation, and of adding new methods to those in use, becomes so valuable that he can command his own terms.

Now, this being so, I think it is extraordinary that so many of the students who go through the prescribed course of training —say for the Bachelor of Science degree—not only show no originality themselves, but seem also to have no desire at the conclusion of their studies to engage in original investigation under the supervision of the teacher. That this is so is certainly my experience as a teacher examiner, and I feel sure that many other teachers will endorse this view of the case.

If we inquire into the reason for this deficiency in originality, we shall, I think, be forced to conclude that it is in a large measure due to the conditions of study and the nature of the courses through which the student is obliged to pass.

A well-devised system of quantitative analysis is undoubtedly valuable in teaching the student accurate manipulation, but it has always seemed to me that the long course of qualitative analysis which is usually considered necessary, and which generally precedes the quantitative work, is not the most satisfactory training for a student.

There can be no doubt that to many students qualitative analysis is little more than a mechanical exercise: the tables of separation are learnt by heart, and every substance is treated in precisely the same manner: such a course is surely not calculated to develop any original faculty which the student may possess. Then, again, when the student passes on to quantitative analysis, he receives elaborate instructions as to the little details he must observe in order to get an accurate result; and even after he has become familiar with the simpler determinations he rarely attempts, and indeed has no time to attempt, anything of the nature of an original investigation in qualitative or quantitative analysis. It indeed sometimes happens that a student at the end of his second year has never prepared a pure substance, and is often utterly ignorant of the methods employed in the separation of substances by crystallisation; he has never conducted a distillation, and has no idea how to investigate the nature and amounts of substances formed in chemical reactions; practically all his time has been taken up with analysis. That this is not the way to teach chemistry was certainly the opinion of Liebig, and in support of this I quote a paragraph bearing on the subject which occurs in a very interesting book on "Justus von Liebig: his Life and Work," written by W. A. Shenstone (pp. 175, 176).

"In his practical teaching Liebig laid great stress on the producing of chemical preparations; on the students preparing, that is to say, pure substances in good quantity from crude materials. The importance of this was, even in Liebig's time, often overlooked; and it was, he tells us, more common to find a man who could make a good analysis than to find one who could produce a pure preparation in the most judicious way."

"There is no better way of making one's self acquainted with the properties of a substance than by first producing it from the raw material, then converting it into its compounds, and so becoming acquainted with them. By the study of ordinary analysis one does not learn how to use the important methods of crystallisation, fractional distillation, nor acquire any considerable experience in the proper use of solvents. In short, one does not, as Liebig said, become a chemist."

One reason why the present system of training chemists has persisted so long is no doubt because it is a very convenient system : it is easily taught, does not require expensive apparatus, and, above all, it lends itself admirably for the purpose of competitive examination.

The system of examination which has been developed during the last twenty years has done much harm, and is a source of great difficulty to any conscientious teacher who is possessed of originality, and is desirous, particularly in special cases, of leaving the beaten track.

In our colleges and universities most of the students work for some definite examination—frequently for the Bachelor of Science degree—either at their own University or at the University of London.

For such degrees a perfectly definite course is prescribed and must be followed, because the questions which the candidate will have to answer at his examination are based on a syllabus which is either published or is known by precedent to be required. The course which the teacher is obliged to teach is thus placed beyond his individual power of alteration, except in minor details, and originality in the teacher is thereby discouraged : he knows that all students must face the same examination, and he must urge the backward man through exactly the same course as his more talented neighbour.

In almost all examinations salts or mixtures of salts are given for qualitative analysis. "Determine the constituents of the simple salt A and of the mixture B" is a favourite examination formula ; and as some practical work of this sort is sure to be set, the teacher knows that he must contrive to get one and all of his students into a condition to enable them to answer such questions.

If, then, one considers the great amount of work which is required from the present-day student, it is not surprising that every aid to rapid preparation for examination should be accepted with delight by the teacher ; and thus it comes about that tables are elaborated in every detail, not only for qualitative analysis in inorganic chemistry, but, what is far worse, for the detection of some arbitrary selection of organic substances which may be set in the syllabus for the examination. I question whether any really competent teacher will be found to recommend this system as one of educational value or calculated to bring out and train the faculty of original thought in students.

If, then, the present system is so unsatisfactory, it will naturally be asked, How are students to be trained, and how are they to be examined so as to find out the extent of the knowledge of their subject which they have acquired ?

In dealing with the first part of the question—that is, the training best suited to chemists—I can, of course, only give my own views on the subject—views which, no doubt, may differ much from those of many of the teachers present at this meeting. The objects to be attained are, in my opinion, to give the student a sufficient knowledge of the broad facts of chemistry, and at the same time so to arrange his practical work in particular as to always have in view the training of his faculty of original thought.

I think it will be conceded that any student, if he is to make his mark in chemistry by original work, must ultimately specialise in some branch of the subject. It may be possible for some great minds to do valuable original work in more than one branch of chemistry, but these are the exceptions ; and as time goes on, and the mass of facts accumulates, this will become more and more impossible. Now a student at the commencement of his career rarely knows which branch of the subject will fascinate him most, and I think, therefore, that it is necessary, in the first place, to do all that is possible to give him a thorough grounding in all branches of the subject. In my opinion the student is taken over too much ground in the lecture courses of the present day : in inorganic chemistry, for example, the study of the rare metals and their reactions might be dispensed with, as well as many of the more difficult chapters of physical chemistry, and in organic chemistry such complicated problems as the constitutions of uric acid and the members of the camphor and terpene series, &c., might well be left out. As matters stand now, instruction must be given on these subjects simply because questions bearing on them will probably be asked at the examination.

And here, perhaps, I might make a confession, in which I do not ask my fellow-teachers to join me. My name is often attached to chemistry papers which I should be sorry to have to answer ; and it seems to me the standard of examination papers, and especially of Honours examination papers, is far too high. Should we demand a pitch of knowledge which our own experience tells us cannot be maintained for long ?

In dealing with the question of teaching practical chemistry, it may be hoped, in the first place, that in the near future a sound training will be given in elementary science in most schools, very much on the lines which I mentioned in the first part of this address. The student will then be in a fit state to undergo a thoroughly satisfactory course of training in inorganic chemistry during his first two years at college. Without wishing in any way to map out a definite course, I may be allowed to suggest that instead of much of the usual qualitative and quantitative analysis, practical exercises similar to the following will be found to be of much greater educational value.

(1) The careful experimental demonstration of the fundamental laws of chemistry and physical chemistry.

(2) The preparation of a series of compounds of the more important metals, either from their more common ores or from the metals themselves. With the aid of the compounds thus prepared the reactions of the metals might be studied and the similarities and differences between the different metals then carefully noted.

(3) A course in which the student should investigate in certain selected cases : (*a*) the conditions under which action takes place ; (*b*) the nature of the products formed ; (*c*) the yield obtained. If he were then to proceed to prepare each product in a state of purity, he would be doing a series of exercises of the highest educational value.

(4) The determination of the combining weights of some of the more important metals. This is in most cases comparatively simple, as the determination of the combining weights of selected metals can be very accurately carried out by measuring the hydrogen evolved when an acid acts upon them.

Many other exercises of a similar nature will readily suggest themselves, and in arranging the course every effort should be made to induce the student to consult original papers, and to avoid as far as possible any tendency to mere mechanical work.

The exact nature of such a course must, however, necessarily be left very much in the hands of the teacher, and the details will no doubt require much consideration ; but I feel sure that a course of practical inorganic chemistry could be constructed which, while teaching all the important facts which it is necessary for the student to know, will, at the same time, constantly tend to develop his faculty of original thought.

Supposing such a course were adopted (and the experiment is well worth trying), there still remains the problem of how the student who has had this kind of training is to be examined.

With regard to his theoretical work there would be no difficulty, as the examination could be conducted on much the same lines as at the present time. In the case of the practical examination I have long felt that the only satisfactory method of arriving at the value of a student's practical knowledge is by the inspection of the work which he has done during the whole of his course of study, and not by depending on the results of one or two days' set examination. I think that most examiners will agree with me that the present system of examination in practical chemistry is highly unsatisfactory. This is perhaps not so apparent in the case of the qualitative analysis of the usual simple salt or mixture ; but when the student has to do a quantitative exercise, or when a problem is set, the results sent in are frequently no indication of the value of the student's practical work. Leaving out of the question the possibility of the student being in indifferent health during the short period of the practical examination, it not infrequently happens that he, in his excitement, has the misfortune to upset a beaker when his quantitative determination is nearly finished, and as a result he loses far more marks than he should do for so simple an accident.

Again, in attacking a problem he has usually only time to try one method of solution, and if this does not yield satisfactory results he again loses marks ; whereas in the ordinary course of his practical work, if he were to find that the first method was faulty, he would try other methods until he ultimately arrived at the desired result.

It is difficult to see why such an unsatisfactory system as this might not be replaced by one of inspection which I think could easily be so arranged as to work well.

A student taking, say, a three years' course for the degree of Bachelor of Science might be required to keep very careful notes of all the practical work which he does during this course, and in order to avoid fraud his notebook could from time to time be initialled by the professor or demonstrator in charge of the laboratory. An inspection of these notebooks could then be made at suitable times by the examiners for the degree, by which means a very good idea would be obtained of the scope of the work which the student had been engaged in, and if thought necessary a few questions could easily be asked in regard to the work so presented. Should the examiners wish to further test the candidate by giving him an examination, I submit that it would be much better to set him some exercise of the nature of a simple original investigation, and to allow him two or three weeks to carry this out, than to depend on the hurried work of two or three days.

The object which I had in view in writing this Address was to call attention to the fact that our present system of training in chemistry does not appear to develop in the student the power of conducting original research, and at the same time to endeavour to suggest some means by which a more satisfactory state of things might be brought about. I have not been able, within the limits of this Address, to consider the conditions of study during the third year of the student's career at college, or to discuss the increasing necessity for extending that course and insisting on the student carrying out an adequate original investigation before granting him a degree, but I hope on some future occasion to have the opportunity of returning to this very important part of the subject. If any of the suggestions I have made should prove to be of practical value, and should lead to the production of more original research by our students, I shall feel that a useful purpose has been served by bringing this matter before this Section. In concluding I wish to thank Prof. H. B. Dixon, Prof. F. S. Kipping and others, for many valuable suggestions, and my thanks are especially due to Dr. Bevan Lean for much information which he gave me in connection with that part of this Address which deals with the teaching of chemistry in schools.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY PROF. W. J. SOLLAS, D.SC., LL.D., F.R.S., PRESIDENT OF THE SECTION.

Evolutional Geology.

THE close of one century, the dawn of another, may naturally suggest some brief retrospective glance over the path along which our science has advanced, and some general survey of its present position from which we may gather hope of its future progress; but other connection with geology the beginnings and endings of centuries have none. The great periods of movement have hitherto begun, as it were, in the early twilight hours, long before the dawn. Thus the first step forward, since which there has been no retreat, was taken by Steno in the year 1669; more than a century elapsed before James Hutton (1785) gave fresh energy and better direction to the faltering steps of the young science; while it was less than a century later (1863) when Lord Kelvin brought to its aid the powers of the higher mathematics and instructed it in the teachings of modern physics. From Steno onward the spirit of geology was catastrophic; from Hutton onward it grew increasingly uniformitarian; from the time of Darwin and Kelvin it has become evolutional. The ambiguity of the word "uniformitarian" has led to a good deal of fruitless logomachy, against which it may be as well at once to guard by indicating the sense in which it is used here. In one way we are all uniformitarians, *i.e.* we accept the doctrine of the "uniform action of natural causes," but, as applied to geology, uniformity means more than this. Defined in the briefest fashion it is the geology of Lyell. Hutton had given us a "Theory of the Earth," in its main outlines still faithful and true; and this Lyell spent his life in illustrating and advocating; but as so commonly happens the zeal of the disciple outran the wisdom of the master, and mere opinions were insisted on as necessary dogma. What did it matter if Hutton as a result of his inquiries into terrestrial history had declared that he found no vestige of a beginning, no prospect of an end? It would have been marvellous if he had! Consider that when Hutton's "Theory" was published William Smith's famous discovery had not been made, and that nothing was then known of the orderly succession of forms of life, which it is one of the triumphs of geology to have revealed; consider, too, the existing state of physics at the time, and that the modern theories of energy had still to be formulated; consider also that spectroscopy had not yet lent its aid to astronomy and the consequent ignorance of the nature of nebulæ; and then, if you will, cast a stone at Hutton. With Lyell, however, the case was different: in pressing his uniformitarian creed upon geology he omitted to take into account the great advances made by its sister sciences, although he had knowledge of them, and thus sinned against the light. In the last edition of the famous "Principles" we read: "It is a favourite dogma of some physicists that not only the earth, but the sun itself, is continually losing a portion of its heat, and that as there is no known source by which it can be restored we can foresee the time when all life will cease to exist on this planet, and on the other hand we can look back to a period when the heat was so intense as to be incompatible with the existence of any organic beings such as are known to us in the living or fossil world. . . . A geologist in search of some renovating power by which the amount of heat may be made to continue unimpaired for millions of years, past and future, in the solid parts of the earth . . . has been compared by an eminent physicist to one who dreams he can discover a source of perpetual motion and invent a clock with a self-winding apparatus. *But why should we despair of detecting proofs of such regenerating and self-sustaining power in the works of a Divine Artificer?*" Here we catch the true spirit of uniformity; it admittedly regards the universe as a self-winding clock, and barely conceals a conviction that the clock was warranted to keep true Greenwich time. The law of the dissipation of energy is not a dogma, but a doctrine drawn from observation, while the uniformity of Lyell is in no sense an induction: it is a dogma in the narrowest sense of the word, unproved, incapable of proof; hence perhaps its power upon the human mind; hence also the transitoriness of that power. Again, it is only by restricting its inquiries to the stratified rocks of our planet that the dogma of uniformity can be maintained with any pretence of argument. Directly we begin to search the heavens the possibility, nay even the likelihood, of the nebular origin of our system, with all that it involves, is borne in upon us. Lyell therefore consistently refused to extend his gaze beyond the rocks beneath his feet, and was thus led to do a serious injury to our science: he severed it from cosmogony, for which he entertained and expressed the most profound contempt, and from the mutilation thus inflicted geology is only at length making a slow and painful recovery. Why do I dwell on these facts? To depreciate Lyell? By no means. No one is more conscious than I of the noble service which Lyell rendered to our cause: his reputation is of too robust a kind to suffer from my unskilful handling, and the fame of his solid contributions to science will endure long after these controversies are forgotten. The echoes of the combat are already dying away, and uniformitarians, in the sense already defined, are now no more; indeed, were I to attempt to exhibit any distinguished living geologist as a still surviving supporter of the narrow Lyellian creed, he would probably feel, if such a one there be, that I was unfairly singling him out for unmerited obloquy.

Our science has become evolutional, and in the transformation has grown more comprehensive: her petty parochial days are done, she is drawing her provinces closer around her, and is fusing them together into a united and single commonwealth—the science of the earth.

Not merely the earth's crust, but the whole of earth-knowledge is the subject of our research. To know all that can be known about our planet, this, and nothing less than this, is its aim and scope. From the morphological side geology inquires, not only into the existing form and structure of the earth, but also into the series of successive morphological states through which it has passed in a long and changeful development. Our science inquires also into the distribution of the earth in time and space; on the physiological side it studies the movements and activities of our planet; and not content with all this it extends its researches into ætiology and endeavours to arrive at a science of causation. In these pursuits geology calls all the other sciences to her aid. In our commonwealth there are no outlanders; if an eminent physicist enter our territory we do not begin at once to prepare for war, because the very fact of his undertaking a geological inquiry of itself confers upon him all the duties and privileges of citizenship. A physicist studying geology is by definition a geologist. Our only regret is, not that physicists occasionally invade our borders, but that they do not visit us oftener and make closer acquaintance with us.

Early History of the Earth: First Critical Period.

If I am bold enough to assert that cosmogony is no longer alien to geology, I may proceed further, and taking advantage of my temerity pass on to speak of things once not permitted to us. I propose, therefore, to offer some short account of the early stages in the history of the earth. Into its nebular origin we need not inquire—that is a subject for astronomers. We are content to accept the infant earth from their hands as a molten globe ready made, its birth from a gaseous nebula duly certified. If we ask, as a matter of curiosity, what was the origin of the nebula, I fear even astronomers cannot tell us. There is an hypothesis which refers it to the clashing of meteorites, but in the form in which this is usually presented it does not help us much. Such meteorites as have been observed to penetrate our atmosphere and to fall on to the surface of tha earth prove on examination to have had an eventful history of their own of which not the least important chapter was a passage through a molten state; they would thus appear to be the products rather than the progenitors of a nebula.

We commence our history, then, with a rapidly-rotating molten planet, not impossibly already solidified about the centre and surrounded by an atmosphere of great depth, the larger part of which was contributed by the water of our present oceans, then existing in a state of gas. This atmosphere, which exerted a pressure of something like 5000 lb. to the square inch, must have played a very important part in the evolution of our planet. The molten exterior absorbed it to an extent which depended on the pressure, and which may some day be learnt from experiment. Under the influence of the rapid rotation of the earth the atmosphere would be much deeper in equatorial than polar regions, so that in the latter the loss of heat by radiation would be in excess. This might of itself lead to convectional currents in the molten ocean. The effect on the atmosphere is very difficult to trace, but it is obvious that if a high-pressure area originated over some cooler region of the ocean, the winds blowing out of it would drive before them the cooler superficial layers of molten material, and as these were replaced by hotter lava streaming from below, the tendency would be to convert the high into a low-pressure area, and to reverse the direction of the winds. Conversely under a low-pressure area the in-blowing winds would drive in the cooler superficial layers of molten matter that had been swept away from the anticyclones. If the difference in pressure under the cyclonic and anticyclonic areas were considerable, some of the gas absorbed under the anticyclones might escape beneath the cyclones, and in a later stage of cooling might give rise to vast floating islands of scoria. Such islands might be the first foreshadowings of the future continents. Whatever the ultimate effect of the reaction of the winds on the currents of the molten ocean, it is probable that some kind of circulation was set up in the latter. The universal molten ocean was by no means homogeneous: it was constantly undergoing changes in composition as it reacted chemically with the internal metallic nucleus; its currents would streak the different portions out in directions which in the northern hemisphere would run from north-east to south-west, and thus the differences which distinguish particular petrological regions of our planet may have commenced their existence at a very early stage. Is it possible that as our knowledge extends we shall be able by a study of the distribution of igneous rocks and minerals to draw some conclusions as to the direction of these hypothetical lava currents? Our planet was profoundly disturbed by tides, produced by the sun; for as yet there was no moon; and it has been suggested that one of its tidal waves rose to a height so great as to sever its connection with the earth and to fly off as the infant moon. This event may be regarded as marking the first critical period, or catastrophe if we please, in the history of our planet. The career of our satellite, after its escape from the earth, is not known till it attained a distance of nine terrestrial radii; after this its progress can be clearly followed. At the eventful time of parturition the earth was rotating, with a period of from two to four hours, about an axis inclined at some 11° or 12° to the ecliptic. The time which has elapsed since the moon occupied a position nine terrestrial radii distant from the earth is at least fifty-six to fifty-seven millions of years, but may have been much more. Prof. Darwin's story of the moon is certainly one of the most beautiful contributions ever made by astronomy to geology, and we shall all concur with him when he says, "A theory reposing on *veræ causæ*, which brings into quantitative correlation the length of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance."

The majority of geologists have long hankered after a metallic nucleus for the earth, composed chiefly, by analogy with meteorites, of iron. Lord Kelvin has admitted the probable existence of some such nucleus, and lately Prof. Wiechert has furnished us with arguments—"powerful" arguments Prof. Darwin terms them—in support of its existence. The interior of the earth for four-fifths of the radius is composed, according to Prof. Wiechert, chiefly of metallic iron, with a density of 8·2; the outer envelope, one fifth of the radius, or about 400 miles in thickness, consists of silicates, such as we are familiar with in igneous rocks and meteorites, and possesses a density of 3·2. It was from this outer envelope when molten that the moon was trundled off, twenty-seven miles in depth going to its formation. The density of this material, as we have just seen, is supposed to be 3·2; the density of the moon is 3·39, a close approximation, such difference as exists being completely explicable by the comparatively low temperature of the moon.

The outer envelope of the earth which was drawn off to form the moon was, as we have seen, charged with steam and other gases under a pressure of 5000 lb. to the square inch; but as the satellite wandered away from the parent planet this pressure continuously diminished. Under these circumstances the moon would become as explosive as a charged bomb, steam would burst forth from numberless volcanoes, and while the face of the moon might thus have acquired its existing features, the ejected material might possibly have been shot so far away from its origin as to have acquired an independent orbit. If so we may ask whether it may not be possible that the meteorites, which sometimes descend upon our planet, are but portions of its own envelope returning to it. The facts that the average specific gravity of those meteorites which have been seen to fall is not much above 3·2, and that they have passed through a stage of fusion, are consistent with this suggestion.

Second Critical Period. "Consistentior Status."

The solidification of the earth probably became completed soon after the birth of the moon. The temperature of its surface at the time of consolidation was about 1170° C., and it was therefore still surrounded by its primitive deep atmosphere of steam and other gases. This was the second critical period in the history of the earth, the stage of the "consistentior status," the date of which Lord Kelvin would rather know than that of the Norman Conquest, though he thinks it lies between twenty and forty millions of years ago, probably nearer twenty than forty.

Now that the crust was solid there was less reason why movements of the atmosphere should be unsteady, and definite regions of high and low pressure might have been established. Under the high-pressure areas the surface of the crust would be depressed; correspondingly under the low-pressure areas it would be raised; and thus from the first the surface of the solid earth might be dimpled and embossed."[1]

Third Critical Period. Origin of the Oceans.

The cooling of the earth would continuously progress, till the temperature of the surface fell to 370° C., when that part of the atmosphere which consisted of steam would begin to liquefy; then the dimples on the surface would soon become filled with superheated water, and the pools so formed would expand and deepen, till they formed the oceans. This is the third critical stage in the history of the earth, dating, aecording to Prof. Joly, from between eighty and ninety millions of years ago. With the growth of the oceans the distinction between land and sea arose—in what precise manner we may proceed to inquire. If we revert to the period of the "consistentior status," when the earth had just solidified, we shall find, according to Lord Kelvin, that the temperature continuously increased from the surface, where it was 1170° C., down to a depth of twenty-five miles, where it was about 1430° C., or 260° C. above the fusion point of the matter, forming the crust.

[1] It would be difficult to discuss with sufficient brevity the probable distribution of these inequalities, but it may be pointed out that the moon is possibly responsible, and that in more ways than one, for much of the existing geographical asymmetry.

That the crust at this depth was not molten but solid is to be explained by the very great pressure to which it was subjected—just so much pressure, indeed, as was required to counteract the influence of the additional 260° C. Thus if we could have reduced the pressure on the crust we should have caused it to liquefy; by restoring the pressure it would resolidify. By the time the earth's surface had cooled down to 370° C. the depth beneath the surface at which the pressure just kept the crust solid would have sunk some slight distance inwards, but not sufficiently to affect our argument.

The average pressure of the primitive atmosphere upon the crust can readily be calculated by supposing the water of the existing oceans to be uniformly distributed over the earth's surface, and then by a simple piece of arithmetic determining its depth; this is found to be 1·718 miles, the average depth of the oceans being taken at 2·393 miles. Thus the average pressure over the earth's surface, immediately before the formation of the oceans, was equivalent to that of a column of water 1·718 miles high on each square inch. Supposing that at its origin the ocean were all "gathered together into one place," and "the dry land appeared," then the pressure over the ocean floor would be increased from 1·718 miles to 2·393 miles, while that over those portions of the crust that now formed the land would be diminished by 1·718 miles. This difference in pressure would tend to exaggerate those faint depressions which had arisen under the primitive anti-cyclonic areas, and if the just solidified material of the earth's crust were set into a state of flow, it might move from under the ocean into the bulgings which were rising to form the land, until static equilibrium were established. Under these circumstances the pressure of the ocean would be just able to maintain a column of rock 0·886 miles in height, or ten twenty-sevenths of its own depth. It could do no more; but in order that the dry land may appear some cause must be found competent either to lower the ocean bed the remaining seventeen twenty-sevenths of its full depth, or to raise the continental bulgings to the same extent. Such a cause may, I think, be discovered in a further effect of the reduction in pressure over the continental areas. Previous to the condensation of the ocean, these, as we have seen, were subjected to an atmospheric pressure equal to that of a column of water 1·718 miles in height. This pressure was contributory to that which caused the outer twenty-five miles of the earth's crust to become solid; it furnished, indeed, just about one fortieth of that pressure, or enough to raise the fusion point 6° C. What, then, might be expected to happen when the continental area was relieved of this load? Plainly a liquefaction and corresponding expansion of the underlying rock.

But we will not go so far as to assert that actual liquefaction would result; all we require for our explanation is a great expansion; and this would probably follow whether the crust were liquefied or not. For there is good reason to suppose that when matter at a temperature above its ordinary fusion point is compelled into the solid state by pressure, its volume is very responsive to changes either of pressure or temperature. The remarkable expansion of liquid carbon dioxide is a case in point: 120 volumes of this fluid at —20° C. become 150 volumes at 33° C.; a temperature just below the critical point. A great change of volume also occurs when the material of igneous rocks passes from the crystalline state to that of glass; in the case of diabase[1] the difference in volume of the rock in the two states at ordinary temperatures is 13 per cent. If the relief of pressure over the site of continents were accompanied by volume changes at all approaching this, the additional elevation of seventeen twenty-sevenths required to raise the land to the sea-level would be accounted for.[2] How far down beneath the surface the unloading of the continents would be felt it is difficult to say, though the problem is probably not beyond the reach of mathematical analysis; if it affected an outer envelope twenty-five miles in thickness, a linear expansion of 4 per cent. would suffice to explain the origin of ocean basins. If now we refer to the dilatation determined by Carl Barus for rise in temperature in the case of diabase, we find that between 1093° and 1112° C. the increase in volume is 3·3 per cent. As a further factor in deepening the ocean basins may be included the compressive effect of the increase in load over the ocean floor: this increase is equal to the pressure of a column of water 0·675 mile in height, and its effect in raising the fusion point would be 2° C., from which we may gain some kind of idea of the amount of compression it might produce on the yielding interior of the crust. To admit that these views are speculative will be to confess nothing; but they certainly account for a good deal. They not only give us ocean basins, but basins of the kind we want, that is, to use a crude comparison once made by the late Dr. Carpenter, basins of a tea-tray form, having a somewha flat floor and steeply sloping sides; they also help to explain how it is that the value of gravity is greater over the ocean than over the land.

The ocean when first formed would consist of highly heated water, and this, as is well known, is an energetic chemical reagent when brought into contact with silicates like those which formed the primitive crust. As a result of its action saline solutions and chemical deposits would be formed; the latter, however, would probably be of no great thickness, for the time occupied by the ocean in cooling to a temperature not far removed from the present would probably be included within a few hundreds of years.

The Stratified Series.

The course of events now becomes somewhat obscure, but sooner or later the familiar processes of denudation and the deposition started into activity, and have continued acting uninterruptedly ever since. The total maximum thickness of the sedimentary deposits, so far as I can discover, appears to amount to no less than 50 miles, made up as follows:—

	Feet	
Recent and Pleistocene ...	4,000 ...	Man.
Pliocene	5,000 ...	Pithecanthropus.
Miocene	9,000 ...	
Oligocene	12,000 ...	
Eocene	12,000	Eutheria.
Cretaceous	14,000 ...	
Jurassic	8,000 ...	
Trias	13,000 ...	Mammals.
Permian	12,000 ...	Reptiles.
Carboniferous	24,000 ...	Amphibia.
Devonian	22,000 ...	Fish.
Silurian	15,000 ...	
Ordovician	17,000 ...	
Cambrian	16,000 ...	Invertebrata.
Keeweenawan	50,000 ...	
Penokee	14,000 ...	
Huronian	18,000 ...	

Geologists, impressed with the tardy pace at which sediments appear to be accumulating at the present day, could not contemplate this colossal pile of strata without feeling that it spoke of an almost inconceivably long lapse of time. They were led to compare its duration with the distances which intervene between the heavenly bodies; but while some chose the distance of the nearest fixed star as their unit, others were content to measure the years in terms of miles from the sun.

Evolution of Organisms.

The stratified rocks were eloquent of time, and not to the geologist alone; they appealed with equal force to the biologist. Accepting Darwin's explanation of the origin of species, the

[1] C. Barus so names the material on which he experimented; apparently the rock is a fresh dolerite without olivine.

[2] Prof. Fitzgerald has been kind enough to express part of the preceding explanation in a more precise manner for me. He writes: "It would require a very nice adjustment of temperatures and pressures to work out in the simple way you state it; but what is really involved is that in a certain state diabase (and everything that changes state with a considerable change of volume) has an enormous isothermal compressibility. Although this is very enormous in the case of bodies which melt suddenly, like ice, it would also involve very great compressibilities in the case of bodies even which melted gradually, if they did so at all quickly, *i.e.* within a small range of temperature. What you postulate, then, is that at a certain depth diabase is soft enough to be squeezed from under the oceans, and that, being near its melting point, the small relief of pressure is accompanied by an enormous increase in volume which helped to raise the continents. Now that I have written the thing out in my own way it seems very likely. It is, anyway, a suggestion quite worthy of serious consideration, and a process that in some places must almost certainly have been in operation, and maybe is still operative. Looking at it again, I hardly think it is quite likely that there is or could be much squeezing sideways of liquid or other viscous material from under one place to another, because the elastic yielding of the inside of the earth would be much quicker than any flow of this kind. This would only modify your theory, because the diabase that expands so much on the relief of pressure might be that already under the land, and raising up this latter, partly by being pushed up itself by the elastic relief of the inside of the earth and partly by its own enormous expansibility near its melting point. The action would be quite slow, because it would cool itself so much by its expansion that it would have to be warmed up from below, or by tidal earth-squeezing, or by chemical action, before it could expand isothermally."

present rate at which form flows to form seemed so slow as almost to amount to immutability. How vast then must have been the period during which by slow degrees and innumerable stages the protozoon was transformed into the man! And if we turn to the stratified column, what do we find? Man, it is true, at the summit, the oldest fossiliferous rocks 34 miles lower down, and the fossils they contain already representing most of the great classes of the Invertebrata, including Crustacea and Worms. Thus the evolution of the Vertebrata alone is known to have occupied a period represented by a thickness of 34 miles of sediment. How much greater, then, must have been the interval required for the elaboration of the whole organic world! The human mind, dwelling on such considerations as these, seems at times to have been affected by a sur-excitation of the imagination, and a consequent paralysis of the understanding, which led to a refusal to measure geological time by years at all, or to reckon by anything less than "eternities."

Geologic Periods of Time.

After the admirable Address of your President last year it might be thought needless for me to again enter into a consideration of this subject; it has been said, however, that the question of geological time is like the Djin in Arabian tales, and will irrepressibly come up again for discussion, however often it is disposed of. For my part I do not regard the question so despondingly, but rather hope that by persevering effort we may succeed in discovering the talisman by which we may compel the unwilling Djin into our service. How immeasurable would be the advance of our science could we but bring the chief events which it records into some relation with a standard of time!

Before proceeding to the discussion of estimates of time drawn from a study of stratified rocks let us first consider those which have been already suggested by other data. These are as follows:—(1) Time which has elapsed since the separation of the earth and moon, fifty-six millions of years, minimum estimate by Prof. G. H. Darwin. (2) Since the "consistentior status," twenty to forty millions (Lord Kelvin). (3) Since the condensation of the oceans, eighty to ninety millions, maximum estimate by Prof. J. Joly.

It may be at once observed that these estimates, although independent, are all of the same order of magnitude, and so far confirmatory of each other. Nor are they opposed to conclusions drawn from a study of stratified rocks; thus Sir Archibald Geikie, in his Address to this Section last year, affirmed that, so far as these were concerned, one hundred millions of years might suffice for their formation. There is then very little to quarrel about, and our task is reduced to an attempt, by a little stretching and a little paring, to bring these various estimates into closer harmony.

Prof. Darwin's estimate is admittedly a minimum; the actual time, as he himself expressly states, "may have been much longer." Lord Kelvin's estimate, which he would make nearer twenty than forty millions, is founded on the assumption that since the period of the "consistentior status" the earth has cooled simply as a solid body, the transference of heat from within outwards having been accomplished solely by conduction.[1]

It may be at once admitted that there is a large amount of truth in this assumption; there can be no possible doubt that the earth reacts towards forces applied for a short time as a solid body. Under the influence of the tides it behaves as though it possessed a rigidity approaching that of steel, and under sudden blows, such as those which give rise to earthquakes, with twice this rigidity, as Prof. Milne informs me. Astronomical considerations lead to the conclusion that its effective rigidity has not varied greatly for a long period of past time.

Still, while fully recognising these facts, the geologist knows—we all know—that the crust of the earth is not altogether solid. The existence of volcanoes by itself suggests the contrary, and although the total amount of fluid material which is brought from the interior to the exterior of the earth by volcanic action may be, and certainly is, small—from data given by Prof. Penck, I estimate it as equivalent to a layer of rock uniformly distributed 2 mm. thick per century; yet we have every reason to believe that volcanoes are but the superficial manifestation of far greater bodies of molten material which lie concealed beneath the ground. Even the wide areas of plutonic rock, which are sometimes exposed to view over a country that has suffered long-continued denudation, are merely the upper portion of more extensive masses which lie remote from view. The existence of molten material within the earth's crust naturally awakens a suspicion that the process of cooling has not been wholly by conduction, but also to some slight extent by convection, and to a still greater extent by the bodily migration of liquid lava from the deeper layers of the crust towards the surface.

The existence of local reservoirs of molten rock within the crust is even still more important in another connection, that is, in relation with the supposed "average rate of increase of temperature with descent below the ground." It is doubtful whether we have yet discovered a rate that in any useful sense can be spoken of as "average." The widely divergent views of different authorities as to the presumed value of this rate may well lead to reflection. The late Prof. Prestwich thought a rise of 1° F. for every 45 feet of descent below the zone of constant temperature best represented the average; Lord Kelvin in his earlier estimates has adopted a value of 1° F. for every 51 feet; the Committee of this Association appointed to investigate this question arrived at a rate of 1° F. for every 60 feet of descent; Mr. Clarence King has made calculations in which a rate of 1° F. for 72 feet is adopted; a re-investigation of recorded measurements would, I believe, lead to a rate of 1° F. in 80 or 90 feet as more closely approaching the mean. This would raise Lord Kelvin's estimate to nearly fifty millions of years.

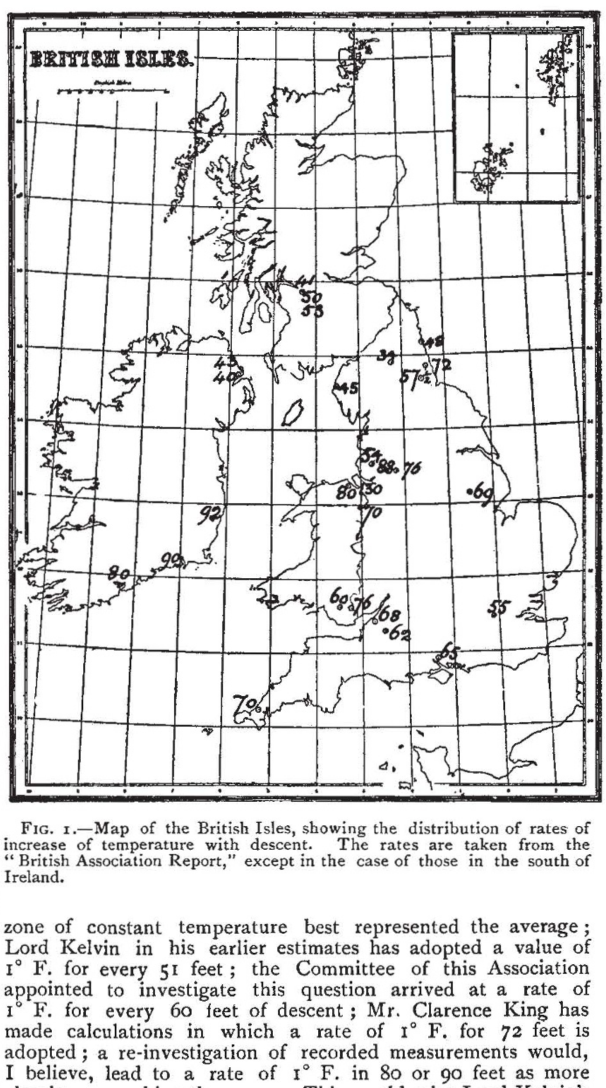

FIG. 1.—Map of the British Isles, showing the distribution of rates of increase of temperature with descent. The rates are taken from the "British Association Report," except in the case of those in the south of Ireland.

When from these various averages we turn to the observations on which they are based, we encounter a surprising divergence of extremes from the mean; thus in the British Isles alone the rate varies from 1° F. in 34 feet to 1° F. in 92 feet, or in one case to 1° F. in 130 feet. It has been suggested, and to some extent shown, that these irregularities may be connected with

[1] The heat thus brought to the surface would amount to one-seventeenth of that conveyed by conduction.

differences in conductivity of the rocks in which the observations were made, or to the circulation of underground water; but many cases exist which cannot be explained away in such a manner, but are suggestive of some deep-seated cause, such as the distribution of molten matter below the ground. Inspection of the accompanying map of the British Isles, on which the rates of increase in different localities have been plotted, will afford some evidence of the truth of this view. Comparatively low rates of increase are found over Wales and in the province of Leinster, districts of relatively great stability, the remnants of an island that have in all probability stood above the sea ever since the close of the Silurian period. To the north of this, as we enter a region which was subject to volcanic disturbances during the Tertiary period, the rate increases.

It is obvious that in any attempt to estimate the rate at which the earth is cooling as a solid body the disturbing influence of subterranean lakes of molten rock must as far as possible be eliminated; but this will not be effected by taking the accepted mean of observed rates of increase of temperature: such an average is merely a compromise, and a nearer approach to a correct result will possibly be attained by selecting some low rate of increase, provided it be based on accurate observations.

It is extremely doubtful whether an area such as the British Isles, which has so frequently been the theatre of volcanic activity and other subterranean disturbance, is the best fitted to afford trustworthy results; the Archæan nucleus of a continent might be expected to afford surer indications. Unfortunately the hidden treasures of the earth are seldom buried in these regions, and bore-holes in consequence have rarely been made in them. One exception is afforded by the copper-bearing district of Lake Superior, and in one case, that of the Calumet and Hecla mine, which is 4580 feet in depth, the rate of increase, as determined by Prof. A. Agassiz, was 1° F. for every 223·7 feet. The Bohemian "horst" is a somewhat ancient part of Europe, and in the Przibram mines, which are sunk in it, the rate was 1° F. for every 126 feet of descent. In the light of these facts it would seem that geologists are by no means compelled to accept the supposed mean rate of increase of temperature with descent into the crust as affording a safe guide to the rate of cooling of a solid globe; and if the much slower rate of increase observed in the more ancient and more stable regions of the earth has the importance which is suggested for it, then Lord Kelvin's estimate of the date of the "consistentior status" may be pushed backwards into a remoter past.

If, as we have reason to hope, Lord Kelvin's somewhat contracted period will yield to a little stretching, Prof. Joly's, on the other hand, may take some paring. His argument, broadly stated, is as follows. The ocean consisted at first of fresh water; it is now salt, and its saltness is due to the dissolved matter that is constantly being carried into it by rivers. If, then, we know the quantity of salt which the rivers bring down each year into the sea, it is easy to calculate how many years they have taken to supply the sea with all the salt it at present contains. For several reasons it is found necessary to restrict attention to one only of the elements contained in sea salt: this is sodium. The quantity of sodium delivered to the sea every year by rivers is about 160,000,000 tons; but the quantity of sodium which the sea contains is at least ninety millions of times greater than this. The period during which rivers have been carrying sodium into the sea must therefore be about ninety millions of years. Nothing could be simpler; there is no serious flaw in the method, and Prof. Joly's treatment of the subject is admirable in every way; but of course in calculations such as this everything depends on the accuracy of the data, which we may therefore proceed to discuss. Prof. Joly's estimate of the amount of sodium in the ocean may be accepted as sufficiently near the truth for all practical purposes. We may therefore pass on to the other factor, the annual contribution of sodium by river water. Here there is more room for error. Two quantities must be ascertained: one the quantity of water which the rivers of the world carry into the sea, the other the quantity or proportion of sodium present in this water. The total volume of water discharged by rivers into the ocean is estimated by Sir John Murray as 6524 cubic miles. The estimate being based on observations of thirty-three great rivers, although only approximate, it is no doubt sufficiently exact; at all events such alterations as it is likely to undergo will not greatly affect the final result. When, however, we pass to the last quantity to be determined, the chemical composition of average river water, we find that only a very rough estimate is possible, and this is the more unfortunate because changes in this may very materially affect our conclusions. The total quantity of river water discharged into the sea is, as we have stated, 6524 cubic miles. The average composition of this water is deduced from analyses of nineteen great rivers, which altogether discharge only 488 cubic miles, or 7·25 per cent. of the whole. The danger in using this estimate is twofold: in the first place 7·25 is too small a fraction from which to argue to the remaining 92·75 per cent., and, next, the rivers which furnish it are selected rivers, *i.e.*, they are all of large size. The effect of this is that the drainage of the volcanic regions of the earth is not sufficiently represented, and it is precisely this drainage which is richest in sodium salts. The lavas and ashes of active volcanoes rapidly disintegrate under the energetic action of various acid gases, and among volcanic exhalations sodium chloride has been especially noticed as abundant. Consequently we find that while the proportion of sodium in Prof. Joly's average river water is only 5·73 per million, in the rivers of the volcanic island of Hawaii it rises to 24·5 per million (Walter Maxwell, "Lavas and Soils of the Hawaiian Islands," p. 170). No doubt the area occupied by volcanoes is trifling compared with the remaining land surface. On the other hand the majority of volcanoes are situated in regions of copious rainfall, of which they receive a full share owing to their mountainous form. Much of the fallen rain percolates through the porous material of the cone, and, richly charged with alkalies, finds its way by underground passages towards the sea, into which it sometimes discharges by submarine springs.

Again, several considerations lead to the belief that the supply of sodium to the ocean has proceeded, not at a uniform, but at a gradually diminishing rate. The rate of increase of temperature with descent into the crust has continuously diminished with the flow of time, and this must have had its influence on the temperature of springs, which furnish an important contribution to river water. The significance of this consideration may be judged from the composition of the water of geysers. Thus Geyser, in Iceland, contains 884 parts of sodium per million, or nearly 160 times as much as Sir John Murray estimates is present in average river water. A mean of the analyses of six geysers in different parts of the world gives 400 parts of sodium per million, existing partly as chloride, but also as sulphate and carbonate.

It should not be overlooked that the present is a calm and quiet epoch in the earth's history, following after a time of fiery activity. More than once, indeed, has the past been distinguished by unusual manifestations of volcanic energy, and these must have had some effect upon the supply of sodium to the ocean. Finally, although the existing ocean water has apparently but slight effect in corroding the rocks which form its bed, yet it certainly was not inert when its temperature was not far removed from the critical point. Water begins to exert a powerful destructive action on silicates at a temperature of 180° C., and during the interval occupied in cooling from 370 to 180° C. a considerable quantity of sodium may have entered into solution.

A review of the facts before us seems to render some reduction in Dr. Joly's estimate imperative. A precise assessment is impossible, but I should be inclined myself to take off some ten or thirty millions of years.

We may next take the evidence of the stratified rocks. Their total maximum thickness is, as we have seen, 265,000 feet, and consequently if they accumulated at the rate of one foot in a century, as evidence seems to suggest, more than twenty-six millions of years must have elapsed during their formation.

Obscure Chapter in the Earth's History.

Before discussing the validity of the argument on which this last result depends, let us consider how far it harmonises with previous ones. It is consistent with Lord Kelvin's and Professor Darwin's, but how does it accord with Professor Joly's? Supposing we reduce his estimate to fifty-five millions; what was the earth doing during the interval between the period of fifty-five millions of years ago and that of only 26½ millions of years ago, when, it is presumed, sedimentary rocks commenced to be formed? Hitherto we have been able to reason on probabilities; now we enter the dreary region of possibilities, and open that obscure chapter in the history of the earth previously hinted at. For there are many possible answers to this question. In the first place the evidence of the stratified rocks may have been wrongly interpreted, and two or three times the amount of time we have demanded may have been consumed in their formation. This is a very obvious possibility, yet again our estimate concerning these rocks may be correct, but we may have erroneously omitted to take into account certain portions of the Archæan complex, which may represent primitive sedimentary rocks, formed under exceptional conditions, and subsequently transformed under the influence of the internal heat of the earth. This, I think, would be Prof. Bonney's view. Finally Lord Kelvin has argued that the life of the sun as a luminous star is even more briefly limited than that of our oceans. In such a case if our oceans were formed fifty-five millions of years ago, it is possible that after a short existence as almost boiling water they grew colder and colder, till they became covered with thick ice, and moved only in obedience to the tides. The earth, frozen and dark, except for the red glow of her volcanoes, waited the coming of the sun, and it was not till his growing splendour had banished the long night that the cheerful sound of running waters was heard again in our midst. Then the work of denudation and deposition seriously recommenced, not to cease till the life of the sun is spent. Thus the thickness of the stratified series may be a measure rather of the duration of sunlight than of the period which has elapsed since the first formation of the ocean. It may have been so—we cannot tell—but it may be fairly urged that we know less of the origin, history, and constitution of the sun than of the earth itself, and that, for aught we can say to the contrary, the sun may have been shining on the just-formed ocean as cheerfully as he shines to-day.

Time required for the Evolution of the Living World.

But, it will be asked, how far does a period of twenty-six millions satisfy the demands of biology? Speaking only for myself, although I am aware that eminent biologists are not wanting who share this opinion, I answer, Amply. But it will be exclaimed, Surely there are "comparisons in things." Look at Egypt, where more than 4,000 years since the same species of man and animals lived and flourished as to-day. Examine the frescoes and study the living procession of familiar forms they so faithfully portray, and then tell us, how comes it about that from changes so slow as to be inappreciable in the lapse of forty centuries you propose to build up the whole organic world in the course of a mere twenty-six millions of years? To all which we might reply that even changeless Egypt presents us with at least one change—the features of the ruling race are to-day not quite the same as those of the Pharaohs. But putting this on one side, the admitted constancy in some few common forms proves very little, for so long as the environment remains the same natural selection will conserve the type, and, so far as we are able to judge, conditions in Egypt have remained remarkably constant for a long period.

Change the conditions, and the resulting modification of the species becomes manifest enough; and in this connection it is only necessary to recall the remarkable mutations observed and recorded by Prof. Weldon in the case of the crabs in Plymouth Harbour. In response to increasing turbidity of the sea water these crabs have undergone or are undergoing a change in the relative dimensions of the carapace, which is persistent, in one direction, and rapid enough to be determined by measurements made at intervals of a few years.

Again, animals do not all change their characters at the same rate: some are stable, in spite of changing conditions, and these have been cited to prove that none of the periods we look upon as probable, not twenty-five, not a hundred millions of years, scarce any period short of eternity, is sufficient to account for the evolution of the living world. If the little tongue-shell, *Lingula*, has endured with next to no perceptible change from the Cambrian down to the present day, how long, it is sometimes inquired, would it require for the evolution of the rest of the animal kingdom? The reply is simple: the cases are dissimilar, and the same record which assures us of the persistency of the *Lingula* tells us in language equally emphatic of the course of evolution which has led from the lower organisms upwards to man. In recent and Pleistocene deposits the relics of man are plentiful: in the latest Pliocene they have disappeared, and we encounter the remarkable form *Pithecanthropus*; as we descend into the Tertiary systems the higher mammals are met with, always sinking lower and lower in the scale of organisation as they occur deeper in the series, till in the Mesozoic deposits they have entirely disappeared, and their place is taken by the lower mammals, a feeble folk, offering little promise of the future they were to inherit. Still lower, and even these are gone; and in the Permian we encounter reptiles and the ancestors of reptiles, probably ancestors of mammals too; then into the Carboniferous, where we find amphibians, but no true reptiles; and next into the Devonian, where fish predominate, after making their earliest appearance at the close of the Silurian times; thence downwards, and the vertebrata are no more found—we trace the evolution of the invertebrata alone. Thus the orderly procession of organic forms follows in precisely the true phylogenetic sequence; invertebrata first, then vertebrates, at first fish, then amphibia, next reptiles, soon after mammals, of the lowlier kinds first, of the higher later, and these in increasing complexity of structure till we finally arrive at man himself. While the living world was thus unfolding into new and nobler forms, the immutable *Lingula* simply perpetuated its kind. To select it, or other species equally sluggish, as the sole measure of the rate of biologic change would seem as strange a proceeding as to confound the swiftness of a river with the stagnation of the pools that lie beside its banks. It is occasionally objected that the story we have drawn from the palæontological record is mere myth or is founded only on negative evidence. Cavils of this kind prove a double misapprehension, partly as to the facts, partly as to the value of negative evidence, which may be as good in its way as any other kind of evidence.

Geologists are not unaware of the pitfalls which beset negative evidence, and they do not conclude from the absence of fossils in the rocks which underlie the Cambrian that pre-Cambrian periods were devoid of life; on the contrary, they are fully persuaded that the seas of those times were teeming with a rich variety of invertebrate forms. How is it that, with the exception of some few species found in beds immediately underlying the Cambrian, these have left behind no vestige of their existence? The explanation does not lie in the nature of the sediments, which are not unfitted for the preservation of fossils, nor in the composition of the then existing sea water, which may have contained quite as much calcium carbonate as occurs in our present oceans; and the only plausible supposition would appear to be that the organisms of that time had not passed beyond the stage now represented by the larvæ of existing invertebrata, and consequently were either unprovided with skeletons, or at all events with skeletons durable enough for preservation. If so, the history of the earlier stages of the evolution of the invertebrata will receive no light from palæontology; and no direct answer can be expected to the question whether, eighteen or nineteen millions of years being taken as sufficient for the evolution of the vertebrata, the remaining available eight millions would provide for that of the invertebrate classes which are represented in the lowest Cambrian deposits. On *a priori* grounds there would appear to be no reason why it should not. If two millions of years afforded time enough for the conversion of fish into amphibians, a similar period should suffice for the evolution of trilobites from annelids, or of annelids from trochospheres. The step from gastrulas to trochospheres might be accomplished in another two millions, and two millions more would take us from gastrulas through morulas to protozoa.

As things stand, biologists can have nothing to say either for or against such a conclusion: they are not at present in a position to offer independent evidence; nor can they hope to be so until they have vastly extended those promising investigations which they are only now beginning to make into the rate of the variation of species.

Unexpected Absence of Thermal Metamorphosis in Ancient Rocks.

Two difficulties now remain for discussion: one based on theories of mountain chains, the other on the unaltered state of some ancient sediments. The latter may be taken first. Prof. van Hise writes as follows regarding the pre-Cambrian rocks of the Lake Superior district: "The Penokee series furnishes an instructive lesson as to the depth to which rocks may be buried and yet remain but slightly affected by metamorphosis. The series itself is 14,000 feet thick. It was covered before being upturned with a great thickness of Keweenaw rock. This series at the Montreal River is estimated to be 50,000 feet thick. Adding to this the known thickness of the Penokee series, we have a thickness of 64,000 feet. . . . The Penokee rocks were then buried to a great depth, the exact amount depending upon their horizon and upon the stage in Keweenaw time, when the tilting and erosion, which brought them to the surface, commenced.

"That the synclinal trough of Lake Superior began to form before the end of the Keweenaw period, and consequently that the Penokee rocks were not buried under the full succession, is more than probable. However, they must have been buried to a great depth—at least several miles—and thus subjected to high pressure and temperature, notwithstanding which they are comparatively unaltered" (*Tenth Annual Report U.S. Geol. Survey*, 1888–89, p. 457).

I select this example because it is one of the best instances of a difficulty that occurs more than once in considering the history of sedimentary rocks. On the supposition that the rate of increment of temperature with descent is 1° F. for every 84 feet, or 1° C. for every 150 feet, and that it was no greater during these early Penokee times, then at a depth of 50,000 feet the Penokee rocks would attain a temperature of nearly 333° C.; and since water begins to exert powerful chemical action at 180° C. they should, on the theory of a solid cooling globe, have suffered a metamorphosis sufficient to obscure their resemblance to sedimentary rocks. Either then the accepted rate of downward increase of temperature is erroneous, or the Penokee rocks were never depressed, in the place where they are exposed to observation, to a depth of 50,000 feet. Let us consider each alternative, and in the first place let us apply the rate of temperature increment determined by Prof. Agassiz in this very Lake Superior district: it is 1° C. for every 402 feet, and twenty-five millions of years ago, or about the time when we may suppose the Penokee rocks were being formed, it would be 1° C. for every 305·5 feet, with a resulting temperature at a depth of 50,000 feet of 163° C. only. Thus the admission of a very low rate of temperature increment would meet the difficulty; but on the other hand it would involve a period of several hundreds of millions of years for the age of the "consistentior status," and thus greatly exceed Prof. Joly's maximum estimate of the age of the oceans. We may therefore turn to the second alternative. As regards this it is by no means certain that the exposed portion of the Penokee series ever was depressed 50,000 feet: the beds lie in a synclinal the base of which indeed may have sunk to this extent, and entered a region of metamorphosis; but the only part of the system that lies exposed to view is the upturned margin of the synclinal, and as to this it would seem impossible to make any positive assertion as to the depth to which it may or may not have been depressed. To keep an open mind on the question seems our only course for the present, but difficulties like this offer a promising field for investigation.

The Formation of Mountain Ranges.

It is frequently alleged that mountain chains cannot be explained on the hypothesis of a solid earth cooling under the conditions and for the period we have supposed. This is a question well worthy of consideration, and we may first endeavour to picture to ourselves the conditions under which mountain chains arise. The floor of the ocean lies at an average depth of 2000 fathoms below the land, and is maintained at a constant temperature, closely approaching 0° C., by the passage over it of cold water creeping from the polar regions. The average temperature of the surface of the land is above zero, but we can afford to disregard the difference in temperature between it and the ocean floor, and may take them both at zero. Consider next the increase of temperature with descent, which occurs beneath the continents: at a depth of 13,000 feet, or at same depth as the ocean floor, a temperature of 87° C. will be reached on the supposition that the rate of increase is 1° C. for 150 feet, while with the usually accepted rate of 1° C. for 108 feet it would be 120° C. But at this depth the ocean floor, which is on the same spherical surface, is at 0° C. Thus surfaces of equal temperature within the earth's crust will not be spherical, but will rise or fall beneath an imaginary spherical or spheroidal surface according as they occur beneath the continents or the oceans. No doubt at some depth within the earth the departure of isothermal surfaces from a spheroidal form will disappear; but considering the great breadth both of continents ond oceans this depth must be considerable, possibly even forty or fifty miles. Thus the sub-continental excess of temperature may make itself felt in regions where the rocks still retain a high temperature, and are probably not far removed from the critical fusion point. The effect will be to render the continents mobile as regards the ocean floor; or, *vice-versâ*, the ocean floor will be stable compared with the continental masses. Next it may be observed that the continents pass into the bed of the ocean by a somewhat rapid flexure, and that it is over this area of flexure that the sediments denuded from the land are deposited. Under its load of sediment the sea-floor sinks down, subsiding slowly, at about the same rate as the thickness of sediment increases; and, whether as a consequence or a cause, or both, the flexure marking the boundary of land and sea becomes more pronounced. A compensating movement occurs within the earth's crust, and solid material may flow from under the subsiding area in the direction of least resistance, possibly towards the land. At length, when some thirty or forty thousand feet of sediment have accumulated in a basin-like form, or, according to our reckoning, after the lapse of three or four millions of years, the downward movement ceases, and the mass of sediment is subjected to powerful lateral compression, which, bringing its borders into closer proximity by some ten or thirty miles, causes it to rise in great folds high into the air as a mountain chain.

It is this last phase in the history of mountain making which has given geologists more cause for painful thought than probably any other branch of their subject, not excluding even the age of the earth. It was at first imagined that during the flow of time the interior of the earth lost so much heat, and suffered so much contraction in consequence, that the exterior, in adapting itself to the shrunken body, was compelled to fit it like a wrinkled garment. This theory, indeed, enjoyed a happy existence till it fell into the hands of mathematicians, when it fared very badly, and now lies in a pitiable condition neglected of its friends.[1]

For it seemed proved to demonstration that the contraction consequent on cooling was wholly, even ridiculously, inadequate to explain the wrinkling. But when we summon up courage to inquire into the data on which the mathematical arguments are based, we find that they include several assumptions, the truth of which is by no means self-evident. Thus it has been assumed that the rate at which the fusion point rises with increased pressure is constant, and follows the same law as is deduced from experiments made under such pressures as we can command in our laboratories down to the very centre of the earth, where the pressures are of an altogether different order of magnitude; so with a still more important coefficient, that of expansion, our knowledge of this quantity is founded on the behaviour of rocks heated under ordinary atmospheric pressure, and it is assumed that the same coefficient as is thus obtained may be safely applied to material which is kept solid, possibly near the critical point, under the tremendous pressure of the depths of the crust. To this last assumption we owe the terrible bogies that have been conjured out of "the level of no strain." The depth of this as calculated by the Rev. O. Fisher is so trifling that it would be passed through by all very deep mines. Mr. C. Davison, however, has shown that it will lie considerably deeper, if the known increase of the coefficient of expansion with rise of temperature be taken into account. It is possible, it is even likely, that the coefficient of expansion becomes vastly greater when regions are entered where the rocks are compelled into the solid state by pressure. So little do we actually know of the behaviour of rock under these conditions that the geologist would seem to be left very much to his own devices; but it would seem there is one temptation he must resist—he may not take refuge in the hypothesis of a liquid interior.

We shall boldly assume that the contraction at some unknown depth in the interior of the earth is sufficient to afford the explanation we seek. The course of events may then proceed as follows. The contraction of the interior of the earth, consequent on its loss of heat, causes the crust to fall upon it in folds, which rise over the continents and sink under the oceans, and the flexure of the area of sedimentation is partly a consequence of this folding, partly of overloading. By the time a depression of some 30,000 or 40,000 feet has occurred along the ocean border the relation between continents and oceans has become unstable, and readjustment takes place, probably by a giving way of the continents, and chiefly along the zone of greatest weakness, *i.e.* the area of sedimentation, which thus becomes the zone of mountain building. It may be observed that at great depths readjustment will be produced by a slow flowing of solid rock, and it is only comparatively near the surface, five or ten miles at the most below, that failure of

[1] With some exceptions, notably Mr. C. Davison, a consistent supporter of the theory of contraction.

support can lead to sudden fracture and collapse; hence the comparatively superficial origin of earthquakes.

Given a sufficiently large coefficient of expansion—and there is much to suggest its existence (*vide*, p. 483)—and all the phenomena of mountain ranges become explicable: they begin to present an appearance that invites mathematical treatment; they inspire us with the hope that from a knowledge of the height and dimensions of a continent and its relations to the bordering ocean we may be able to predict when and where a mountain chain should arise, and the theory which explains them promises to guide us to an interpretation of those world-wide unconformities which Suess can only account for by a transgression of the sea. Finally it relieves us of the difficulty presented by mountain formation in regard to the estimated duration of geological time.

Influence of Variations in the Eccentricity of the Earth's Orbit.

This may perhaps be the place to notice a highly interesting speculation which we owe to Prof. Blytt, who has attempted to establish a connection between periods of readjustment of the earth's crust and variations in the eccentricity of the earth's orbit. Without entering into any discussion of Prof. Blytt's methods, we may offer a comparison of his results with those that follow from our rough estimate of one foot of sediment accumulated in a century.

Table showing the Time that has elapsed since the Beginning of the Systems in the first column, as reckoned from Thickness of Sediment in the second column, and by Prof. Blytt in the third:—

	Years	Years
Eocene ...	4,200,000 ...	3,250,000
Oligocene ...	3,000,000 ...	1,810,000
Miocene ...	1,800,000 ...	1,160,000
Pliocene ...	900,000 ...	700,000
Pleistocene ...	400,000 ...	350,000

It is now time to return to the task, too long postponed, of discussing the data from which we have been led to conclude that a probable rate at which sediments have accumulated in places where they attain their maximum thickness is one foot per century.

Rate of Deposition of Sediment.

We owe to Sir Archibald Geikie a most instructive method of estimating the existing rate at which our continents and islands are being washed into the sea by the action of rain and rivers: by this we find that the present land surface is being reduced in height to the extent on an average of 1/2400 foot yearly (according to Prof. Penck 1/3600 foot). If the material removed from the land were uniformly distributed over an area equal to that from which it had been derived, it would form a layer of rock 1/2400 foot thick yearly, *i.e.* the rates of denudation and deposition would be identical. But the two areas, that of denudation and that of deposition, are seldom or never equal, the latter as a rule being much the smaller. Thus the area of that part of North America which drains into the Gulf of Mexico measures 1,800,000 square miles; the area over which its sediments are deposited is, so far as I can gather from Prof. Agassiz' statements, less than 180,000 square miles; while Mr. McGee estimates it at only 100,000 square miles. Using the largest number, the area of deposition is found to measure one-tenth the area of denudation; the average rate of deposition will therefore be ten times as great as the rate of denudation, or $\frac{1}{240}$ foot may be supposed to be uniformly distributed over the area of sedimentation in the course of a year. But the thickness by which we have measured the strata of our geological systems is not an average but a maximum thickness; we have therefore to obtain an estimate of the maximum rate of deposition. If we assume the deposited sediments to be arranged somewhat after the fashion of a wedge with the thin end seawards, then twice the average would give us the maximum rate of deposition; this would be one foot in 120 years. But the sheets of deposited sediment are not merely thicker towards the land, thinner towards the sea, they also increase in thickness towards the rivers in which they have their source, so that a very obtuse-angled cone, or, better, the down-turned bowl of a spoon, would more nearly represent their form. This form tends to disappear under the action of waves and currents, but a limit is set to this disturbing influence by the subsidence which marks the region opposite the mouth of a large river. By this the strata are gradually let downwards, so that they come to assume the form of the bowl of a spoon turned upwards. Thus a further correction is necessary if we are to arrive at a fair estimate of the maximum rate of deposition. Considering the very rapid rate at which our ancient systems diminish in thickness when traced in all directions from the localities where they attain their maximum, it would appear that this correction must be a large one. If we reduce our already corrected estimate by one-fifth, we arrive at a rate of one foot of sediment deposited in a century.

No doubt this value is often exceeded; thus in the case of the Mississippi River the bar of the south-west pass advanced between the years 1838 and 1874 a distance of over 2 miles, covering an area 2·2 miles in width with a deposit of sediment 80 feet in thickness; outside the bar, where the sea is 250 feet in depth, sediment accumulates, according to Messrs. Humphreys and Abbot, at a rate of 2 feet yearly. It is quite possible, indeed it is very likely, that some of our ancient strata have been formed with corresponding rapidity. No gravel or coarse sand is deposited over the Mississippi delta; such material is not carried further seawards than New Orleans. Thus the vast sheets of conglomerate and sandstone which contribute so largely to some of our ancient systems, such as the Cambrian, Old Red Sandstone, Millstone Grit, and Coal Measures, must have accumulated under very different conditions, conditions for which it is not easy to find a parallel; but in any case these deposits afford evidence of very rapid accumulation.

These considerations will not tempt us, however, to modify our estimate of one foot in a century; for though in some cases this rate may have been exceeded, in others it may not have been nearly attained.

Closely connected with the rate of deposition is that of the changing level of land and sea; in some cases, as in the Wealden delta, subsidence and deposition appear to have proceeded with equal steps, so that we might regard them as transposable terms. It would therefore prove of great assistance if we could determine the average rate at which movements of the ground are proceeding; it might naturally be expected that the accurate records kept by tidal gauges in various parts of the world would afford us some information on this subject; and no doubt they would, were it not for the singular misbehaviour of the sea, which does not maintain a constant level, its fluctuations being due, according to Prof. Darwin, to the irregular melting of ice in the polar regions. Of more immediate application are the results of Herr L. Holmström's observations in Scandinavia, which prove an average rise of the peninsula at the rate of 3 feet in a century to be still in progress; and Mr. G. K. Gilbert's measurements in the great Lake district of North America, which indicate a tilting of the continent at the rate of 3 inches per hundred miles per century. But while measurements like these may furnish us with some notion of the sort of speed of these changes, they are not sufficient even to suggest an average; for this we must be content to wait till sufficient tidal observations have accumulated, and the disturbing effect of the inconstancy of the sea-level is eliminated.

It may be objected that in framing our estimate we have taken into account mechanical sediments only, and ignored others of equal importance, such as limestone and coal. With regard to limestone, its thickness in regions where systems attain their maximum may be taken as negligible; nor is the formation of limestone necessarily a slow process. The successful experiments of Dr. Allan, cited by Darwin, prove that reef-building corals may grow at the astonishing rate of six feet in height per annum.

In respect of coal there is much to suggest that its growth was rapid. The carboniferous period well deserves its name, for never before, never since, have Carbonaceous deposits accumulated to such a remarkable thickness or over such wide areas of the earth's surface. The explanation is doubtless partly to be found in favourable climatal conditions, but also, I think, in the youthful energy of a new and overmastering type of vegetation, which then for the first time acquired the dominion of the land. If we turn to our modern peat-bogs, the only Carbonaceous growths available for comparison, we find from data given by Sir A. Geikie that a fairly average rate of increase is six feet in a century, which might perhaps correspond to one foot of coal in the same period.

The rate of deposition has been taken as uniform through the whole period of time recorded by stratified rocks; but lest it should be supposed that this involves a tacit admission of uniformity, I hasten to explain that in this matter we have no choice; we may feel convinced that the rate has varied from time to time, but in what direction, or to what extent, it is impossible to conjecture. That the sun was once much hotter is probable, but equally so that at an earlier period it was much colder; and even if in its youth all the activities of our planet were enhanced, this fact might not affect the maximum thickness of deposits. An increase in the radiation of the sun, while it would stimulate all the powers of subaërial denudation, would also produce stronger winds and marine currents; stronger currents would also result from the greater magnitude and frequency of the tides, and thus while larger quantities of sediment might be delivered into the sea they would be distributed over wider areas, and the difference between the maximum and average thickness of deposits would consequently be diminished. Indications of such a wider distribution may perhaps be recognised in the Palæozoic systems. Thus we are compelled to treat our rate of deposition as uniform, notwithstanding the serious error this may involve.

The reasonableness of our estimate will perhaps best appear from a few applications. Fig. 2 is a chart, based on a map by De Lapparent, representing the distribution of land and sea over the European area during the Cambrian period. The strata of this system attain their maximum thickness of 12,000 feet in Merionethshire, Wales; they rapidly thin out northwards, and are absent in Anglesey; scarcely less rapidly towards Shropshire, where they are 3000 feet thick; still a little less rapidly towards the Malverns, where they are only 800 feet thick; and most slowly towards St. David's Head, where they are 7400 feet thick. The Cambrian rocks of Wales were in all probability the deposits of a river system which drained some vanished land once situated to the west. How great was the extent of this land none can say; some geologists imagine it to have obliterated the whole or greater part of the North Atlantic Ocean. For my part I am content with a somewhat large island. What area of this island, we may ask, would suffice to supply the Cambrian sediments of Wales and Shropshire? Admitting that the area of denudation was ten times as large as the area of deposition, its dimensions are indicated by the figure *a b c d* on the chart. This evidently leaves room enough on the island to furnish all the other deposits which are distributed along the western shores of the Cambrian Sea, while those on the east are amply provided for by that portion of the European continent which then stood above water.

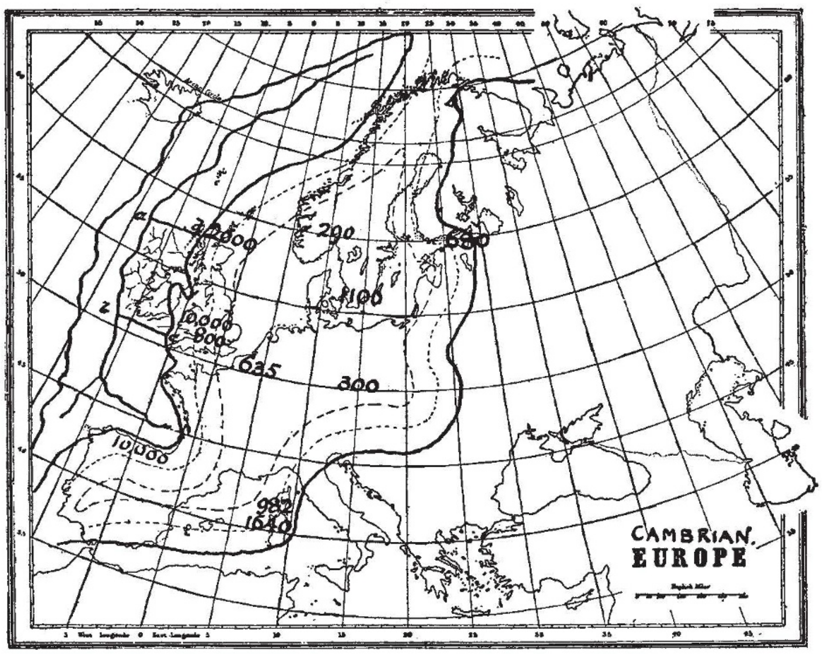

FIG. 2.—Chart of the distribution of land and sea, and of the thickness of deposits of the Cambrian system. The dotted lines indicate distances of 100 and 200 miles from the shore

If one foot in a century be a quantity so small as to disappoint the imagination of its accustomed exercise, let us turn to the Cambrian succession of Scandinavia, where all the zones recognised in the British series are represented by a column of sediment 290 feet in thickness. If 1,600,000 years be a correct estimate of the duration of Cambrian time, then each foot of the Scandinavian strata must have occupied 5513 years in its formation. Are these figures sufficiently inconceivable?

In the succeeding system, that of the Ordovician, the maximum thickness is 17,000 feet. Its deposits are distributed over a wider area than the Cambrian, but they also occupied longer time in their formation; hence the area from which they were derived need not necessarily have been larger than that of the preceding period.

Great changes in the geography of our area ushered in the Silurian system: its maximum thickness is found over the Lake district, and amounts to 15,000 feet; but in the little island of Gothland, where all the subdivisions of the system, from the Landovery to the Upper Ludlow, occur in complete sequence, the thickness is only 208 feet. In Gothland, therefore, according to our computation, the rate of accumulation was one foot in 7211 years.

With this example we must conclude, merely adding that the same story is told by other systems and other countries, and that, so far as my investigations have extended, I can find no evidence which would suggest an extension of the estimate I have proposed. It is but an estimate, and those who have made acquaintance with "estimates" in the practical affairs of life know how far this kind of computation may guide us to or from the truth.

This Address is already unduly long, and yet not long enough for the magnitude of the subject of which it treats. As we glance backwards over the past we see catastrophism yield to uniformitarianism, and this to evolution, but each as it disappears leaves behind some precious residue of truth. For the future of our science our ambition is that which inspired the closing words of your last President's Address, that it may become more experimental and exact. Our present watchword is Evolution. May our next be Measurement and Experiment, Experiment and Measurement.

NOTES.

ANOTHER of those disastrous hurricanes which occasionally visit the West Indies and United States at this season of the year has to be recorded. On the 8th inst. a storm of great violence struck the coasts of Louisiana and Texas, and, owing to the thickly populated districts over which it swept and to the high water wave which accompanied it, immense destruction to property and lamentable loss of life ensued. The fury of the storm is said to have been felt for at least a hundred miles inland, but up to the present time scarcely any details have arrived as to its character and the exact path that it followed. This part of America is one of the three regions referred to in the works of Prof. W. M. Davis from which tropical storms move into temperate latitudes in the northern hemisphere; but we must wait for further details before it can be stated whether the one in question was of the nature of a tornado, which differs from an ordinary hurricane chiefly in its excessive violence over a small, instead of a large, area. From the description so far