THURSDAY, SEPTEMBER 10, 1908.

NEW EDITION OF STRASBURGER'S BOTANY.

A Text-book of Botany. By Dr. E. Strasburger, Dr. Fritz Noll, Dr. H. Schenck, and Dr. G. Karsten. Third English edition, revised with eighth German edition by Dr. W. H. Lang. Pp. x+746. (London: Macmillan and Co., Ltd., 1908.) Price 18*s.* net.

THIS work is divided into two main divisions, "General Botany" and "Special Botany." The general botany commences with a section on morphology, external and internal. Some alterations from the second English edition have been made here, notably in the account and diagrams of mitosis. A paragraph on "reduction division," with illustrations, has been added. The description of the secondary tissues and their development is particularly good, but the new diagrams illustrating this subject do not add to the clearness.

Physiology is treated under the heads, stability of the plant body, nutrition, respiration, growth, movement, reproduction. There are numerous small changes and additions, chiefly due to research since the second English edition appeared. Several new illustrations, mostly taken from Schimper's "Plant Geography," have been introduced.

This is an excellent account of plant physiology for its size. A few criticisms, however, suggest themselves. Through an attempt to crowd too much into a small space, inadequacy, if not inaccuracy, has sometimes resulted. For example, in the discussion on the cause of the ascent of the transpiration current, the evidence for the theories based on "the cooperation of the living cells" and "the cohesive force of water" respectively is slurred over in a dozen lines, and twice as much space is devoted to dismissing "capillarity" and "atmospheric pressure," which, all admit, are quite insufficient as causes.

It seems a pity that the idea embodied in the term "circulation of nitrogen" has not been given prominence. Recent results tending to bridge over the gap between the taking in of CO_2 and the appearance of carbohydrates in assimilation are not mentioned.

The paragraph on hybridisation has been enriched by a short account of Mendelism. It has not been made sufficiently clear that the Mendelian proportions 2 : 1 : 1, &c., are only approximated to when large numbers are dealt with, and are dependent on the laws of chance. A slight inaccuracy in statement occurs in the last few lines of p. 314. It is not 50 per cent. *of the serrate leaved individuals,* but of the whole second generation, which are of hybrid nature. These 50 per cent. exhibit the dominant serrate character.

The greater part of the special botany has been re-written. The order of treatment is, as in the previous edition, from the lowest to the highest forms. Most of the sections on the Thallophytes have been enlarged and altered, and two extra classes have been added by the separation of the Heterocontæ from the green algæ and the division of the fungi into the classes Phycomycetes and Eumycetes. Numerous excellent new illustrations have been included which greatly add to the value of this part of the book. Particularly noticeable are several new figures of the behaviour of nuclei in the reproductive phenomena of the fungi. The Rhodophyceæ do not seem to have received enough attention compared with the other groups.

Amongst the Archegoniatæ the mosses are little altered. The classification of the ferns has been re-arranged, and there are several new figures.

The remainder of the book, devoted to Phanerogams, has been completely re-written and re-illustrated. In the introduction to this part there is a very useful comparative table of the "Alternation of Generations." The treatment of the Gymnosperms, and especially of the Cycads, is distinctly good. In the Angiosperms, after a general description of each family, an account of some important genera of the family is given, the official plants of the British Pharmacopœia being noted in each case. This part of the book is very copiously and excellently illustrated, a large number of the figures being coloured. The colouring leaves something to be desired in several cases, but is much improved from the second English edition.

Throughout the work the arrangement and division into paragraphs is such as to secure the utmost degree of clearness. In each paragraph a leading idea is printed in larger type, so that it impresses itself on the memory, and serves as a centre round which the subsidiary ideas may be grouped. The usefulness of this is perhaps most noticeable in the part on physiology.

There are very few misprints. The only one worth noticing is on p. 280, line 41, where "heliotropism" should, of course, have been "geotropism."

The translator has been most successful in his work, the book reading as though originally written in English. In spite of the criticisms offered on a few points, this is one of the best, if not the best, text-book extant.

L. B. S.

FOREST ENTOMOLOGY.

Forest Entomology. By A. T. Gillanders. Pp. 422+xxii; 348 figures. (Edinburgh and London: Wm. Blackwood and Sons, 1908.) Price 15*s.* net.

MR. GILLANDERS, who is woods manager to His Grace the Duke of Northumberland, has produced a useful book for beginners in his "Forest Entomology." "The main feature which is attempted," we are told in the preface, "is recognition of the insect from the damage, together with systematic characters and life-history details." The first-named ideal has been well carried out, the figures given from photographs of the damage done being a great help to the practical forester and novice of forest entomology; we much regret, however, to see that the insects which cause the damage are frequently not shown at all.

The introduction, of eighteen pages, deals briefly with classification, metamorphosis, and structure.

This part of the work we shall hope to see thoroughly revised in a future edition. The figure (10, p. 9) of typical mouth parts of insects is very poor, and the figure (2, p. 4) of the eggs of the lackey moth are certainly not typical of that insect, if they are the eggs of it at all. Several other figures in this section are also very unsatisfactory, such as Fig. 3, showing typical forms of larvæ; in this latter we see no caterpillar, no sawfly larva, and those that are shown are very unnatural. The first chapter deals with the Eriophyidæ or gall mites. The more common species that we find in woods and forests have their galls figured. In reference to the literature consulted and quoted, it is a pity the most important writings on the big-bud mite of Lewis and of Warburton are not mentioned.

There are two chapters on Coleoptera, the first dealing with chafers, long-horns, and weevils, including the troublesome pine weevils (*Hylobius abietis*) and the two Pissodes; we should like to have read a good deal more concerning these and the destructive beech Orchestes (*Orchestes fagi*).

The second chapter, of forty-five pages, deals entirely with bark beetles (Scolytidæ). The wood sculpturing of these destructive insects is shown by means of photographic reproductions. This part of the book is nearly complete, and alone makes it of value to the forester.

Pages 130 to 163 deal with oak galls formed by the Cynipidæ, and the chapter contains a useful synoptic table. Chapter v. deals with sawflies, including descriptions of the injurious pine sawflies (*Lophyrus pini* and *L. rufus*) and the large larch sawfly (*Nematus erichsoni*); the same chapter contains all that is essential for the forest student on the Siricidæ or wood wasps and the strange Megastigmi, parasitic on the seeds of the Douglas and silver firs.

Scale insects or Coccidæ form the subject of chapter vi.; the more important forest species are briefly described, including the ash scale (*Chionaspis salicis*) and the felted beech coccus (*Cryptococcus fagi*). Nothing is said about the great harm done by the former, or any suggestions as to how we may easily check its ravages. Regarding the felted beech coccus, the author writes:—

"A most interesting remedial measure has been brought under my notice at Blagdon, in Northumberland. With an inch auger bore three holes at about equal distances right into the centre of the trunk about three feet from the ground and sloping slightly towards the root of the tree. Into these holes [place] as much 'flowers of sulphur' as can be conveniently got in and then cork them firmly up with a plug of soft wood. This should be done in autumn and will be found successful."

We may point out that this has been tried frequently, and the coccus has not been affected in the least, except about four inches around the auger holes! A comparatively small number of Lepidoptera are described in chapter vii., but some of the more important ones are mentioned, such as the goat moth, vapourer, winter moth, oak tortrix, pine shoot tortrix, larch coleophora, and the new larch pest, *Argyresthia laevigatella*. The chapter on aphides, a family of insects at present little understood, contains an account of the pine chermes, six species being detailed and their general effects well illustrated. The recent valuable work of Börner[1] will, however, have to be included in a subsequent edition. Naturally, Diptera take up only a few pages, mostly on gall-flies or Cecidomyiidæ, which cause various deformities or galls on leaves, buds, wood, &c.

Several Psyllidæ are detailed in chapter x., including species on the ash, hawthorn, alder, and box.

There are also chapters on collecting and preparing insects, one on insecticides and general remedies, and also a list of trees with injurious insects.

The subject of beneficial insects is very cursorily dealt with, only four pages being devoted to this interesting part of economic entomology, but the author tells us that "the field of beneficial beetles in forest entomology is rather an unworked one," and probably the same may be said of the other groups, and hence, wisely, a few pages only are allotted to this subject, which is of more interest than any practical importance. There are two points we are very disappointed with in this work, and these are that the author, with all his wide, *practical* knowledge, has not told us, firstly, more of his own ideas, and, secondly, more of how we can prevent and destroy these interesting forest insects, which levy such a heavy toll amongst our forests, woods, and plantations.

Had this been done, the work, coming as it does from such an authority on forest insects and their ways, would have been of much greater value.

FRED. V. THEOBALD.

[1] "Eine monographische Studie über die Chermiden." By Dr. Carl Börner. (Berlin, 1908.)

OUR BOOK SHELF.

An Introduction to the Theory of Groups of Finite Order. By H. Hilton. Pp. xii+236. (Oxford: Clarendon Press, 1908.) Price 14s. net.

IN many ways this book will prove a useful companion to treatises already available; especially, perhaps, on account of the large number of examples which it contains, and the hints for their solution. It may be confidently asserted that no example in group-theory is too elementary to be useful; the subject is on one side so very abstract, while on the other the individual properties of groups are numerous, and the protean disguises of the same group are amazingly varied.

The scope of Mr. Hilton's treatise may be indicated by stating that there is a chapter on Sylow's theorem, one on composition-series, and one on the characteristics of an Abelian group. All the main properties of Abelian groups appear to be mentioned; other groups that receive attention are those of the regular solids, and those known as Hamiltonian, linear homogeneous, and quaternion groups.

The chapter on characteristics is the last one, and does not profess to be more than a preliminary outline; it marks very well the limits of the author's plan, and will serve to induce the student to proceed to the very remarkable papers on this part of the subject by Frobenius, Burnside, and others. It is a pity that Mr. Hilton has not given a reference to these and some other of the most important memoirs; of course, no elaborate bibliography is expected in a work of this kind, but a select list would be useful to the beginner.

In writing on group-theory clearness is essential, and in this respect Mr. Hilton appears to be successful. Group-theory is so important that every advanced mathematical student ought to know something about its principles and methods. University teachers will now have a text-book which ought to help them in making the subject attractive and popular. A good many years ago Cayley foretold the development of group-theory, and his prophecy has been fully justified. The fact is that all analysis may be brought into connection with group-theory; and not only so, but in making this connection clear, we are submitting the particular subject (theory of numbers, algebraic functions, or what not) to its ultimate logical test, and disclosing its real and most fundamental basis.

It should be added that, with the help of Prof. Burnside, Mr. Hilton has given, by way of appendix, a list of twelve problems in group-theory which have not yet been solved. The best known of these is "Can a group of odd order be both non-cyclic and simple?" A definite answer to this question would give great satisfaction to students of group-theory, and as in the case of problems in higher arithmetic, a novice with a natural gift for these researches may succeed where the veterans have failed. G. B. M.

A Short History of Philosophy. By A. B. D. Alexander. Pp. xxii+601. (Glasgow: MacLehose and Sons, 1907.) Price 8*s*. 6*d*. net.

THE author offers this work as a substitute for G. H. Lewes's well-known "Biographical History of Philosophy," which, if for no other reason than that it was written expressly to discredit philosophy, has too long enjoyed its position as the one British attempt to exhibit the entire course of European speculation. Mr. Alexander does not emulate Lewes's literary brilliance, but he writes for a generation of readers who are willing to take the philosophic view even of philosophy, and to regard it not as a noxious counterfeit of knowledge, but as a necessary complement of positive thought at each epoch of man's history—an indispensable and highly significant part of the form and pressure of the time; such readers will welcome him as a competent and trustworthy guide to the salient features in the evolution of speculative thought.

The accounts which Mr. Alexander gives of the various systems of philosophy are clear and sound, and in all important cases have the vital quality that comes from first-hand acquaintance with the classics of his subject. He has dealt more fully with modern than with ancient philosophy, devoting nearly three-quarters of his book to post-Renaissance thinkers and more than half to writers since Hume. It is, perhaps, to be regretted that so much of the space rendered available by the author's restraint in the earlier stages of his enterprise has been given to German philosophers whose importance is national rather than European. It must be admitted, on the other hand, that the great names have received their due, and that, in particular, the chapters on Hegel will give renewed hope to many an honest student who has found the master himself only a shade more perplexing than some of his English interpreters.

The pages which we grudge to the lesser Teutonic lights might well have been used to make more adequate the author's picture of recent philosophical discussion in this country. The writer of a handbook for students must, of course, be reserved in his treatment of current controversies, but, in the case of a subject like the history of philosophy, he will give point to his whole work by a conclusion in which the questions of vital contemporary interest are at least indicated and set in their relations to the classical speculative movements. It is to be hoped that Mr. Alexander will find in a second edition of his useful work an opportunity of supplementing it in a manner which would render it still more acceptable to many others besides his scientific readers.

LETTER TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Size of the Mammoth.

SEVERAL references have recently been made in NATURE to the size of the mammoth, and I venture to present some notes on the subject, the result of several years' observation and measurements, principally of North American species.

Three good species of true elephant occur in North America—the northern mammoth, *E. primigenius*; the southern, or Columbian, mammoth, *E. colombi*; and the Imperial mammoth, *E. imperator*. The first of these is the one commonly known as the mammoth, and is the species found in northern Siberia and Europe. This attains a height of about 9 feet or 9 feet 6 inches, though an occasional specimen may exceed this, just as now and then an Indian elephant exceeds the average size of the species. The Columbian mammoth reached a height of 11 feet, and the Imperial mammoth 13 feet to 13 feet 6 inches, being, so far as I know, the tallest species of elephant on record. Unfortunately, the Columbian and Imperial mammoths are mainly known from scattered teeth and odd bones, so that their exact proportions cannot be definitely given, even in the case of the Columbian mammoth, the most complete specimen of which lacks the lower limb bones. It may, furthermore, be said that it is occasionally difficult, if not impossible, to say whether a given tooth belongs to the Columbian or Imperial mammoth, but the typical or full-sized specimen may readily be distinguished.

The three species noted above occupied fairly definite ranges in North America, although there was a great overlapping of their boundaries, particularly between the two southern species. The southern boundary of the northern mammoth roughly follows that assigned to the great North American ice-sheet, and the Columbian slightly overlaps this on the east and west, and in the interior of the continent runs far northwards. The Imperial mammoth is not positively known to have reached the Mississippi River, but extended south into Mexico and west to the Pacific coast. This is a westward extension of the range assigned to the species in the report of the Maryland Geological Survey, and is based on material examined since that report was published.

Referring to the mammoth in the museum of the Chicago Academy of Science, it should be said that this specimen has been restored, all the long bones being lengthened, and that the specimen stands certainly 2 feet higher than it should. It has been painted over, so that it is very difficult to tell where the original bones leave off and the restoration commences. The animal is probably the Columbian mammoth, and it is said that the skull is that of a recent Indian elephant.

Finally, a word might be said in regard to the American mastodon, the size and proportions of which are definitely known. This species rarely reached a height of 9 feet 6 inches, the majority of specimens running about 9 feet; but it was a much more heavily built animal than the mammoth or the Indian elephant, so that a specimen 9 feet 6 inches high would weigh from one-third to one-half more than an Indian elephant of the same height—that is, it would weigh from eight to nine tons.

Brooklyn Institute Museum. F. A. LUCAS.

SURVEYING FOR ARCHÆOLOGISTS.[1]

II.

Horizons—Earthly and Heavenly.

SO long as we are dealing with measurements of azimuth on the horizon, and altitude above the horizon, we are considering only our position on the earth—on that part of it which is bounded by our horizon. We are not dealing with the true position in the heavens of any body, whether sun or star, which may rise or set in the directions defined by our measures.

We are only dealing, in fact, with what is termed the *sphere of observation.*

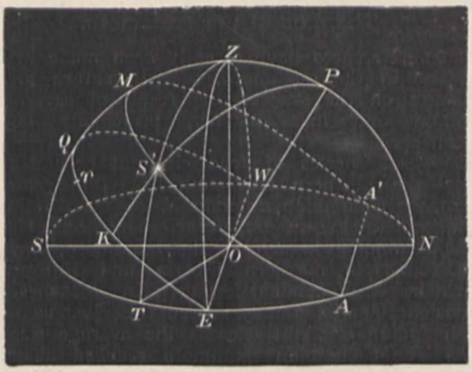


FIG. 4.—The sphere of observation. O, the position of an observer at the centre of the sphere, surrounded by the horizon; N. E. S. W. (the true cardinal points), with Z, the zenith, the point over the observer's head. The line N. S. is the true meridian line. The plane bounded by the line N. E. S. W. is the plane of the horizon, or the horizontal plane. The line O Z, a *vertical* line, is at right angles to the plane of the horizon.

The figure will illustrate what has already been said about azimuth, and will enable us to define some new technical terms which will be used later on. For *true* azimuths the zero is at N., which represents the N. point of the horizon, so that the azimuth of the E. point is 90°, of the S. point 180°, of the W. point 270°. Next let us take some intermediate points, A and T. The arc NA is the azimuth of A, the arc NT the azimuth of T. Sometimes it is convenient to define the position of a point on the horizon, not from the N. point (*azimuth*), but from the E. or W. point; we speak of this measure as *amplitude.* In any quadrant the one is the complement of the other, that is, added together, they make 90°.

The points A, T, like the points N. E. S. W., are represented as being on the horizon, so the distances of all these points from Z, called the *zenith distance,* are the same. If we represented these points not *on,* but *above* or *below* the horizontal plane, it is obvious that the zenith distances would not be the same. The higher the point is above the true horizon, as would happen if there were a hill there, the less the zenith distance.

The circle which we actually observe all round us when the heavens seem to rest on the surface which we see is termed the *visible horizon.* We imagine a plane parallel to the plane of the visible horizon, but passing through the centre of the earth; this is called the *rational* or *true* plane of the horizon.

So much for the horizon as a part of the earth's surface.

In the astronomical survey of ancient monuments, the determination of the azimuth of the various sight-lines, and the altitude of that part of the horizon which bounds them, is for the purpose of studying the sight-lines in relation to the rising or setting places of sun or star.

What we have to do, therefore, is to study the relation of the sphere of observation to what is called *the celestial sphere,* the sphere on which in old time the stars were supposed to be fixed by golden nails.

To do this we must pass from the consideration of the sphere of observation at any place to a study of the earth as a whole, and its movements, or at all events of some of them.

We have the earth in space with the universe of stars, almost infinitely removed, all round it, and we now know that the apparent movements of the stars from east to west, their daily risings, passing over the meridian and setting, in the sphere of observation at any place, are only the reflections of the earth's daily movement, or spin, on its axis from west to east.

The points at which this axis cuts the earth's surface are called the N. and S. poles, and half-way between these the earth is bounded by a circle called the *equator.* Now, as the daily motion of the earth is reflected in the apparent daily motion of the stars, so is the system of defining positions on the earth reflected in the system employed by astronomers in defining positions in the heavens.

As the earth is belted by *parallels of latitude* and *meridians of longitude,* so are the heavens belted to the astronomer with *parallels of declination* and *meridians of right ascension.* If we suppose the plane in which our equator lies extended to the stars, it will pass through all those which have no declination (0°). Above and below we have north and south declination, as on the earth's surface we have north and south latitude, until we reach the poles of the equator (90°). As on the earth we start from the *meridian of Greenwich* in the measure of *longitude,* so do we start from a certain point in the celestial equator occupied by the sun at the vernal equinox, called the *first point of Aries,* in the measure of what is termed *right ascension.*

So that we have terrestrial *latitude,* reckoned from the terrestrial equator, corresponding with celestial *declination,* reckoned from the celestial equator, and longitude corresponding with right ascension.

It is the *declination,* that is, the distance from the celestial equator, with which archæologists chiefly have to deal, for the reason that the rising and setting places of celestial bodies depend upon their declination; *bodies with the same declination rise and set in the same azimuths.*

Now the presentation of the plane of the horizon of a place to the surrounding stars which together constitute the celestial sphere varies vastly with its position on the earth's surface. Whether stars rise and set at all, or if they do whether they rise and set vertically or obliquely, depends upon this position, or, to be more precise, upon the latitude of the position. It is a pity that "calisthenics and the use of the globes" no longer form part of a liberal education, for a study of a terrestrial globe, which is a model of the earth in relation to the celestial sphere, gives us help in the matters we are now considering.

Such a globe is furnished with a wooden horizon, which represents the true or rational horizon passing through the centre of the earth as before defined. The axis of the globe prolonged is fixed into a brass ring representing the meridian, and the axis can be inclined at any angle in regard to the wooden horizon.

Now, wherever the archæologist is working, his

[1] Continued from p. 393.

observing place, bounded by his horizon, appears to lie at the top of the earth, and therefore parallel to

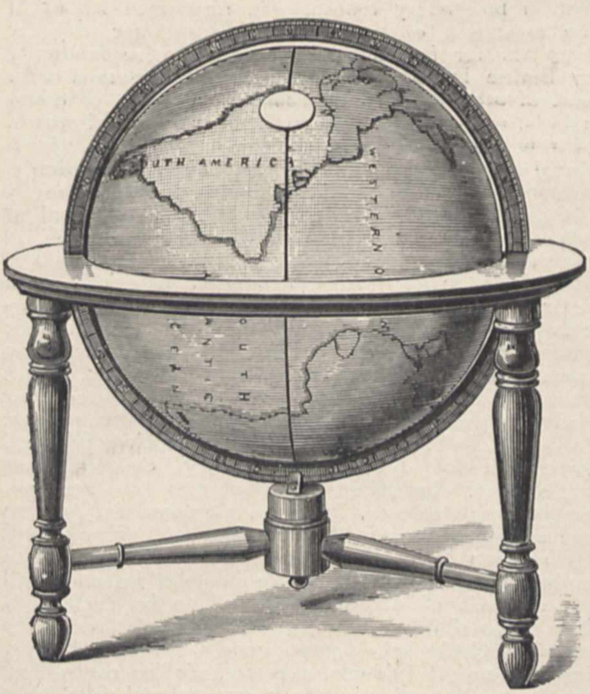


FIG. 5.—A model of the earth, showing that when the poles lie in the plane of the true horizon, and therefore of the wooden horizon which represents it, the horizon, represented by a wafer, of an observer situated on the equator, is carried vertically up and down by the earth's rotation; this motion reflected causes the apparent up-and-down motion of the stars as observed at the equator.

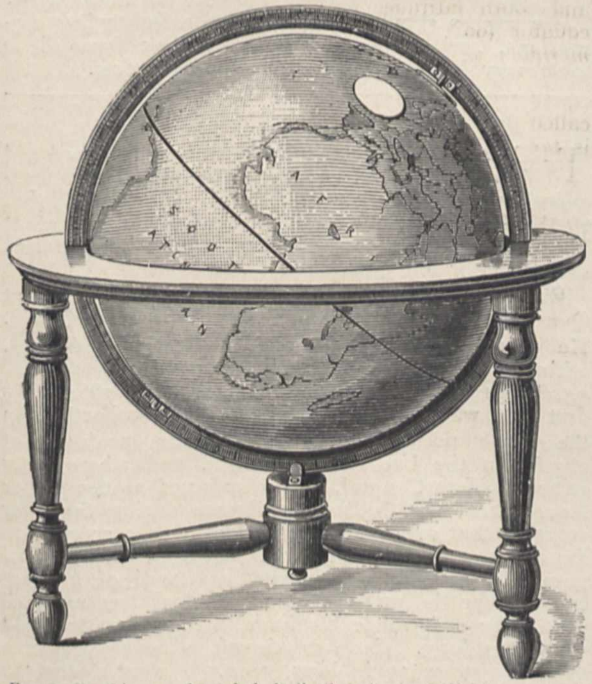


FIG. 6.—In this case the axis is inclined to the wooden horizon, which is parallel to the horizon of Britain when at the top of the globe. The wafer representing the horizon of Stonehenge is carried obliquely up and down in a direction parallel to the equator, so that the sun and stars rise obliquely to the horizon.

the wooden horizon; let us therefore use two wafers to represent local horizons, and place one on the equator and the second on Britain.

When we bring the equatorial wafer to the top of the globe, where it lies parallel to the wooden horizon, we find that on rotating the globe it sweeps down in a vertical plane. The wafer over Britain, parallel to the wooden horizon when it is brought to the top of the globe, when the globe is rotated takes an *inclined* path to the horizon. This happens because the axis, instead of lying in the plane of the wooden horizon, is inclined to it. This inclination of the axis varies with the latitude of the place, and so the angle of inclination of the path of the wafer to the wooden horizon varies with the latitude. If we so arrange our model earth that the inclination of the axis is the greatest possible and the earth's equatorial plane lies in the plane of the wooden horizon, it is obvious that the earth's movement will only cause a wafer at the pole to rotate; with this exception it will remain at rest, and as there is no vertical motion to reflect, the stars will neither rise nor set.

Now the value of these little experiments depends upon the already stated fact that the *apparent* movements of the heavenly bodies are brought about by the real movements of the earth, and the experiments show us that in regard to the horizon at any place the *true* movement of the underlying earth, and therefore the *apparent* movement of the overlying heavens, is vastly different.

At the equator an observer's horizon is being whirled round in a vertical plane at the rate of 1000 miles an hour; at the poles the horizon remains parallel to itself. In Britain we have a midway condition. Correspondingly with these differences, at the equator we have stars rising and setting vertically and rapidly; in Britain stars rising and setting obliquely and more slowly; at the poles the stars neither rise nor set.

We may now return for a moment to Fig. 4, which we have so far considered in relation to the sphere of observation. It really enables us to study as well the conditions of the celestial sphere for the horizon N. E. S. W. of, let us say, Stonehenge in lat. 51° N. P represents the position of the celestial pole, and EQW the inclination to the horizon of the celestial equator for that latitude. The lines EQ and ASM give the angle of slant as the sun or a star on the equator or in a northern declination rises above the horizon.

Two or three technical terms which will be often used afterwards may here again be referred to. PN gives the height of the celestial pole, which is the same as the latitude of the place, ZP its *zenith distance*; it will be seen that these are complementary to each other, that is, together they make up 90°. S representing a star or the sun, PS is its *polar distance*, as KS is its *declination* or distance from the equator; it is seen that these again are complementary to each other. The line SZ represents its zenith distance.

NORMAN LOCKYER.

INTERNATIONAL CONGRESS ON TUBERCULOSIS AT WASHINGTON.

THE International Congress on Tuberculosis to be held in Washington between September 21 and October 12 promises to be one of the most interesting and important in the history of these meetings. Presided over by the President of the United States, assisted by Dr. Edward L. Trudeau, acting as honorary president, Dr. John S. Fulton, secretary-general, Mr. Henry Phipps, of New York, as treasurer, an exceedingly strong national committee has been brought together, and very complete arrangements have been made for the exhibition of interesting plans and specimens, and for a full discussion of some of the more interesting questions and problems, medical and social, bearing upon the treatment and prevention of tuberculosis.

Great Britain, acting through an executive committee selected from a very large representative national general committee, and presided over by Sir William Church, with Dr. Theodore Acland as treasurer and Dr. J. J. Perkins as secretary, has for some time past been engaged in preparing a series of reports on the methods of combating the spread of tuberculosis and in carrying out treatment of this disease, which reports will be presented by national and other delegates.

When the executive committee was first formed the whole of the members met in London; but as soon as the general plan of work had been laid down it was decided that national committees in England, Scotland, and Ireland should meet in London, Edinburgh and Dublin, the Dublin committee being presided over by Her Excellency the Countess of Aberdeen. These committees have made the arrangements for the report from each country to be presented to the congress. They have also combined to send out an "exhibit" of plans, sections, pathological specimens, and other preparations for the large museum which has been arranged by the American Exhibition Committee working under Dr. Henry Beyer, of Washington. The keenest interest is being taken in the work of the congress, and President Roosevelt, in accepting the presidency, speaks of the modern crusade against tuberculosis as bringing "hope and bright prospects of recovery to hundreds and thousands of victims of the disease, who under old teachings were abandoned to despair. The work of this congress will bring the results of the latest studies and investigations before the profession at large, and place in the hands of our physicians all the newest and most approved methods of treating the disease—a knowledge which will add many years of valuable life to our people, and will thereby increase our public wealth and happiness. . . . Our country, which is honoured this year as the host of other nations in this great gathering of leaders and experts, and as the custodian of the magnificent exhibit which will be set up by the entire world, should manifest its appreciation by giving the congress a setting worthy of the cause, of our guests, and of ourselves. We should endeavour to make it the greatest and most fruitful congress which has yet been held, and I assure you of my interest and services to that end." Should this spirit pervade, as no doubt it does, the whole of the American executive, we may be assured of the fruitfulness of the congress.

From Great Britain Government delegates are being sent out in the interests of the various local government boards, and it is to be hoped, in view of proposed legislation on tuberculosis, that as full a report as possible of the work of the congress may be placed in the hands of those whose duty it will be to draw up legislation to be placed before the Parliament of the country. The universities, various medical schools and examining bodies, the Royal Commission on Tuberculosis, the Royal Society of Medicine, municipalities, the Victoria Jubilee Institute for Nurses, King Edward VII. Sanatorium, the National Association for the Prevention of Consumption, Invalid Children's Association, and other institutions are sending out representatives to assist in the discussion of such questions as the portals of entry, sources and channels of infection, especially the path of the tubercle bacillus from the exterior to the lungs, vital importance of early diagnosis, comparative importance of treatment in sanatoria near at hand, of entire change of climate, the present status of sanatoria treatment, diet in pulmonary tuberculosis, graduated labour in the treatment of tuberculosis, urgent necessity for hospitals for far advanced cases, relative frequency by bovine infection of lung disease compared with that of other organs, the economical aspect of tuberculosis, adverse industrial conditions, the social control of tuberculosis, after care of arrested cases, educational methods and agencies, promotion of immunity, responsibility of society for tuberculosis, &c.

One of the most interesting sections is that dealing with State and municipal control of tuberculosis, in which the provisional programme includes laws and ordinances relating to tuberculosis, especially with reference to notification, Government care of tuberculous patients, educational propaganda and scientific research under Government auspices, sanitary measures in the home, including disinfection, better housing, ventilation, &c., sanitary surveillance over travellers and those engaged in trades and occupations, in public buildings, factories and workshops, &c., prevention of tuberculosis among children and adolescents, including the subjects of heredity, environment, schools, factories, playgrounds, &c. In the section dealing with tuberculosis in animals and its relation to man, the prevalence of the disease amongst domestic animals, the modes of infection and the methods of diagnosis are all to be dealt with in a series of interesting papers, as are also resistance to tuberculosis in different genera, species, breeds, families and individuals, the methods for controlling the disease in animals, the comparative bacteriology and pathology of tuberculosis in animals, the relation of tuberculosis in animals to the pubilc health, including the evidence for and against the transmission of tuberculosis from animals to man, milk hygiene and meat hygiene in relation to tuberculosis in animals. These papers should lead to most enlightening discussions, and we may confidently look forward to some very interesting and important reports.

THE LATE M. MASCART.

THE ranks of French physicists have suffered sad losses of late. Last week it was Henri Becquerel whose obituary we published. To-day it is that of M. Mascart, whose death occurred on August 26 at his country residence at Poissy, where he had lain suffering for some months.

Éleuthère Élie Nicolas Mascart was born at Quarouble, near Valenciennes, on February 20, 1837. He was a scholar of the Ecole normale supérieure, taking his first degree in science in 1858, was admitted *agrégé* in 1861, and *docteur-ès-sciences* in 1864. His first post was that of conservator of the collections in the École normale. Then he became professor of physics in the Lycée de Versailles, and subsequently at the Collège Chaptal. He also acted as deputy for Regnault at the Collège de France during the later years of that great master; and in 1872 succeeded to the occupancy of his chair. Devoted to experimental physics, and, like his master, possessed of a great capacity for the methodical and patient treatment of details, he early made his mark in the scientific study of meteorology. It was therefore an appropriate appointment when in May, 1878, he was elected to the post of director of the Central Bureau of Meteorology in Paris. This post he filled for nearly thirty years, retiring only in 1907. He succeeded in the face of numerous difficulties in gradually perfecting the equipment and organisation of his bureau, and in establishing the systematic publication in France of weather-charts and weather-forecasts.

Mascart's earliest researches were chiefly devoted to optics; later electricity, magnetism, and the determination of the electrical units claimed his attention. The pages of the *Comptes rendus*, of the *Annales scientifiques de l'École normale*, and of the *Journal de Physique* attest his industry and his scientific insight. It must suffice here to indicate a few of his principal investigations. He was one of the first to apply photography to the study of spectrum analysis, and in 1862 constructed a spectrograph with a quartz train with which he photographed the ultra-violet spectra of many of the metals, adding many new lines to those already known, and directing attention to the harmonic relations presented by groups of lines, for example, by those of magnesium. He then made a number of standard determinations of wave-lengths by use of Nobert's gratings. Fizeau reported to the Academy of Sciences that this was the most thorough and satisfying piece of work on wave-lengths that had been made since the researches of Fraunhofer, and on his recommendation Mascart was awarded the Prix Bordin. He also prepared tables of the dispersion of the principal kinds of glass used by opticians, and of iceland spar. He devised, with M. Perrin, a novel optometer, and studied the distribution of the colour-sensation over the retina of the eye. In the theory of light he presented, in 1871, an elaborate memoir on the calculation of the interference fringes formed in different circumstances, carrying out the investigation with great generality, and giving the results of comparison between theory and experiment. He investigated the phase-relations in the light reflected from metallic films of great tenuity; and he wrote a series of didactic articles in the *Journal de Physique*, then (1872) newly-founded, on the application of the spectroscope to the observation of interference phenomena. He also produced an improved apparatus for the study of interference, based on the phenomenon of Talbot's fringes. For the study of colour-mixtures he devised an instrument producing three parallel spectra, each of variable intensity, which could be superposed on one another, and displaced so as to yield a mixture of any three spectrum tints in any proportion. An important paper in 1874, followed by another in 1878, was devoted to refraction and dispersion in gases, some twenty being examined. Another research dealt with the index of refraction of water under pressure. Doppler's theory also was examined, and an investigation was made, of great interest in the light of recent ether theories, whether the proper motion of the earth had any appreciable effect on the phenomena of optics. The conclusion was that optical phenomena give no indication of the absolute motion of a body, but only of its relative motion. This memoir was awarded the Grand Prix des Sciences mathématiques in 1874 on the report of M. Fizeau.

In 1876, M. Mascart published a treatise on statical electricity greatly in advance of any previously existing on the Continent in that it introduced to Continental readers the potential theory developed on the basis of Green's book, and the electrometric work of Lord Kelvin. The volume included several matters of original interest, comprising a research on discharges across long distances, and on the measurement of great differences of potential. About this time also he made new observations on atmospheric electricity, on the influence of ozone (*i.e.* air ionised by the passage of sparks) on the formation of fogs, and on the influence of electricity on evaporation. In 1877 he published in the *Journal de Physique* an elegant exposition of the elementary theory of magneto-electric and electrodynamic machines, based on the energy formulæ of Helmholtz and Thomson. In this article the law of efficiency of motors, at that date generally misunderstood, was correctly stated. In the succeeding year, in collaboration with M. Angot, he made many tests on Gramme machines and others, to test his formulæ. The influence of Lord Kelvin's volume of reprinted papers on electrostatics and magnetism now became very great on Mascart's work. He communicated to the Académie des Sciences a paper on the reciprocal action of two electrified spheres, employing Thomson's method of electric images; another paper on the propagation of electric impulses along conductors; and another on the theory of induction.

Public work began to fall upon M. Mascart, in connection with the electrical machinery shown in the Paris Exhibition of 1878; and, still more, in connection with the Electrical Exhibition and the International Electric Congress of 1881. In the congress he took an active part, particularly in the debates on the then burning question of the electric units. He contributed to the settling of these matters by a fine determination of the absolute electrochemical equivalent of silver, which he deposited from a nitrate solution, measuring the current in absolute terms by means of a current-weigher, a balance of his own design. The value found was about one-half of one per cent. below those respectively found by Lord Rayleigh and Prof. Kohlrausch. Between 1881 and 1884 he completed a re-determination of the unit of resistance, by the methods of Weber and Kirchhoff, finding as a result 106·3 centimetres for the length of the mercury column to represent the ohm, Lord Rayleigh's figures being 106·28 and 106·24. In these years also, he had, in conjunction with his friend M. Joubert, prepared a text-book of electricity and magnetism, based on his courses at the Collège de France. It introduced many points from the treatise of Maxwell, and the use of the C.G.S. system of units.

In 1884 he was elected to the Académie des Sciences in the place of Jamin. Of that distinguished body he became an active member, being at various times vice-president, perpetual secretary, and in 1904 president.

Being a man of affairs he was frequently in request to advise the Government on matters within his competence. He was vice-president of the consultative committee on arts and manufactures, and president of the commission on inventions for the War Ministry. He was also a member of the Bureau of Longitudes, and of the International Bureau of Weights and Measures. In recognition of his public services he was created Grand Officer of the Legion of Honour. He took a prominent part in organising the electrical sections of the exhibitions of 1889 and 1900, and in the latter year was president of the electrical congress which met in the exhibition. He was widely travelled, and had been an active member of the Chicago congress in 1893, which he followed up by a visit to the Yellowstone Park. He was profoundly interested in the establishment of the meteorological station at the top of the Eiffel Tower, and it was a particular pleasure with him, during the exhibition of 1900, to conduct parties of scientific friends to the special gallery above the highest to which the public had access, to show them the observing instruments therein installed.

Amidst these busy avocations he still found time to write. His "Traité d'Optique," in four volumes, which appeared between 1890 and 1893, possesses all the elegance of style peculiar to writers trained in the school of Laplace and Arago and Verdet. It is particularly rich in the sections of interferences and meteorological optics. In 1900 he published a "Traité de Magnétisme terrestre" in one volume.

Mascart was president of the Société française de Physique, and at another time of the Société internationale des Électriciens. He was elected in 1885 an honorary member of the Physical Society of London; in 1892, Foreign Member of the Royal Society; in 1900 vice-president, and in 1901 honorary member of the Institution of Electrical Engineers.

After his retirement last year, at the age of seventy, from the directorate of the Bureau of Meteorology, his health, which had suffered under his strenuous activities, broke down, and even the repose of his country residence failed to bring recovery. He was buried with military honours on Saturday, August 29, in the cemetery of Montparnasse.

THE LATE EARL OF ROSSE.

THE Earl of Rosse, whose death on August 29 has been already announced, inherited a name of great renown in science. It was during his childhood that his father, the third Earl, erected the mighty reflecting telescopes at his seat at Birr Castle by which the name of Lord Rosse became famous throughout the world. The third Earl was endowed by Nature with much mechanical skill, and as a means of utilising his tastes and opportunities in the best possible manner for the advancement of knowledge he commenced to make reflecting telescopes. Every detail of the work was carried out in the workshops which gradually grew about Birr Castle. Incessant experiments were made to improve the methods of casting, grinding, and polishing the specula, until at last his efforts culminated in the mighty six-foot reflector which even at this day, notwithstanding the advances of the last sixty years, has still the greatest aperture of any astronomical instrument in the world.

The great six-foot telescope at Birr, or Parsonstown, as the little country town used then to be called, soon gave abundant proof of its power. The most notable achievement was the discovery of the spiral nebulæ, which were not visible by any other telescope at that time existing. Indeed, the spiral nebulæ were not altogether credited in some quarters, until the advent of photography in recent years put an end to all doubts and showed that the spiral nebulæ abound in such myriads as to form, next to the fixed stars themselves, the most characteristic objects in the sidereal spaces.

It was under the shadow of the great telescope and amid such inspiring surroundings that Lord Rosse was reared. The sons of the third Earl inherited the mechanical tastes of their father, and joined eagerly in the practical work of the laboratories and workshops at Birr Castle. The eldest, Lord Oxmantown, succeeded to his father's scientific gifts no less than to his title and estates, and the youngest, the Hon. C. A. Parsons, following the natural development of his tastes from childhood, has achieved fame for his country as well as for himself by the splendid invention of the steam turbine.

The education which Lord Rosse derived from his father's precept and example was, of course, supplemented by the necessary education of a more conventional type. In this he was also exceptionally fortunate. The two first mathematical men of their year (1855) in Trinity College, Dublin, were John Purser, the late distinguished professor of mathematics in Belfast, and the Rev. T. T. Gray, who is at present a most respected senior fellow of his college. First one of these men (Gray) became resident at Birr, and to him the education of Lord Oxmantown was entrusted. He was succeeded by Purser, and under such admirable tuition the future Earl of Rosse developed much power in mathematics and its physical applications. In due course he entered Trinity College, Dublin, and had there a distinguished career.

The third Earl had been president of the Royal Society for several years, and his personal scientific distinction, as well as his unrivalled position as one of the most bountiful and most capable patrons of science, naturally placed him in intimate association with the leading men of science of the day. Sir John Herschel, Romney Robinson, Sabine, Fairbairn, Lyell, South, and many other distinguished persons in the middle of the last century were the friends of Lord Rosse. As Lord Oxmantown always resided with his father either in the ancestral home at Birr Castle or when a visit was paid to London, or a cruise was taken in their yacht, his years of early manhood were passed in close association with the illustrious friends of his father, and he had thus unique advantages of making acquaintance with science and with scientific workers. On one occasion (more than forty years ago) we know of Lord Oxmantown's spending a long day with Babbage, who was enthusiastically explaining to him the details of that wonderful analytical engine which would perform every description of calculation up to fifty significant figures that the mind of man could render into formulæ. Babbage had many parts of the engine to exhibit. But though the differential engine was to some extent completed, the much more formidable analytical engine had not made much progress beyond the drawings, in which, however, it was believed that the characteristic mechanical difficulties had been overcome. Another time, Lord Oxmantown and his brothers would be the guests of Wheatstone for an afternoon, who would explain to them his inventions of the moment, such as the original printing telegraph or the inverted stereoscope, that presented objects hollowed out instead of in relief. Even in those early days of electricity Gassiot, at his home in Clapham, showed to the great Earl, as well as to Lord Oxmantown and his brothers, his wonderful battery of many thousand cells by which effects which at that time seemed marvellous were produced.

A specially notable incident in the early career of Lord Rosse as an astronomer was a visit which he paid in 1866 to the observatory of Sir W. Huggins at Tulse Hill. It was a memorable time in modern astronomy. Huggins had commenced that great series of spectroscopic discoveries which, by the labours of himself and others, have so amazingly extended our knowledge of the heavens. On the night in question Huggins was observing the new star T Coronæ, which, after a few days of brightness, had then declined to the sixth magnitude. We are now so much accustomed to the outbreak of new stars and to the occurrence of bright lines in the spectra of such stars that it requires a special effort to recall the interest with which these discoveries were received at the time of their making. Huggins showed these lines to Lord Rosse, who also saw another most interesting object on that same evening. It was the linear spectrum of the first planetary nebula of which the gaseous nature had recently been announced.

With such opportunities and with the splendid instruments available at Birr, Lord Rosse devoted himself keenly to practical astronomical work. His first achievement was his magnificent drawing of the great nebula in Orion. It is probably the most elaborate piece of astronomical portraiture ever completed. It occupied about seven years of practically continuous work at all available opportunities with the six-foot reflector. The beautiful engraving which was made from Lord Rosse's drawing of the nebula is a familiar object on the walls of astronomical observatories. Among his other astronomical investigations we may mention those of the lunar radiation of heat. On this he was engaged up to the time of his last illness, and, indeed, at the recent meeting of the British Association in Dublin Sir Howard Grubb exhibited a short-focus mirror of remarkable construction which he had recently made at Lord Rosse's request to provide further instrumental power for his lunar work.

Lord Rosse had been Chancellor of the University of Dublin since 1885, and he served as president of the Royal Dublin Society (1887–1892) and president of the Royal Irish Academy (1895–1900). He was also one of the visitors of Greenwich Observatory.

Lord Rosse married in 1870 the Hon. Frances Cassandra Hawke, only child of the fourth Lord Hawke. He is succeeded by his eldest son, Lord Oxmantown, who was born in 1873. His second son is the Hon. Geoffrey Laurence Parsons, and his daughter, Lady Muriel Parsons, was married in 1906 to Colonel H. M. Grenfell, C.B.

THE DUBLIN MEETING OF THE BRITISH ASSOCIATION.

ONE of the largest and most successful among recent meetings of the British Association has just been concluded in Dublin. The following return shows the number of tickets issued in the various classes of members:—

Old life members	288
New life members...	24
Old annual members	459
New annual members	111
Associates	1,152
Ladies	222
Foreign members	14
Total	2,270

The various sections began work on Thursday, September 3. Most of them were located within the ample walls of Trinity College, and outlying centres were easily reached by means of the free motor service organised by an indulgent local committee. That service, creditable as it was, was greatly surpassed in value by the indicator boards announcing what papers were "up" in the various sections. These boards were mounted in places visible to everybody present. The letters A to L were written in large type in a horizontal row, and underneath each letter was hung a card bearing a number indicating the paper just being read in the section denoted by the letter. The service was maintained by four special operators per section, and it enabled members to bide their time comfortably in any section until their favourite paper took its turn in another. This useful innovation must be put to the credit of Prof. W. H. Thompson, one of the local hon. secretaries.

Thursday's sections began in a downpour of rain which contrasted unfavourably with the sunshine of the day before. The intersectional motor service was little in request, as nobody cared to leave the sheltering roof once it was reached. The various sectional meetings were, however, well attended, notably those of geology, educational science, and agriculture, the average attendance being about 150 per section.

About fifty members visited Guinness's Brewery at St. James's Gate at noon, and were shown over the vast works by the principal members of the scientific staff.

The Provost's garden party in the afternoon was largely attended in spite of the prevailing drizzle, though many members were absent. The Provost of Trinity College, Dr. Anthony Traill, braved the wintry blast manfully, and stood at the gate of the Fellows' Garden to receive his unexpectedly numerous guests. The latter kept to the marquees and the gravel walks, and enjoyed themselves prodigiously.

The conversazione given in the evening by the Royal Dublin Society at Leinster House proved one of the largest receptions on record. The 3000 members of the Royal Dublin Society were, of course, all invited, and as practically all the members of the British Association present in Dublin attended, the number of guests was more than 4000. The queue of carriages extended along several streets, and took two hours to discharge the occupants. On arrival the guests were received by Lord Ardilaun (president of the Society), the Right Hon. Frederick Trench, and Sir Howard Grubb. There were numerous scientific exhibits by local men.

In the lecture theatre Mr. W. H. Vipond Barry gave an organ recital, while subsequently lantern demonstrations were given by Rev. W. S. Greene and Dr. E. MacDowel Cosgrave, the subjects being "Scenes and Incidents in the West of Ireland" and "Old Dublin" respectively.

The Lord Lieutenant, accompanied by several members of his staff, arrived at 9.30, and spent some time inspecting the exhibits.

The sections started in full force on Friday, the weather having cleared up completely. The encounter between Sir William Ramsay and Prof. Rutherford in Section A drew a large attendance of distinguished physicists, but the keenest local interest was evoked by Mr. T. W. Russell's appearance at the section for Economic Science and Statistics, and the discussion on land purchase and the nationalisation of railways in which he took part.

A special meeting of the Senate of Dublin University was held at 2 p.m. for the purpose of conferring honorary degrees. The University Caput consisted of Mr. Justice Madden, Vice-Chancellor, Dr. Anthony Traill, Provost, and Mr. Frederick Purser, Senior Master. As each candidate was summoned to the dais, the Public Orator, Dr. L. C. Purser, proclaimed his titles and qualifications in Latin. The names of those who received degrees are to be found on p. 471 of the present issue under the head of University and Educational Intelligence.

Meeting of the General Committee.

At a quarter-past three o'clock a meeting of the general committee was held at Trinity College, Mr. Francis Darwin occupying the chair, when it was decided to hold the meeting of 1910 at Sheffield, and that of 1911 at Portsmouth.

On the motion of Sir Arthur Rücker, Prof. J. J. Thomson, F.R.S., was elected President for the Winnipeg meeting in 1909, the date of which was fixed for August 25 to September 1.

The afternoon engagements of Friday were divided between the general committee, a garden party at Dunsink Observatory, another at Saint Patrick's Cathedral, a visit to Messrs. Jacob and Co.'s biscuit and cake factory, and a special *matinée* of Irish plays at the Abbey Theatre, where Mr. W. B. Yeats gave an address on the recent development of native Irish drama.

The drive to Dunsink in special brakes was enjoyed by 200 members, who accepted the invitation of the Astronomer Royal and Mrs. Whittaker. The Observatory is situated to the north of Phœnix Park, and is best known to the Dublin public as the centre from which "Irish time" is furnished to the public timepieces of Ireland. The transit circle was, naturally, inspected with special interest.

Dean Bernard's garden party at St. Patrick's was also well attended, and was distinguished by the presence of the Lord Lieutenant of Ireland.

The Lord Mayor of Dublin took sixty members of the Association in his "flagship," the *Shamrock* (he is admiral of the Port of Dublin), down the Liffey to see the main drainage and electric light works at the Pigeon House, and gave a luncheon on board after the works had been inspected.

In the evening, Prof. H. H. Turner, F.R.S., gave an address on Halley's Comet at the Royal University before a crowded audience.

Saturday was devoted to excursions, in which 1100 persons took part. The objectives chosen were (1) the Boyne valley; (2) Bray, Powerscourt, and Kilruddery; (3) Glendalough; (4) the Rock of Cashel; (5) the Shannon and Clonmacnoise.

In the evening, the Classical Association of Ireland gave a reception in the Royal College of Physicians, and Dr. A. E. Tutton, F.R.S., gave the annual lecture to the operative classes before a large audience in the Royal University, choosing for his subject "The Crystallisation of Water."

Sunday was observed by special services in the Episcopalian, Presbyterian, and Roman Catholic chuches, the attitude of the respective churches towards science being expounded by the various preachers.

The afternoons of Monday, Tuesday, and Wednesday were set apart for garden parties at St. Anne's, Clontarf (Lord Ardilaun's home), the Zoological Gardens, and the Viceregal Lodge respectively.

On Monday, September 7, Prof. W. M. Davis, of Harvard University, gave a largely-attended lecture on "The Lessons of the Colorado Cañon."

The final meeting of the Association took place on Wednesday, September 9, at 3 p.m., at the Royal University.

E. E. FOURNIER.

Subjoined is a synopsis of grants of money appropriated for scientific purposes by the general committee at the Dublin meeting.

	£
Section A.—Mathematical and Physical Science.	
Turner, Prof. H. H.—Seismological Observations	60
Shaw, Dr. W. N.—Kites Committee	10
Preece, Sir W. H.—Magnetic Observations at Falmouth	50
Gill, Sir David—Establishing a Solar Observatory in Australia	50
Section B.—Chemistry.	
Roscoe, Sir H. E.—Wave-length Tables of Spectra	10
Divers, Prof. E.—Study of Hydro-aromatic Substances	15
Armstrong, Prof. H. E.—Dynamic Isomerism	35
Kipping, Prof. F. S.—Transformation of Aromatic Nitramines	10
Kipping, Prof. F. S.—Electro-analysis	30
Section C.—Geology.	
Lamplugh, G. W.—Fossiliferous Drift Deposits	11
Herdman, Prof. W. A.—Fauna and Flora of British Trias	8
Harker, Dr. A.—Crystalline Rocks of Anglesey	1
Gregory, Prof. J. W.—Faunal Succession in the Carboniferous Limestone in British Isles	10
Kendall, Prof. P. F.—Erratic Blocks	12
Lapworth, Prof. C.—Palæozoic Rocks	15
Watts, Prof. W. W.—Composition of Charnwood Rocks	2
Watts, Prof. W. W.—Igneous and Associated Sedimentary Rocks of Glensaul	20
Joly, Prof. J.—Investigations at Briskra	50
Section D.—Zoology.	
Woodward, Dr. H.—Index Animalium	75
Hickson, Prof. S. J.—Table at the Zoological Station at Naples	75
Herdman, Prof. W. A.—Hereditary Experiments	10
Shipley, A. E.—Feeding Habits of British Birds	5
Section E.—Geography.	
Murray, Sir John—Rainfall and Lake and River Discharge	10
Murray, Sir John—Investigations in the Indian Ocean	35
Section F.—Economic Science and Statistics.	
Palgrave, R. H. Inglis—Gold Coinage in Circulation in the United Kingdom	6
Cannan, Prof. E.—Amount and Distribution of Income	15
Section G.—Engineering.	
Preece, Sir W. H.—Gaseous Explosions	75
Section H.—Anthropology.	
Munro, Dr. R.—Glastonbury Lake Village	5
Myres, Prof. J. L.—Excavations on Roman Sites in Britain	5
Read, C. H.—Age of Stone Circles	30
Read, C. H.—Anthropological Notes and Queries	40
Hogarth, D. G.—Researches in Crete	70
Section I.—Physiology.	
Schäfer, Prof. E. A.—The Ductless Glands	35
Sherrington, Prof. C. S.—Body Metabolism in Cancer	20
Waller, Dr. A. D.—Electrical Phenomena and Metabolism of *Arum spadices*	10
Hickson, Prof. S. J.—Table at the Zoological Station at Naples	25
Waller, Dr. A. D.—Reflex Muscular Rhythm	10
Waller, Prof. A. D.—Anæsthetics	25
Starling, Prof. E. H.—Tissue Metabolism	20
Sherrington, Prof. C. S.—Mental and Muscular Fatigue	40
Section K.—Botany.	
Scott, Dr. D. H.—Structure of Fossil Plants	5
Oliver, Prof. F. W.—Botanical Photographs	10
Darwin, Dr. F.—Experimental Study of Heredity	30
Blackman, Dr. F. F.—Symbiosis between Turbellarian Worms and Algæ	10
Johnson, Prof. T.—Survey of Clare Island	65
Section L.—Education.	
Magnus, Sir P.—Studies suitable for Elementary Schools	5
Lodge, Sir Oliver—Curricula of Secondary Schools	5
Corresponding Societies' Committee.	
Whitaker, W.—For Preparation of Report	21
Total	1191

SECTION B.

CHEMISTRY.

OPENING ADDRESS BY PROF. F. S. KIPPING, D.SC., PH.D., F.R.S., PRESIDENT OF THE SECTION.

ON taking the Chair of this Section my first duty is to express my personal thanks to the Council of the British Association for having chosen me to fill this position of honour.

At this meeting the Association is enjoying, not for the first, but for the fourth time, the generous and genial hospitality of the citizens of Dublin; it is my privilege, on behalf of all the members of this Section, to tender our cordial thanks to our hosts for giving us this opportunity of meeting again in the capital of Ireland.

During the past few months we have read in the daily journals—and we sincerely hope it may be true—that there are signs of the commencement of a great development of the resources of this island; as such a desirable event must be closely connected with, and, indeed, may even be dependent on, the vitality of the chemical industries of the country, the moment seems opportune for the consideration of a subject which has a direct bearing on both commerce and chemistry.

Although this Section is chiefly occupied with matters relating to pure science, the discussion of industrial questions is also regarded as one of its important functions; it does not attempt to distinguish pure from applied chemistry, and any problem which concerns either is deemed worthy of its attention.

From this point of view I propose to consider whether any steps can be taken to place the chemical industries of the United Kingdom of Great Britain and Ireland in a more prominent position than that which they now occupy in the world of commerce.

The subject is not new; it has been dealt with by many, but principally by those more directly interested—prominent members of the Society of Chemical Industry, who are far better qualified to express opinions on commercial matters than am I. It is perhaps presumption on my part to attempt to add anything to what has been said by such leaders of industrial chemistry, but I propose to deal with the subject from a very different standpoint—namely, from that of the teacher in the class-room and laboratory. Even if I fail to make a single suggestion of immediate practical value, the question is one of such magnitude and so many-sided that I feel justified in bringing it under the notice of this Section. It is not merely a matter of money, of a few millions or of a few tens of millions sterling. There are few branches of industry to which chemistry, in one way or another, is not of supreme importance. Whether we look to the great shipbuilding interests, dependent on the progress of metallurgy; to our cotton and linen trades, where cellulose reigns supreme; to our dye-houses or to our breweries, or to any other industry, great or small, there do we find problems in chemistry awaiting solution, and the nation which solves them will not only progress in civilisation and contentment, but will also justly claim to have taken a leading part in the advancement of science.

It is not then in any grudging spirit of envy that we approach this question; recognising the splendid work of men of other countries, rejoicing in the services which they have rendered to the world at large, our only desire is not to lag behind in the general intellectual and industrial advance of nations.

It is unnecessary to trouble you with any detailed comparison of the position which we occupy to-day with that which we have taken in the past. The fiftieth anniversary of the epoch-making discovery of mauve was held only two years ago, and the proceedings are still fresh in our recollection; the pæans of congratulation addressed to the discoverer (now, alas! no longer with us) were marred by a plaintive note, a note of lamentation over our lost industry, the manufacture of dyes. The jubilee of the founder of the colour industry in this country was also the occasion for pronouncing its funeral oration. If this were the full extent of our loss we might bear it with equanimity; but it is not so much what has already gone as what is going and what may go that are matters of such deep concern. Those who doubt the seriousness of our condition may find statistical evidence, more than sufficient to convince them, in the technical journals and in the Board of Trade reports of recent years.

The facts there disclosed show that in the manufacture of "fine chemicals," including perfumes, alkaloids, and crude coal-tar products, as well as dyes, the decadence of our industry is far advanced; in the case of heavy chemicals our position, perhaps, is not quite so serious at the present moment, but the future is dark and threatening. Chemical industries are so intimately connected and dependent on one another that the fate of one may determine the fate of all; the by-product of one process is often the raw material of another. Who, then, can deny that the patience, perseverance, and high scientific skill, which have built up the colour industry abroad, if applied, as they have been and are being applied, to the manufacture of heavy chemicals, will not soon defy all competition from less progressive countries?

Such a possibility is full of national danger. It has been pointed out—and the prophecy cannot be regarded as unduly pessimistic—that from present indications a time will arrive when we shall be dependent on outside sources, not only for our food-supply, but also for our means of self-defence. When nitrates are exhausted, when nitric acid and ammonia are prepared from the components of the atmosphere, when all chemical industries have been so highly developed abroad that they have completely vanished from these Islands, and when their loss has reacted on all our other important industries, then, indeed, shall we feel the pinch of poverty; then, indeed, must we submit to national decay.

Is it possible to remedy the present unsatisfactory state of affairs, and to guard against an ominous future?

During the Perkin Jubilee celebrations Prof. Carl Duisberg answered this question, in so far as it concerns the coal-tar colour industry, by an uncompromising negative. In an able and interesting speech he pointed out that, although the Briton is in general a practical man, he is lacking in patience, in the power of waiting for success; he expects to be compensated in hard cash, and at once, for his work or for his capital outlay. The German, on the other hand, is primarily a theorist possessing endless patience, and works without any immediate prospect of pecuniary reward; he has now learnt to be practical as well, but not at the expense of his ideals. It is to this happy combination of qualities that Prof. Duisberg ascribes the success of his countrymen in the coal-tar colour industry—a success which he considers we are powerless to emulate, with which it would be futile for us to try and compete.

With this view that our chemical industries must submit to gradual extinction, even when it is held by so high an authority, we cannot and must not agree; if one nation can learn to be practical, we—the four nations of these islands—one or all, can learn to be plodding and patient, and to appreciate the importance of theory. We may be encouraged in our efforts to do so by the opinions of others, countrymen of Prof. Duisberg, eminent in pure or applied science. Prof. Ostwald, discussing this subject, said that he was sure the difficulties were considerable only in the beginning,[1] while Prof. Lunge, in an address to the Royal Institution,[2] made use of the following words: "Seeing that in pure science the people of Great Britain have never lagged behind any other nation, and that, on the contrary, the land of Newton and Faraday has been a beacon to all others at more than one epoch, there is absolutely no valid reason why she should now, or at any other time, be behind any other in the combination of science with practice."

Here, indeed, is encouragement, and from one who has had ample opportunity for studying the conditions which obtain in this country. Surely, therefore, we ought to have some confidence in ourselves and try our best to regain a strong and healthy position rather than fold our hands in a spirit of hopeless resignation.

The new Patent Act which came into force this year, and for which the country is so much indebted to the strenuous advocacy of Mr. Levinstein and Sir Joseph Lawrence, seems to many to have inaugurated a new era, and to have removed one of the principal causes of the decline of our chemical industries; if this be so, it is all the more important that the representatives of chemical science should be ready and willing to join hands with the manufacturers in order to assist in the process of regeneration.

The principal changes which have been introduced by the new law are, of course, familiar to all. The most important one, which came into operation on August 28 last, is that which requires that the article or process which is protected by the patent must be manufactured or carried on to an adequate extent in the United Kingdom after the expiration of four years from the date of the patent. If this condition is not fulfilled, any person may apply for the revocation of the patent.

Some of the results of this amendment, and some indications of the great industrial changes which it will bring about, are already obvious. Foreign firms or individuals who hold British patents and who have not sufficient capital to work them in this country, or who do not think they are worth working here, are attempting to sell their British patent rights. Others are building or buying works in Great Britain, and it has been estimated that in the immediate future a sum of at least 25,000,000*l.* of foreign capital will have been thus invested in order to comply with the new law.

We need not stop to consider the economic effects of this transfer of capital on the general trade of this country, but we may well pause a moment in order to try and forecast the consequences of these new conditions in so far as they concern our chemical industries.

The prospective establishment of branches of two of the largest German chemical works at Ellesmere Port and at Port Sunlight respectively are already matters of common

1 *Journ. Soc. Chem. Ind.*, 1906, 1019.
2 March 15, 1907.

knowledge, and it may be presumed that these firms will avail themselves to a large extent of British labour. If this be the case, and if they are successful—as they, no doubt, will be—the complaint that the inferior technical education of our artisans is responsible for our lack of success will thereby be proved to be groundless. Even if we admit that at the present time the British workman is an inferior operative in a chemical works, and only capable of undertaking the less-skilled labour, these firms will gradually raise a considerable number of trained men who will be ready to undertake more responsible duties under our own manufacturers when the good time comes; a school for chemical operatives will be created in our midst, and, as in the past, we shall reap the benefit of knowledge and experience brought to our shores. It also seems reasonable to expect that, as is the case abroad, these works will be equipped with laboratories and staffed by chemists, although possibly only so far as is necessary for routine work. Many of these chemists may settle permanently in our midst, become members of our Chemical Society and Society of Chemical Industry, and thus infuse us with their patience and perseverance. It is not beyond the bounds of possibility that these great firms may even employ British chemists in their works, if we can supply men sufficiently well trained to be of value. On the other hand, as experience seems to have shown that industrial chemistry cannot succeed with imported scientific labour, it is not very probable that many posts in the laboratory will be filled by our countrymen, who, in this connection, must be regarded as foreigners.

Now at the present time most chemical products can be manufactured more cheaply abroad than here, otherwise we should not have any reason to consider our position. Dr. Duisberg told us that even when an important firm in England had a licence to work all the British patents of two of the largest German colour works, merely paying for the privilege a small percentage of the net profits, it failed to take any advantage of the opportunity. If, then, in this free-trade kingdom production is cheaper than abroad, the foreign firms which have branches here will be in a position superior to that which they now occupy in their own countries. If, on the other hand, owing to inefficient labour, higher wages, freights, and other economic conditions, production is more costly, the superior efficiency and scientific organisation of these foreign firms will nevertheless enable them to command our home market with the goods made here, and to cut us out in the world market, as they do now, with those made abroad.

The conclusion which thus seems forced upon us is, that, although the new Patent Act will prove to be of great value in many respects, it will do little to foster British chemical trade and the development of British chemistry; it places us on an equality with other countries as regards patent rights, and thus remedies an outstanding grievance; but, unless we have something to patent, this equality will be valueless and our chemical industries will continue to decline, possibly more rapidly than heretofore.

Let us therefore pass in review the other causes which have been suggested as contributory to our failure; after eliminating those connected with freights and tariffs, and with the alleged supineness of the Government in assisting industry, matters which may be left to the manufacturers to deal with, there still remain several which are well within the purview of this Section.

These are: (1) the unsatisfactory condition of secondary education; (2) the nature of the training which is given to chemists in our universities and other institutions; (3) the insufficiency of the time and money devoted to research in the manufacturing industries; (4) the lack of cooperation between manufacturers and men of science.

There are some who believe that the first of these is the primary, if not the sole, cause of our weakness; that if our secondary education were placed on a sound basis all the other evils would disappear of their own accord; that a steady and broad stream of well-trained boys from the secondary schools would afford ample material from which good chemists could be fashioned in the universities and colleges; that these trained chemists would be greedily seized by the manufacturers, whose minds had been widened by improved educational methods; and once installed in the works these chemists would have no difficulty in persuading their employers to spend time and money on research work in cooperation with the leaders of science.

Whether such desirable and far-reaching results would in fact follow if our system of secondary education were very much improved it is impossible to predicate; but there is no doubt that at the present time we are moving in an exactly opposite direction.

The shadow of the cypress rests upon our chemical trade, and manufacturers do not see their way to employ chemists; students are not attracted to chemistry as a profession because there are so few openings; without an ample and increasing supply of such students chemical industry must continue to decline, and as a necessary consequence the development of pure chemistry is cramped and hindered to a far greater extent than is generally realised.

In a Presidential Address to the Chemical Society last year Prof. Meldola discussed the position and prospects of chemical research in Great Britain, and in view of the importance of the subject and the able manner in which it had been treated the Council of the Society ordered the publication of five thousand copies of his Address for distribution among the members of various public bodies. We were told in this Address that many of our universities are distinct failures as centres of chemical research, and that the output of original work from our colleges, polytechnics, and similar institutions is emphatically not representative of the productive power of the teachers there employed. The causes of the failure of our universities were only lightly touched upon, and I propose to refer to them later; but in the case of our other institutions they were more fully discussed. May I venture to direct attention to one cause, which I believe is by far the most effective drag on research in the vast majority of such institutions not of university rank? It is simply the lack of those more advanced students who, while gaining valuable experience in the methods of research, would also render useful assistance to their teacher. The governing body of the institution may not realise the importance of research; the Principal, as, alas! is sometimes the case, may throw cold water on such work; the teacher may be overburdened with routine duties, and he may be most inadequately remunerated; if, however, the research spirit is strong within him, he would overcome all these difficulties were there any prospect whatsoever of success; but what chance has he when he must do everything himself, even to washing out his own test-tubes? Provide him with a few advanced students, and he would doubtless find time to undertake the necessary pioneer research work, which would then be extended and developed with their assistance.

It might be suggested that an efficient and enthusiastic man would soon attract a number of research students. This, no doubt, is true as regards the universities, but it must be remembered that a polytechnic or other institution which does not grant degrees can hardly expect to compete with a university as a centre for research; all those students who intend to undergo a so-called "complete" course of study—that is to say, all who are likely to become capable of undertaking research work—naturally proceed to one of the degree-giving universities. There are not enough students to go round, to satisfy the research requirements of the teachers, and the principal reason is—the limited demand for trained chemists on the part of the manufacturers.

Even of the small number of those who leave our teaching institutions fairly well trained in research, how many have a chance of passing into works and directly advancing applied science? A very small proportion indeed. Most of the better ones drift into other posts, become demonstrators, emigrate—anything rather than wait on with the prospect of accepting as works-chemist a salary which, meagre though it be, may be stopped altogether if dividends are low.

With whom rests the responsibility for this state of affairs? Is it with the teachers, and, if so, is it because they are incapable of training chemists or because their system is at fault?

To answer this question it is necessary in the first place to arrive at some conclusion as to the kind of training which is required for the future works-chemist. On consulting the opinions of the manufacturers it would seem that they attach great importance to what is called the "practical side"; they believe that, in addition to a knowledge of theoretical chemistry, the prospective works-chemist should also have some acquaintance with engineering, should understand the apparatus and machinery used in the particular manufacturing operations with which he is going to deal, and should have had practical experience in working the given process. It is from this point of view that we build and equip large technological chemistry departments, such as those in the Universities of Birmingham and Leeds and in the Manchester Municipal School of Technology, departments fitted up with complete apparatus and machinery for carrying out operations on a miniature manufacturing scale.

The arguments in favour of this view, that it is a hybrid chemist-engineer who is required in a chemical works, seem to me to be fundamentally unsound, and the kind of training suggested by them for the works-chemist can only result in the production of a sort of combined analytical machine and foreman. A two or three years' course of science, followed by one year's practical work in the dye-house, in paper-making, or in some other technological department, is quite inadequate if the student trained in this way is expected to do anything beyond routine analytical work and supervision.

We cannot possibly expect such a poorly trained Jack-of-all-trades to run a chemical works successfully in the face of competition directed by a large staff of scientific experts in chemistry and in engineering. It is no use spending immense sums of money on expensive machinery of the newest type in order that the works-chemist may be able to tell his future employer that the machinery used in his employer's works is completely out of date. In the course of time, moreover, unless expenditure is practically unlimited, the reverse conditions will obtain, and the technological department of the university or other institution will become more of the nature of a museum of antiquities. The great cost of the upkeep and of the working of such plant is also a very serious matter; is it possible to believe that the educational results of running, say, a large puddling furnace, such as is fitted up in Birmingham University, are in any way commensurate with their cost? The conditions in a chemical works cannot be successfully imitated in a university or polytechnic; attempts to do so can only lead to mistaken conclusions, and thus have the effect of rendering the works-chemist quite helpless when he passes from the elegant models of his educational apparatus to the workaday appliances of the manufactory.

Here, it seems to me, we touch the bed-rock of our trouble. The state of our chemical industries must be attributed to the erroneous views which have been and still are held as to the functions, and consequently as to the training, of a works-chemist. We have failed to realise that industrial chemistry must be based on a foundation of continuous and arduous research work. In the past we have sent out from our universities and other institutions students who no doubt were qualified to undertake routine analytical work, but the great majority of whom knew nothing of the methods of research. We are doing the same to-day. Just when a student has reached a stage at which his specialised scientific training should begin his course is finished, and whether he has been to a university or to a polytechnic matters little; he joins the band of those who subsist on but who do nothing to advance chemical industry. He enters a works; the manufacturer does not realise exactly what his chemist ought to do, but he expects some immediate results, and in consequence is generally disappointed; the lack of success of the chemist is put down to his ignorance of practical matters, and there is an outcry for technical education; science is most unjustly discredited, and any suggestion of spending money on research work is scouted as a mere waste.

The consequence is that if there is a scientific problem which intimately concerns all the members of some large industry what course do they adopt? Through their trade journal, and as an association representing a total capital of which I should not like to hazard a guess, they offer a bronze or possibly a silver medal, or may even offer the extravagant sum of 20*l.*, to the happy person who will provide them with a solution. It is difficult to imagine the class of solvers to whom these princely rewards may appeal, more difficult still to believe that any useful result can be attained, and it is almost incredible that such methods should be adopted by any influential industrial organisation. This way of attempting to get research work "on the cheap" is certainly not unknown even in more enlightened countries, but that is hardly a sufficient justification for its employment.

Contrast these methods with those adopted by the Badische Anilin- und Soda-Fabrik and Meister, Lucius, and Brünig in their attempts to solve the problem of the commercial synthesis of indigo. Could there be a greater antithesis? If five thousand copies of Brunck's Paper on this subject[1] could be circulated among the manufacturers of this country—a task which might be fittingly undertaken by the Society of Chemical Industry—the study of the truly magnificent results attained by the systematic application of pure science, and of the indisputable evidence of their commercial value, might prove an object-lesson far more effective than argument for the accomplishment of a sorely needed reform.

Now if we are to meet successfully the very formidable scientific and commercial organisation opposed to us in chemical industry, we must perforce adopt the methods of our competitors; not only must we learn patience and perseverance, but we must also call to our aid the best brain-power available. We must recognise clearly that the scientific works-chemist, the only man who is likely to make discoveries of commercial value, must be thoroughly trained in the methods of research by those best qualified to do so, and we must not imagine that when he enters the works he should or could immediately become an engineer and a commercial expert; his place is in the research laboratory. The practical man—that is to say, the man who has a thorough and useful knowledge of some particular manufacturing process—must be trained under practical men in the works, and we must not imagine that a course of evening classes will convert him into an expert chemist. The ideal man who combines high scientific training and sound practical knowledge cannot be produced unless the period of his education is extended to half a life-time, and even then only through the cooperation of the chemistry teacher and the manufacturer.

Admitting the truth of these statements—and I do not think that they can be successfully controverted—we have now to consider what steps can be taken to provide these highly trained works-chemists, and to ensure for them a cordial reception on the part of the manufacturers.

The first fact which we have to bear in mind is that the great and rapid development of chemistry in recent times has lengthened the period which is required for the collegiate study of the subject. In order to acquire the necessary knowledge of facts and theory, and afterwards to devote even the minimum time to gaining experience in research methods, the future works-chemist must be prepared to continue at the university or other institution during at least five years. The course of study during the first three years might be on the lines now adopted by many of our universities for the B.Sc. pass examination, but to grant this degree in one or two subjects only, and then to call it an Honours degree, is in my opinion a serious mistake, as is also the admission of research work at this stage, both of which proceedings lead to far too early specialisation. The pass degree should be regarded merely as an indication of a sound general education in science, and the future works-chemist should then devote at least two years more to research and to special work in chemistry, on the results of which the Honours degree might be awarded. Every encouragement in the form of low fees, free admission, research scholarships, and so on, should be offered to such students, according to their merit and circumstances, in order that they may prolong their studies; the cost of these remissions or awards would not be very serious, and the money would be well spent. Teachers should then refuse to recommend, and manufacturers should refuse to employ, as a works-chemist, any student who had not passed through such a course satis-

[1] *Ber.*, 1900, i., lxxi.

factorily, unless it was understood that he was only expected to undertake routine analysis or work outside the research laboratory. By thus extending the period of training, and making research work compulsory as far as possible, a great deal would be gained; pure science would reap an immediate benefit from the investigations of the students—as has been the case abroad—and this stimulus would necessarily react on industrial chemistry; the manufacturers could be assured that they were being supplied with men of the right type; they would soon come to recognise that fact, and the demand for works-chemists would expand. In the laboratory of the works the manufacturer would then have the opportunity of gauging the capabilities and special leanings of every chemist on his staff. Those who were best fitted for directing operations in the works could be trained on the spot, as they could not possibly hope to be trained in any university or polytechnic; those who proved to be the best research chemists would, of course, remain in the laboratory working out scientific problems. Organisations of this kind could not fail to command success, and the opsonic curve of our chemical industries would soon begin to rise.

There is one institution, not a teaching body, which might greatly assist in this movement; I refer to the Institute of Chemistry of Great Britain and Ireland. This body desires and claims to represent chemistry, not only in these islands, but in all our dominions, and also to exercise some supervision or control over public appointments. It examines in chemistry and grants diplomas, and claims that its examinations are a test of practical ability rather than of theoretical knowledge. I have not a word to say against the character of these examinations, but to imagine that the Institute of Chemistry qualification is the hall-mark of a chemist is ridiculous. An average student can obtain the diploma after three, or at the very most four, years' work subsequent to matriculation, and more easily than the London B.Sc. (external). Here, again, it should be recognised that the present Institute of Chemistry qualification is only a step in the training of a chemist; the permission to present a thesis for the Associate examination should be withdrawn, and good research work should be insisted on in the case of all candidates for the Fellowship. It would then be possible to distinguish between those who are capable routine chemists and those who might be expected to advance pure and applied science. It is certainly a grave matter for an Institution entirely controlled by chemists to set such a bad example by ignoring the necessity of research work; if all our official chemical appointments and many of our posts in works are to be filled by men who have done no independent scientific work, the results will be most serious; the research habit and the research method are not easily acquired without assistance, and therefore it is all the more important to make use of this assistance while it is within reach, and before the budding chemist begins to believe that he has nothing more to learn.

As a necessary corollary to making research compulsory in the training of works-chemists, all our important teaching institutions must afford ample opportunities for such work, and measures must be adopted to guard against that failure of some of our universities as centres of research which was pointed out by Prof. Meldola.

Such failure, whatever may be the contributory causes, must be principally due to the absence of sufficient interest in research work on the part of the professor, and it certainly seems surprising, at first sight, that in these days many such professors are to be found; but it must be remembered that although by members of this Section research work is regarded as the highest and most important of all professorial duties, this is not always the view of those who make an appointment to a Chair.

In selecting a professor there are many other considerations which come into play: his ability as a teacher in the class-room and laboratory; his qualifications as a popular exponent of science; his power of organisation; his bearing towards his colleagues and his students—all these matters are of great and direct importance to a university, and it is not to be wondered at that a man highly qualified in these accessories may sometimes be chosen even though he may take no special interest in research work.

The results of such an appointment, however, cannot fail to be most prejudicial to the highest interests of the university and of the country; the chemistry department becomes a chemistry school, but not a school of chemistry.

Unfortunately, moreover, the results extend over a long period: this raises another question which certainly requires attention if we are to become more efficient.

It is far from my object to create any gratuitous insecurity of tenure in chairs of chemistry, but is it not desirable that in our teaching institutions the conditions of all appointments should include a superannuation clause? Not that a rigid age-limit should be introduced, but there should be a possibility of bringing about the retirement of those who for any reason can no longer adequately fulfil their duties. When, owing to the lapse of time, such retirement became necessary, the aged and honoured professor, pensioned by a grateful university, might still retain an intimate connection with its scientific life; as emeritus professor, with a research laboratory at his disposal, he might remain to advise and encourage his youthful successor even when the duties of teaching and the general supervision of a department had become too arduous.

It cannot be suggested that my remarks on this delicate topic are inspired by the impatience of youth or by freedom from personal consequences; the time when superannuation becomes desirable may arrive for one and all, and I have ventured to direct attention to the matter simply and solely because of its grave importance in connection with the subject of my Address. The country cannot afford to allow periods of inactivity or decadence in our seats of learning, and the interests of the individual must be subordinated to those of the nation.

Even if by adopting the above suggestions the training of our chemists is improved, and all our higher educational institutions become permanent and active centres of research, the manufacturers may still remain unresponsive, what can be done in other ways to bring about the active cooperation of pure and applied science?

The great proportion of the original work now done in this country, judging from the published records, is absolutely free from any utilitarian bias; the time, brain-power, and money devoted to this work are considerable, and the results from a scientific point of view eminently satisfactory. If even a fraction of the same skill and energy were brought to bear under proper conditions on problems of applied science, who can doubt but that the effect on our chemical industries would be one of vast importance? And yet it is the rarest possible occurrence to find any record of research work undertaken with a commercial object even in the natural home of such records, the Journal of the Society of Chemical Industry.

One reason for this may be that the discoveries made in the works-laboratories are not given to the world at large, but are quietly and lucratively applied in some secret manufacturing process. Another reason, unfortunately the more probable one, may be that nearly all the principal research workers are completely shut off from any industrial influences.

Now the worker in pure science, unaided by the advice of the manufacturer and business man, has little chance of solving any important technological problem, except as the result of accident; he has not the requisite acquaintance with commercial conditions, does not realise the enormous difference between operations on the laboratory and the manufacturing scales, or, if he does so, is unable to enter fully and with confidence into questions of fuel, labour, and so on which often determine the success or otherwise of a process. Further, much of the research work of direct commercial value concerns methods for reducing the cost of processes already in operation, and demands an intimate practical knowledge of these processes.

It is obvious, therefore, that, even if all the research capacity of the country were henceforth devoted to purely technical matters, any great improvement in our industries could hardly be anticipated without the active cooperation of the manufacturers.

Now it has been stated [1] that the authorities of the Manchester Municipal School of Technology intend to under-

[1] Levinstein, *Journ. Soc. Chem. Ind.*, 1903, 845.

take investigations for local manufacturers and merchants in connection with difficulties which may be met with in their works or business. This method of securing the interest and support of those engaged in applied chemistry may or may not be workable according to the conditions under which such cooperation is carried out. The staff and the laboratories of a university or polytechnic cannot be placed at the unrestrained and gratuitous disposal of any manufacturer who is in some trivial difficulty, nor can the choice of the subjects to be investigated be decided by the governing body. If however the arrangements, pecuniary and otherwise, are left entirely in the hands of those directly concerned, namely, the manufacturer and the responsible head of the chemistry department, the scheme should then prove exceedingly valuable, and should be adopted as widely as possible. It should be understood, and the fact might even be advertised by the governing body, that for purposes of research work in applied chemistry—but not of course for analytical work—the laboratory of the university, college, or polytechnic is, under certain conditions, at the service of the manufacturers; that although primarily and unswervingly devoted to work in pure science, such institutions recognise that, for their own interests, they must do all they can to assist chemical industries.

It might be thought that these conditions prevail at the present time, and that any manufacturer, if he so choose, may consult the university staff on any problem in which he is interested. Possibly this is true to a limited extent, but in most institutions the members of the staff are restrained from undertaking any outside work; in others, such work may only be done with the sanction of the authorities.

These conditions, of course, are only laid down because the governing body believes that they safeguard the interests of the institution, and if it were shown that their enforcement is really contrary to those interests they would soon be abrogated. Many or all such authorities readily permit the members of their staff to undertake outside examination work because they consider this course to be to the advantage of their institution; but how incomparably more important is the object of gaining the confidence and support of the manufacturers.

Pray do not let it be imagined that this is some subtle scheme for increasing the pecuniary rewards of the teachers. I greatly fear that to many of those who are now engaged in research work the suggestion that they should give some attention to applied chemistry would be very distasteful, simply because it would involve an immediate encroachment on the time, already far too limited, which they are able to give to the immediate scientific problem which is one of their principal joys in life. To those who might have fears of this kind I would point out that there would soon be some compensation; once the cooperation of the manufacturers is secured, the demand for research chemists would expand, and the laboratories would be filled with students whose help in pure science would be invaluable.

The possible objection that the teaching staff would devote too much time to applied work and neglect other duties is one which could be left for the governing body to deal with unsparingly. If the institution took some percentage of all extraneous remuneration, or any similar arrangement were made, the funds thus provided could be used for increasing the staff of assistants and demonstrators—a most desirable reform in itself.

One of the greatest advantages of a working arrangement such as that here indicated would be that, like the method already suggested, it would lead to the evolution of what is otherwise almost unattainable—namely, men thoroughly trained in both science and practice. The research students of the teaching institution, engaged on a given problem for a manufacturer, would of course be allowed to study its practical aspects in the works; on the other hand, works-chemists, with considerable practical experience, would be granted permission to proceed to the university laboratory, where they would study the problem with the assistance of the highest scientific knowledge, and acquire further training in the methods of research.

Combinations such as these could hardly fail to lead to valuable results, which would form the subject of patents; the monopolies thus acquired would place the manufacturers in a favourable position, and the revival of our chemical industries would follow in due course. There is nothing Utopian in this scheme, and there are no great initial difficulties to be overcome; it may be set in operation by the manufacturer, and possibly also, as will be indicated later, by the worker in pure science. Reading between the lines certain records which have recently appeared in the science journals and the patent lists, it may even be inferred that such arrangements are already in force in one of our large industrial centres.

There are other ways in which it might be possible to obtain the active cooperation of the manufacturers. Any individual or firm interested in a problem of applied science might be invited to found a temporary research scholarship at the university or other institution for the definite object of the particular problem in question. The maximum period during which such a scholarship would be tenable might be fixed beforehand, so that the financial liability of the founder would be limited and proportionate to the importance of the object in view. The holder of the scholarship might be nominated by the university, or by the founder and the university jointly, and suitable conditions would be drawn up to ensure the interests of the founder; he would of course have the benefit of all the results of the work, and would secure the patent rights of any new invention, subject possibly to the payment of a small percentage of the profits to the university and to the holder of the scholarship. During the tenure of the scholarship, the holder, and also the founder, would have the advantage of the scientific knowledge of the university; the scholarship holder would also be allowed to gain practical experience in the works, and, if successful, there is little doubt but that he would have the option of working the process on the large scale and of obtaining permanent employment under satisfactory conditions. After a given period the scientific results of the work would be published through the usual channels in the ordinary way.

This idea of applied research scholarships had taken shape in my mind when I happened to come across a book recently published in the United States, called "The Chemistry of Commerce," in which I found that a similar proposal had been made by the author, R. K. Duncan, Professor of Industrial Chemistry at the University of Kansas. The scheme is there worked out in some detail, and a form of legal agreement to be signed by the university authorities and by the founder of the "Industrial Fellowship" is suggested.

Thinking it would be of interest to know how the plan had worked out in practice, I wrote to Prof. Duncan and received a reply a few weeks ago. He very courteously informed me that five industrial fellowships had already been established in his laboratories, that the agreements for two additional ones were being prepared, and that he might have obtained more, but wished to proceed conservatively; also that he had no reason to doubt the entire practicability of the scheme, and that experience had shown that the terms of the agreement could be made more favourable to the university than those which were first drawn up. One of the new conditions is that the industrial fellowship holder shall give two hours a week gratuitous instruction in the work of the chemistry department—an arrangement which has proved to be of great inspirational value. The fellowships are tenable during two years, and are of the value of 500 dollars or 1000 dollars per annum.

It is too soon to be able to form any opinion as to the commercial importance of the work carried out under this scheme, but it is obvious that the foundation of such scholarships for the study of general or special problems in applied chemistry is most desirable. One of their great advantages would be that they might be founded by those manufacturers who cannot afford permanently to engage a research chemist. Large and successful firms like the United Alkali Co., Brunner, Mond and Co., and many others which can employ a staff of chemists, are of course eminently capable of managing their own affairs without outside assistance or advice, and it is only for those which are less prosperous that the foregoing suggestions are made.

The great benefits which are conferred on pure science by the open research scholarships at present available afford some indication of what might be done for industrial chemistry by the foundation of such scholarships in applied science. There are, no doubt, scattered over the country many men who possess originality and inventive talent, and who have practical experience in industrial operations, but who have not been sufficiently trained in science; if it were possible to attract this dormant talent by means of open scholarships it might be directed into proper channels instead of being allowed to run to waste.

It is easy to say how money might be spent advantageously, but very difficult to suggest how the funds for such open scholarships should be raised. An appeal to the manufacturers by this Association or by the Society of Chemical Industry might meet with some response, and it is also possible that public bodies might render assistance. If the Government of Bengal, under the spur of dire necessity, can subsidise research work on indigo, and if our county councils can offer scholarships for dairy work, and grants for experiments on turnip-growing, bee-keeping, and so on, our city and borough councils might award scholarships in applied chemistry for subjects of especial importance to the dominant trades of the district. By so doing they would be utilising to the best advantage the chemistry departments of our universities and polytechnics.

I noted a few moments ago that practically all the published research work of this country has no direct reference to any industrial problem; nevertheless the results of this work are often of such a character that they might be of considerable technological importance. New reactions are discovered; new or improved methods of preparing known compounds; new facts as to the conditions under which important general reactions occur; and, needless to add, a great many new compounds are prepared.

Now, abroad, all or nearly all such matters are protected by patents, generally taken out by some firm of manufacturers. To the uninitiated it seems absurd to think that there is money in the great majority of such patents, and yet it is obvious that the employment of this system must pay in the long run. Why should it not be adopted in this country—at any rate to a limited extent to start with?

If all those who are engaged in purely scientific research work would seriously consider the desirability of obtaining provisional protection for any discovery which they may make, and would then consult some manufacturer or industrial expert with whom the further development of the matter might be undertaken, there is reason to believe that in some cases at least the patent might prove to be a commercial success.

The examination of the therapeutic action of compounds discovered in our laboratories is also a possible means of assisting our chemical industries; the matter is not so trivial as it may seem; a monopoly in the manufacture of some valuable medicinal preparation would serve as a *point d'appui* from which more important operations could be undertaken.

Unfortunately the investigation of the physiological action of new preparations is a matter of some difficulty in this country, as it is to some extent connected with vivisection in the public mind; we may poison rats with impunity, and even create an organisation for their extermination, but we may not individually try the effect of a new compound on a rabbit.

In drawing this Address to a conclusion I cannot but feel that my suggestions may seem utterly inadequate to the attainment of those important results which are so greatly to be desired. If so, I can only plead that more drastic measures are hardly available, and that even in the most favourable circumstances improvement can take place only very slowly. Whatever differences of opinion may be held as to the details of any scheme for regaining our lost ground, the main lines seem to be clearly indicated. The workers in pure science must recognise that it is their duty to do all they can to promote the industrial welfare of their country; the manufacturers must concede the paramount importance of science and the impossibility of dispensing with its counsels. Guided by these principles and by a spirit of cordial cooperation, a sustained and strenuous effort on the part of the leaders of chemical industry and of chemical science can hardly fail to accomplish the end in view.

In elaborating this Address I have enjoyed the advantage of the criticisms and suggestions of my friend and relative Prof. Perkin, F.R.S., to whom my sincere thanks are here expressed.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY PROF. JOHN JOLY, M.A., D.Sc., F.R.S., PRESIDENT OF THE SECTION.

URANIUM AND GEOLOGY.

Introduction.

IN our day but little time elapses between the discovery and its application. Our starting-point is as recent as the year 1903, when Paul Curie and Laborde showed experimentally that radium steadily maintains its temperature above its surroundings. As in the case of many other momentous discoveries, prediction and even calculation had preceded it. Rutherford and McClung, two years before the date of the experiment, had calculated the heat equivalent of the ionisation effected by uranium, radium, and thorium. Even at this date (1903) there was much to go upon, and ideas as to the cosmic influence of radio-activity were not slow in spreading.[1]

I am sure that but few among those whom I am addressing have seen a thermometer rising under the influence of a few centigrams of a radium salt; but for those who pay due respect to the principles of thermodynamics, the mere fact that at any moment the gold leaves of the electroscope may be set in motion by a trace of radium, or, better still, the perpetual motion of Strutt's "radium clock," is all that is required as demonstration of the ceaseless outflow of energy attending the events proceeding within the atomic systems.

Although the term "ceaseless" is justified in comparison with our own span of existence, the radium clock will in point of fact run down, and the heat outflow gradually diminish. Next year there will be less energy forthcoming to drive the clock, and less heat given off by the radium by about the one three-thousandth part of what now are evolved. As geologists accustomed to deal with millions of years, we must conclude that these actions, so far from being ceaseless, are ephemeral indeed, and that if importance is to be ascribed to radium as a geological agent, we must seek to find if the radium now perishing off the earth is not made good by some more enduringly active substance.

That uranium is the primary source of supply cannot be regarded as a matter of inference only. The recent discovery of ionium by Boltwood serves to link uranium and radium, and explains why it was that those who sought for radium as the immediate offspring of uranium found the latter apparently unproductive, the actual relation of uranium to radium being that of grandparent. But even were we without this connected knowledge, the fact of the invariable occurrence in Nature of these elements, not only in association but in a quantitative relationship, can only be explained on a genetic connection between the two. This evidence, mainly due to the work of Boltwood, when examined in detail, becomes overwhelmingly convincing.

Thus it is to uranium that we look for the continuance of the supplies of radium. In it we find an all but eternal source. The fraction of this substance which decays each year, or, rather, is transformed to a lower atomic weight, is measured in tens of thousands of millionths; so that the uranium of the earth one hundred million years ago was hardly more than 1 per cent. greater in mass than it is to-day.

As radio-active investigations became more refined and extended, it was discovered that radium was widely diffused over the earth. The emanation of it was obtained from the atmosphere, from the soil, from caves. It was

[1] See letters appearing in NATURE of July 9 and September 24, 1903, from the late Mr. W. E. Wilson and Sir George Darwin referring to radium as a solar constituent, and one from the writer (October 1, 1903) on its influence as a terrestrial constituent.

extracted from well waters. Radium was found in brick-earths, and everywhere in rocks containing the least trace of demonstrable uranium, and Rutherford calculated that a quantity of radium so minute as $4\cdot6\times10^{-14}$ grams per gram of the earth's mass would compensate for all the heat now passing out through its surface as determined by the average temperature gradients. In 1906 the Hon. R. J. Strutt, to whom geology owes so much, not only here but in other lines of advance, was able to announce, from a systematic examination of rocks and minerals from various parts of the world, that the average quantity of radium per gram was many times in excess of what Rutherford estimated as adequate to account for terrestrial heat-loss. The only inference possible was that the surface radium was not an indication of what was distributed throughout the mass of the earth, and, as you all know, Strutt suggested a world deriving its internal temperature from a radium jacket some 45 miles in thickness, the interior being free from radium.[1]

My own experimental work, begun in 1904, was laid aside until after Mr. Strutt's paper had appeared, and a valued correspondence with its distinguished author was permitted to me. This address will be concerned with the application of my results to questions of geological dynamics.

Did time permit I would, indeed, like to dwell for a little on the practical aspect of measurements as yet so little used or understood; for the difficulties to be overcome are considerable, and the precautions to be taken many. The quantities dealt with are astoundingly minute, and to extract with completeness a total of a few billionths of a cubic millimetre of the radio-active gas—the emanation—from perhaps half a litre or more of a solution rich in dissolved substances cannot be regarded as an operation exempt from possibility of error; and errors of deficiency are accordingly frequently met with.

Special difficulties, too, arise when dealing with certain classes of rocks. For in some rocks the radium is not uniformly diffused, but is concentrated in radio-active substances. We are in these cases assailed with all the troubles which beset the assayer of gold who is at a loss to determine the average yield of a rock wherein the ore is sporadically distributed. In the case of radium determinations this difficulty may be so much the more intensified as the isolated quantities involved are the more minute and yet the more potent to affect the result of any one experiment. There is here a source of discrepancy in successive experiments upon those rocks in which, from metamorphic or other actions, a segregation of the uranium has taken place. With such rocks the divergences between successive results are often considerable, and only by multiplying the number of experiments can we hope to obtain fair indications of the average radio-activity. It is noteworthy that these variations do not, so far as my observations extend, present themselves when we deal with a recent marine sediment or with certain unaltered deposits wherein there has been no readjustment of the original fine state of subdivision, and even distribution, which attended the precipitation of the uranium in the process of sedimentation.

But the difficulties attending the estimation of radium in rocks and other materials leave still a large balance of certainty—so far as the word is allowable when applied to the ever-widening views of science—upon which to base our deductions. The emanation of radium is most characteristic in behaviour; knowledge of its peculiarities enables us to distinguish its presence in the electroscope not only from the emanation of other radio-active elements, but from any accidental leakage or inductive disturbance of the instrument. The method of measurement is purely comparative. The cardinal facts upon the strength of which we associate radium with geological dynamics, its development of heat and its association with uranium, are founded in the first case directly on observation, and, in the second, on evidence so strong as to be equally convincing. Recent work on the question of the influence of conditions of extreme pressures and temperatures on the radio-active properties of radium appear to show that, as would be anticipated, the effect is small, if indeed existent. As observed by Makower and Rutherford, the small diminution noticed under very extreme conditions in the γ radiation possibly admits of explanation on indirect effects. These observations appear to leave us a free hand as regards radio-thermal effects unless when we pursue speculations into the remoter depths of the earth, and even there while they remain as a reservation, they by no means forbid us to go on.

[1] Proc. R.S., lxxvii., p. 472, and lxxviii., p. 150.

The precise quantity of heat to which radium gives rise, or, rather, which its presence entails, cannot be said to be known to within a small percentage, for the thermal equivalent of the radio-active energy of uranium, actinium, and ionium, and of those members of the radium family which are slow in changing, has not been measured directly. Prof. Rutherford has supplied me, however, with the calculated amount of the aggregate heat energy liberated per second by all these bodies. In the applications to which I shall presently have to refer I take his estimate of $5\cdot6\times10^{-2}$ calories per second as the constant of heat-production attending the presence of one gram of elemental radium.

To these words of introduction I have to add the remark, perhaps obvious, that the full and ultimate analysis of the many geological questions arising out of the presence of radium in the earth's surface materials will require to be founded upon a broader basis than is afforded by even a few hundred experiments. The whole sequence of sediments has to be systematically examined; the various classes of igneous materials, more especially the successive ejecta of volcanoes, fully investigated. The conditions of entry of uranium into the oceanic deposits have to be studied, and observations on sea-water and deep-sea sediments multiplied. All this work is for the future; as yet but little has been accomplished.

The Radium in the Rocks and in the Ocean.

The fact first established by Strutt that the radium distributed through the rock materials of the earth's surface greatly exceeds any permissible estimate of its internal radio-activity has not as yet received any explanation. It might indeed be truly said that the concentration of the heaviest element known to us (uranium) at the surface of the earth is just what we should not have expected. Yet a simple enough explanation may be at hand in the heat-producing capacity of that substance. If it was originally scattered through the earth-stuff, not in a uniform distribution, but to some extent concentrated fortuitously in a manner depending on the origin of terrestrial ingredients, then these radio-active nuclei heating and expanding beyond the capacity of surrounding materials would rise to the surface of a world in which convective actions were still possible and, very conceivably, even after such conditions had ceased to be general; and in this way the surface materials would become richer than the interior. For instance, the extruded mass of the Deccan basalt would fill a sphere 36 miles in radius. Imagine such a sphere located originally somewhere deep beneath the surface of the earth surrounded by materials of like density. The ultimate excess of temperature, due to its uranium, attained at the central parts would amount to about 1000° C., or such lesser temperature as convective effects within the mass would permit. This might take some thirty million years to come about, but before so great an excess of temperature was reached the force of buoyancy developed in virtue of its thermal expansion must inevitably bring the entire mass to the surface. This reasoning would, at any rate, apply to material situated at a considerable distance inwards, and may possibly be connected with vulcanicity and other crustal disturbances observed at the surface.[1] The other view, that the addition of uranium to the earth was mainly an event subsequent to its formation in bulk, so that radio-active substances were added from without and, possibly, from a solar or cosmic source, has not the same *à priori* probability in its favour.[2]

I have in this part of my address briefly to place before you an account of my experiments on the amounts of radium distributed in surface materials. Here, indeed, direct knowledge is attainable; but this knowledge takes us but a very few miles inwards towards the centre of the earth.

[1] See Appendix A.

[2] NATURE, lxxv., p. 294.

The Igneous Rocks.—The basalt of the Deccan, to which I have referred, known to cover some 200,000 square miles to a depth of from 4000 to 6000 feet or more, appears to be radio-active throughout. A fine series of tunnel and surface specimens sent to me by the Director of the Indian Geological Survey has enabled me to examine the radio-activity at various points. It is remarkable that the mean result does not depart much from that afforded by a long series of experiments on North of Ireland basalt and on the basalt of Greenland.

Again, the granites and syenites—and those of Mourne, Aberdeen, Leinster, Plauen, Finsteraarhorn have been examined—while variable, yet approximate to the same mean result.

In the Simplon and St. Gothard tunnels igneous rocks have been penetrated at considerable depth beneath the surface. The greatest true depth is attained, I think, in the central St. Gothard massif. It is remarkable, and may be significant, that in these rocks I have reached the lowest radio-activities I have met—down to almost one-billionth of a gram of radium per gram; although the general mean of the St. Gothard igneous rocks, owing to the high radio-activity of the Finsteraar granite at the north end of the tunnel, is not exceptionally low. Radio-active minerals seem common in the Simplon rocks, involving considerable variations in successive experiments. Some of the highest results are omitted in the mean given below, but as it is difficult to know what to allow for purely sporadic radium the mean is not very certain. In the case of a specially high result I asked Prof. Emil Werner to determine the uranium: my result was confirmed. My list of mean results on igneous rocks up to the present is the following:—

Basalts (14)	5·0[1]	Lewisian Gneiss (3).	5·7
Granites (6)	4·1	Simplon (32)	7·6
Syenites (1)	6·8	St. Gothard (32) ...	5·1

The general mean is 6·1.

From the igneous rocks have originated the sediments after a toll of dissolved substances has been paid to the ocean. It does not of course follow necessarily that the percentage of radium, or more correctly of uranium, in the sedimentary rocks should be less than in the igneous. The residual materials might keep the original percentage of the parent rock, or even improve upon it. There are reasons for believing, however, that there would be a diminution.

Those sedimentary rocks which have been derived from materials formerly in solution offer a different problem. In their case there is little or none of the original materials carried into the secondary rock, and the radio-activity will depend mainly upon how far uranium is precipitated or abstracted with the rock-making substances. In other words, upon how far the waters of the ocean will restore to the rocks what it has borrowed from them.

This brings me to consider the condition of the ocean as preparatory to quoting experiments on the sediments.

The Ocean and its Sediments.—The waters of the ocean, covering five-sevenths of the earth's surface to a mean depth of 3·8 kilometres, represent the most abundant surface material open to our investigation. As the mean of a very large number of experiments upon twenty-two different samples of sea-water from various widely separated parts of the ocean, I obtain a mean of $0{\cdot}016 \times 10^{-12}$ gram per cubic centimetre. There is considerable variability. Taking the mass of the ocean as $1{\cdot}458 \times 10^{18}$ tonnes, there must be about 20×10^{9} grams (20,000 tons) of radium in its waters.

The experiments which I have been able to make on deep-sea deposits, thanks mainly to the kind cooperation of Sir John Murray, apply to ten different materials of typical character.

The results are so consistent as to lead me to believe that although so few in number they cannot be far wrong in their general teaching.

[1] This number is to be multiplied by 10^{-12}, and represents billionths of a gram of radium per gram of material investigated. Throughout the rest of my address this understanding holds, unless where a different meaning is specified. The numbers in parentheses signify the number of different specimens investigated.

The means are:—

	Radium	Extension: Millions of square miles
Globigerina Ooze	7·2	49·5
Radiolarian „	36·7	2·5
Red Clay	33·3	51·5

Diatom Oozes have not yet been examined.

It is apparent from these results that the more slowly collecting sediments are those of highest radio-activity, as if the organic materials raining downwards from the surface of the ocean carried everywhere to the depths uranium and radium abstracted from the waters, but in those regions where the conditions were inimical to the preservation of the associated calcareous tests there was the less dilution of the radio-active substances accumulating beneath. The next table shows that radio-activity and the percentage of calcareous matter in these deposits stand in an inverse relation:—

		Calcium carbonate per cent.	Radium
Globigerina Ooze, *Challenger*	338	92·24	6·7
„ „ „	295	64·34	7·4
Red Clay ... ... „	5	12·00	15·4
„ ... ... „	276	28·28	52·6
Radiolarian Ooze „	272	10·19	22·8
„ „ „	274	3·89	50·3

The percentages of calcium carbonate are from the Report of the *Challenger* Expedition. The Red Clay in the table, which reads as an apparent exception, is probably a case of recent change in the character of the deposit, for the evidence of manganese nodules and sharks' teeth brought up with this clay is conclusive as to the slow rate of its collection. Readers of Sir John Murray's and Prof. Renard's report will remember many cases where recent change in the character of a deposit is to be inferred.

A point of much importance in connection with our views on oceanic radio-activity is that of the presence in the waters and in the deposits of the parent radio-active substance, uranium. The evidence that the full equivalent amount of uranium is present is, I believe, conclusive.

In the first place, to so vast a reservoir as the ocean the rivers cannot be supposed to supply the radium sufficiently fast to make good the decay. In a very few thousand years, in the absence of uranium, the rivers must necessarily renew almost the entire amount of radium present. I have made examination of the water of one great river only—the Nile. The quantity of radium detected was $0{\cdot}0042 \times 10^{-12}$ per cubic centimetre. That is less than the oceanic amount. In short, it is evident that the uranium must accumulate year by year in the oceanic reservoir, like other substances brought in by the rivers, and that the present state of the waters is the result of such actions prolonged over geological time.

While this reasoning is conclusive as regards the waters of the ocean, it does not assure us that the sediments accumulating in their depths are throughout as radio-active as their surface parts would indicate. There might be a precipitation of radium unattended by uranium, in which case their deeper parts would not be radio-active.

Against this possibility there is the evidence of such true deep-sea deposits as were formed in past times and to-day still preserve their radio-activity. For instance, the chalk, which, considering that it was undoubtedly a very rapidly formed deposit, exhibits a radio-activity quite comparable with that of the Globigerina Oozes, deposits which it most nearly resembles. In this deposit, clearly, the uranium must have collected along with the calcareous materials. We can with security argue that the similar oozes collected to-day must likewise contain uranium. In the case of the Red Clays we have the direct determination of the uranium which Prof. Emil Werner was so good as to make at my request. Considering the difficulties attending its separation, the result must be taken as supporting the view that here, too, the radium is renewed from the uranium. Regarding the efforts of other observers to detect uranium in such deposits, it is noteworthy that without the guidance of the radium, enabling specially rich materials to be selected for analysis, the success of the investigation must have been doubtful. The material used was a Red Clay with the relatively large quantity of 54·4 billionths of a gram per gram. In a few grams of this Werner obtained up to seven-twelfths of the total theoretic amount, and of course the separation of the uranium is not likely to have been complete.

It might be thought a hopeless task to offer any estimate of the total bulk of the sub-oceanic deposits, and from this to arrive at some idea of the quantity of radium therein contained. Nevertheless, such an estimate is not only possible, but is based on deductions which possess considerable security. As a major limit I believe the estimate of the total mass of deposit is unassailable, and such deductions as might be applied will still leave it an approximation to the truth.

The elements of the problem are simple enough; we know that the sedimentary rocks have been derived from the igneous, some 30 per cent. of the latter entering into solution in the process of conversion. Some of the soluble constituents, owing to their great solubility, have remained in solution since they entered the ocean.[1] These are the salts of sodium. An estimate of the amount of these salts in the ocean gives us a clue to the total amount of rock substance which has contributed to oceanic salts and oceanic deposits since the inception of the oceans. Some years ago I deduced on this basis that the igneous rocks which are parent to the sodium in the sea must have amounted to about 91×10^{16} tons.[2] This figure in no way involves the rate of supply by the rivers, or our estimate of geological time. It only involves the quantity of sodium now in the ocean—a fairly well-known factor—and the loss of this element, which occurs when average igneous rocks are degraded into sedimentary rocks—a factor also fairly well known. Mr. F. W. Clark, to whom geological science is indebted for so much exact investigation, has recently repeated this calculation, using data deduced anew by himself, and arrives at the result that the bulk of the parent igneous rock was $84{\cdot}3 \times 10^{6}$ cubic miles.[3] On a specific gravity of 2·6 my estimate in tons gives nearly the same result: 84×10^{6} cubic miles.

Now about one-third part of this parent rock goes into solution when breaking up into a detrital sediment. The limestones upon the land are part of what was once so brought into solution. Having made deduction of these former marine deposits (and I here avail myself of Van Hise's and Clark's estimates of the total amount of the sedimentaries and the fraction of these which are calcareous),[4] and, allowing for the quantity remaining in solution in the ocean, the result leaves us with the approximation of twenty million cubic miles of matter once in solution, and now for the greater part existing as precipitated or abstracted deposits at the bottom of the ocean. We are to distribute this quantity over its floor. If the rate of collection had been uniform in every part of the ocean throughout geological time, a depth of about one-seventh of a mile (240 metres) of deposit would cover the ocean bed.

While, I believe, we can place considerable reliance on this approximation, we are less sure when we attempt an estimate of its mean radio-activity. If we assume for it an average radio-activity similar to that of Globigerina Ooze, we find that the quantity of radium involved must be considerably more than a million tons. Apart from the value which such estimates possess as presenting us with a perspective view of the great phenomena we are dealing with, it will now be seen that it supports the finding of the experiments on sedimentary rocks, and leads us to anticipate a real difference in the radio-activity of the two classes of material.

The Sedimentary Rocks.—The radium content of those of detrital character is indicated in the following sandstones, slates, and shales:—

Shales, sandstones, grits (10)	4·4
Slates (Cambrian, Devonian)	4·7
Mud from Amazon	3·2

Some of the above are from deep borings in Carboniferous rocks (the Balfour and Burnlip bores),[1] and from their nature, where not actually of fresh-water origin, can owe little to oceanic radio-activity. Many of the following belong to the class of precipitates, and therefore owe their uranium wholly or in part to oceanic source:—

Marsupites chalk	4·2
Green sandstone	4·9
Green sand (dredged)	4·5
Limestones and dolomites [Trenton, Carboniferous, Zechstein, Lias, Solenhofen (7)]	4·1
Keuper gypsum	6·9
Coral rock, Funafuti bore (4)[2]	1·7
Trias-Jura sediments, Simplon: 17 rocks of various characters	6·9
Mesozoic sediments, St. Gothard: 19 rocks of various characters	4·2

The general mean on sixty-two rocks is 4·7.

Making some allowance for uncertainties in dealing with the Simplon rocks, I think the experiments may be taken as pointing to the result:—

Igneous rocks from 5 to 6.
Sedimentary rocks from 4 to 5.

If our estimate of oceanic radium be applied to the account of the sedimentary rocks in a manner which will be understood from what I have already endeavoured to convey, there will be found to exist a fair degree of harmony between the great quantities which we have found to be in the sediments of the ocean and the impoverishment of the sediments which the experiments appear to indicate.

In all these results fresh and unweathered material has been used. The sand of the Arabian desert gave me but 0·4. Similarly low results have been found by others for soils and such materials. These are not to be included when we seek the radio-activity of the rocks.

As regards generally my experiments on the radium-content of the rocks, I cannot say with confidence that there is anything to indicate a definite falling off in radio-activity in the more deeply seated materials I have dealt with. The central St. Gothard and certain parts of the Deccan have given results in favour of such a decrease. On the other hand, as will be seen later, the granite at the north end of the St. Gothard and the primitive gneiss of the Simplon show no diminution. According to the view I have put forward above as to the origin of the surface richness in radium it is, I think, to be expected that, while the richest materials would probably rise most nearly to the surface, there might be considerable variability in the radio-activity of the deeper parts of the upper crust.

Uranium and the Internal Heat of the Earth.

While forced to deny of the earth's interior any such richness in radium as prevails near the surface, the inference that uranium exists yet in small quantities far down in the materials of the globe is highly probable. This view is supported by the presence of radium in meteoric substances and by its very probable presence in the sun—that greatest of meteorites. True, the radio-thermal theory cannot be supposed to account for any great part of solar heat unless we are prepared to believe that a very large percentage of uranium can be present in the sun, and yet yield but feeble spectroscopic evidence of its existence. Taken all together, the case stands thus as regards the earth. We are assured of radium as a widely distributed surface material, and to such depths as we can penetrate. By inference from the presence of radium in meteoric substances and its very probable presence in the sun, from which the whole of terrestrial stuff probably originated, as well as by the inherent likelihood that every element at the surface is in some measure distributed throughout the entire mass, we arrive at the conclusion that radium is indeed a universal terrestrial constituent.

The dependent question then confronts us—Are we living on a world heated throughout by radio-thermal actions? This question—one of the most interesting which has

[1] Trans. Royal Dublin Soc., vol. vii., ser. ii., p. 23 *et seq.*
[2] *Ibid.*, p. 46.
[3] "The Data of Geochemistry," by F. W. Clark, p. 29.
[4] *Ibid.*, p. 31.

[1] For these rocks, and for much other valuable material, I have to thank Mr. D. Tate, of the Scottish Geological Survey.
[2] For these I have to thank the Trustees of the British Museum and Mr. A. S. Woodward, F.R.S.

originated in the discovery that internal atomic changes may prove a source of heat—can only be answered (if it can be answered at all) by the facts of geological science.

I will not stop to discuss the evidence for and against a highly heated interior of the earth. I assume this heated interior as the obvious and natural interpretation of a large class of geological phenomena, and pass on to consider certain limitations to our knowledge which have to be recognised before we are in a position to enter on the somewhat treacherous ground of hypotheses.

In the first place, we appear debarred from assuming that the surface and central interior of the earth are in thermal connection, for it seems certain that, since the remote period when (probable) convective effects became arrested by reason of increasing viscosity, the thermal relations of the surface and interior have become dependent solely on conductivity. From this it follows if the state of matter in the interior is such as Lord Kelvin assumed —that is, that the conductivity and specific heat may be inferred from the qualities of the surface materials—we have remained in thermal isolation from the great bulk of the interior for hundreds of millions of years, and perhaps even for more than a thousand millions of years. Assuming a diffusivity similar to that of surface rocks, and starting with a temperature of 7000° F., Kelvin found that after 1000 million years of cooling there would be no sensible change at a depth from the surface greater than 568 miles. In short, even if this great period—far beyond our estimates of geological time—has elapsed since the *consistentior status*, the cooling surface has as yet borrowed heat from only half the bulk of the earth.

It is possible, on the other hand, that the conductivity increases inwards, as Prof. Perry has contended; and if the central parts are more largely metallic, this increase may be considerable. But we find ourselves here in the regions of the unknown.

With this limitation to our knowledge, the province of geothermal speculation is a somewhat disheartening one. Thus if with Rutherford, who first gave us a quantitative estimate of the kind, we say that such and such a quantity of radium per gram of the earth's mass would serve to account for the $2{\cdot}6\times10^{20}$ calories which, according to the surface gradients, the earth is losing per annum, we cannot be taken as advancing a theory of radio-active heating, but only a significant quantitative estimate. For, in fact, the heat emitted by radium in the interior may never have reached the surface since the convective conditions came to an end.

And here, depending upon the physical limitations to our knowledge of the earth's interior, a possibility has to be faced. That uranium is entirely absent from the interior is, as I have said, in the highest degree unlikely. If it is present, then the central parts of the earth are rising in temperature. This view, that the central interior is rising in temperature, is difficult to dispose of, although we can adduce the evidence of certain surface-phenomena to show that the rise in temperature during geological time must be small or its effects in some manner kept under control. In a word, whether we assume that the whole heat-loss of the earth is now being made good by radio-active heating or not, we find, on any probable value of the conductivity, a central core almost protected from loss by the immense mass of heated material interposed between it and the surface, and within this core very probably a continuous source of heat. It is hard to set aside any of the premisses of this argument.[1]

We naturally ask, Whither does the conclusion lead us? We can take comfort in a possible innocuous outcome. The uranium itself, however slowly its energy is given up, is not everlasting. The decay of the parent substance is continually reducing the amount of heat which each year may be added to the earth's central materials. And the result may be that the accumulated heat will ultimately pass out at the surface by conductivity, during remote future times, and no physical disturbance result.

The second limitation to our hypotheses arises from this transformation and gradual disappearance of the uranium. And this limitation seems as destructive of definite geothermal theories as the first. To understand its significance requires a little consideration. The fraction of uranium decaying each year is vanishingly small, about the ten thousand-millionth part; but if the temperature of the earth is maintained by uranium, and consequently its decay involves the fall in temperature of the whole earth, the quantity of heat escaping at the surface attendant on the minute decrement would be enormous. An analogy may help to make this clear. Consider the familiar case of a boiler maintained at a particular temperature by a furnace within. Let the combustion diminish and the furnace temperature fall a little. The whole mass of the boiler and its contents follow the downward movement of temperature, heat of capacity escaping at the surface. An observer, only noting the outflow of radiated heat and unable to observe the minute drop of temperature, would probably ascribe to the continued action of the furnace heat which, although derived from it in the past, should no longer be regarded as indicating the heating value of the combustion. Magnify the boiler to terrestrial dimensions: the minutest fall in temperature of the entire mass involves immense quantities of heat passing out at the surface, which no longer indicate the sustaining radio-thermal actions within.

It is easy to see the nature of the difficulties in which we thus become involved. In fact, the heat escaping from the earth is not a measure of the radium in the earth, but necessarily includes, and for a great part may possibly be referred to, the falling temperature, which the decay of the uranium involves. If we take λ (the fraction of uranium transforming each year) as approximately 10^{-10} and assume for the general mass of the earth a temperature of 1500°, a specific heat of 0·2, and, taking 6×10^{27} as its mass in grams, we have, on multiplying these values together, a loss in calories per annum of $1{\cdot}8\times10^{20}$. This by hypothesis escapes at the surface. But the surface loss, as based on earth-gradients of temperature, is but $2{\cdot}6\times10^{20}$ calories. We are left with $0{\cdot}8\times10^{20}$ calories as a measure of the radium present. On this allowance our theories, in whatever form, must be shaped. Nor does it appear as if relief from this restriction can be obtained in any other way than by denying to the interior parts of the earth the requisite high thermal conductivity. Taking refuge in this, we are, however, at once confronted with the possibility of internal stores of radium of which we know nothing, save that they cannot, probably, be very great in amount. In short, I believe it will be admitted on full examination of this question that, while we very probably are isolated thermally from a considerable part of the earth's interior, the decay of the uranium must introduce a large subtractive correction upon our estimates of the limiting amounts of radium which might be present in the earth.

But, finally, is there in all these difficulties sufficient to lead us to reject the view that the present loss of earth-heat may be nearly or quite supplied by radium, and the future cooling of the earth controlled mainly by decay of the uranium? I do not think there are any good grounds for rejecting this view. Observe, it is the condition towards which every planetary body and every solar body containing stores of uranium must tend; and apparently must attain when the rate of loss of initial stores of heat, diminishing as the body grows colder, finally arrives at equilibrium with the radio-thermal supplies. This final state appears inevitable in every case unless the radio-active materials are so subordinate that they entirely perish before the original store of heat is exhausted.

Now, judging from the surface richness in radium of the earth and the present loss of terrestrial heat, it does not seem reasonable to assign a subordinate influence to radio-thermal actions; and it appears not improbable that the earth has attained, or nearly attained, this final stage of cooling.

How, then, may we suppose the existing thermal state maintained? A uniformly radio-active surface layer possessing a basal temperature in accordance with the requirements of geology is, I believe, not realisable on any probable estimate of the allowable radium, or on any concentration of it which my own experiments on igneous rocks would justify.

But we may take refuge in a less definite statement,

[1] Prof. H. A. Wilson has made a suggestive estimate of the thermal effects of radium enclosed in the central parts of the earth (NATURE, February 20, 1908).

and assume a distribution by means of which the existing thermal state of the crust may be maintained. A specially rich surface layer we must recognise, but this need be no more than a very few miles deep; after which the balance of the radium may be supposed distributed to any depth with which we are thermally connected. Below that our knowledge is indefinite. The heat outflow at the surface is in part from the surface radium, in part due to the cooling arising from the diminishing amount of uranium, in part from the deep-seated radium. In this manner the isogeotherms are kept in their places, and a state is maintained which is in equilibrium with the thermal factors involved, but which cannot be considered steady, using the word in a strictly accurate sense, in view of the decay of the uranium.

While the existing thermal state may, I think, thus be maintained by radio-active heating and radio-active decay, we find ourselves in considerable difficulties if we extend this view into the past and assume that the same could be said of any previous stage of the earth's history. If the heat emitted by the earth, when the surface was at melting temperature, was in a state of equilibrium with the radio-active supplies, then, at that date, there must have been many thousands of times the present amount of uranium on the earth, and the period of the *consistentior status* must be put back by thousands of millions of years. Apart from hopeless contradiction with every geological indication as to the age of the earth, difficulties in solar physics arise. For the sun must be supposed of equal duration, and we are required to assume impossible amounts of uranium to maintain his heat all that great lapse of time; and again this uranium would perish at just the same rate as that upon the earth, so that at the present time the solar mass must be, for by far the greater part, composed of inert materials of high atomic weight: the products of the transformations of the uranium family. The difficulty is best appreciated when we consider that even to maintain his present rate of heat-loss by radium supplies, some 60 per cent. of his mass must be composed of uranium. But there are other troubles to face if we adopt this view. The earth, or rather those parts of it which are sufficiently near the surface to lose heat at the requisite rate, would have cooled but one per cent. in 10^8 years. Shrinkage of the outer parts and crustal thickness will be proportionately small, and we must put back our epochs of mountain building to suit so slow a rate of cooling and shrinkage and refer the earlier events of the kind to a past of inconceivable remoteness. Otherwise we must abandon the only tenable theory of mountain formation with which we are acquainted. On such a time-scale the ocean would be supersaturated under the influence of the prolonged denudation like the waters of certain salt lakes, and the sediments would have accumulated a hundredfold in thickness.

Nor do the facts as we know them require from us such sacrifices. We are not asked to raise these difficulties on supposititious quantities of uranium for the existence of which there is no evidence. Radium has occasioned no questioning of the older view that the cooling of the earth from a *consistentior status* has been mainly controlled by radiation. But, on the contrary, this new revelation of science has come to smooth over what difficulties attended the reconciliation of physical and geological evidence on the Kelvin hypothesis. It shows us how the advent of the present thermal state might be delayed and geological time lengthened, so that Kelvin's forty or fifty million years might be reconciled with the hundred million years which some of us hold to be the reading of the records of denudation.

On this more pacific view of the mission of radium to geology, what has been the history of the earth? In the earlier days of the earth's cooling the radiation loss was far in excess of the radio-thermal heating. From this state by a continual convergence, the rate of radiation loss diminishing while the radio-thermal output remained comparatively constant, the existing distribution of temperature near the surface has been attained when the radio-thermal supply may nearly or quite balance the loss by radiation. The question of the possibility of final and perfect equilibrium between the two seems to involve the interior conductivity and in this way to evade analysis.

It will be asked if the facts of mountain building and earth-shrinkage are rendered less reconcilable by this interference of uranium in the earth's physical history. I believe the answer will be in the negative. True, the greatest development of crustal wrinkling must have occurred in earlier times. This must be so, in some degree, on any hypothesis. The total shrinkage is, however, not the less because delayed by radio-thermal actions, and it is not hard to point to factors which will attend the more recent upraising of mountain chains tending to make them excel in magnitude those arising from the stresses in an earlier and thinner crust.

Underground Temperature.

It would be a matter of the highest interest if we could definitely connect the rise of temperature which is observed in deep borings and tunnels with the radio-activity of the rocks. We are confronted, however, by the difficulty that our deepest borings and tunnels are still too near the surface to enable us to pronounce with certainty on the influence of the radium met with in the rocks. This will be understood when it is remembered that a merely local increase of radio-activity must have but little effect upon the temperature unless the increase be of a very high order indeed. A clear understanding of this point shows us at once how improbable it is that volcanic temperatures can be brought within a very few miles of the surface by local radio-activity of the rocks. To account on such principles for an elevation of temperature of, say, 1200° at a depth of three or four miles from the surface, a richness in radium must be assumed far transcending anything yet met with in considerable rock masses; and as volcanic materials appear to show nothing of such exceptional richness in radium we can hardly suppose local radio-activity of the upper crust responsible for volcanic phenomena.

When we come to apply calculation to results on the radio-activity of the materials penetrated by tunnels and borings, we at once find that we require to know the extension downwards of the rocks we are dealing with before we can be sure that radium will account for the thermal phenomena observed. At any level between the surface and the base of a layer of radio-active materials—suppose the level considered is that of a tunnel—the temperature depends, so far as it is due to local radium, on the total depth of the rock-mass having the observed radio-activity. This is evident. It will be found that for ordinary values of the radium content it is requisite to suppose the rocks extending downwards some few kilometres in order to account for a few degrees in temperature at the level under observation. There is, of course, every probability of such a downward extension. Thus in the case of the Simplon massif the downward continuance of the gneissic rocks to some few kilometres evokes no difficulties. The same may be said of the granite of the Finsteraarhorn massif and the gneisses of the St. Gothard massif, materials both of which are penetrated by the St. Gothard tunnel, and which appear to possess a considerable difference in radio-activity. In dealing with this subject, comparison of the results obtained at one locality with those obtained at another is the safest procedure. We must accordingly wait for an increased number of results before much can be inferred. I will now lay the cases of the two great tunnels as briefly as possible before you.

And first as to the temperature effects observed in the two cases.

The Simplon tunnel for a length of some seven or eight kilometres lies at a mean distance of about 1700 metres from the surface. At the northerly end of this stretch the rock temperature attains 55°, and at the southern extremity has fallen to about 35°. The temperature of 55° is the highest encountered. The maximum predicted by Stapff, basing his estimates on his experience of the St. Gothard tunnel, was 47°. Other authorities in every case predicted considerably lower temperatures. Stockalper, who also had experience of the St. Gothard, predicted 36° at a depth of 2050 metres from the surface, and Heim 38° to 39°.[1]

[1] See the account given by Schardt, *Verhandl. Schweizerischen Naturf. Gesellsch.*, 1904, lxxxvii., "Jahresversammlung," p. 204 *et seq.*

When the unexpectedly high temperatures were met with, various reasons were assigned. Mr. Fox has suggested volcanic heat. Others point to the arrangement of the schistosity and the dryness of the rocks, where the highest temperatures were read. The latter is evidently to be regarded more as explanation of the lower temperatures at the south end of the tunnel, where the water circulation was considerable, than of the high temperatures of the northern end. The schistosity may have some influence in bringing the isogeotherms nearer to the surface; however, not only are the rocks intensely compact in every direction, but what schistosity there is by no means inclines in the best directions for retention of heat. From the sections the schistosity appears generally to point upwards at a steep angle with the tunnel axis.[1]

Where there is such variability in the temperatures, irrespective of the depth of overlying rock, there is difficulty in assigning any significant mean gradient. The highest readings are obviously those least affected by the remarkable water-circulation of the Italian side. The higher temperatures afford such gradients as would be met in borings made on the level—about 31 metres per degree.

The temperatures read in the St. Gothard rocks were of a most remarkable character. For the central parts of the tunnel the gradients come out as 46·6 metres per degree. Stapff, who made these observations and conducted the geological investigations, took particular pains to ascertain the true surface temperatures of the rock above the tunnel; and from these ascertained temperatures, the temperatures in the tunnel rock and the overlying height of mountain, he calculated the gradients.

But this low gradient is by no means the mean gradient. At the north end, where the tunnel passes through the granite of the Finsteraarhorn massif, there is a rise in the temperature of the rock sufficient to steepen the gradient to 20·9 metres per degree. Stapff regarded this local rise of temperature as unaccountable save on the view that the granite retained part of the original heat. This matter I will presently return to.

Now, it is a fact that the radium-content of the Simplon rocks, after some allowance for what I have referred to as sporadic radium, stands higher than is afforded by the rocks in the central section of the St. Gothard, where the gradient is low. For the Simplon the general mean is (on my experiments) 7·1 billionths of a gram per gram. This mean is well distributed as follows:—

Jurassic and Triassic altered sediments	6·4
Crystalline schists, partly Jurassic and Triassic, partly Archæan	7·3
Monte Leone gneiss and primitive gneiss	6·3
Schistose gneiss (a fold from beneath)...	6·5
Antigorio gneiss	6·8

The divisional arrangement is Prof. Schardt's. Forty-nine typical rocks are used in obtaining these results, and the experiments have been in many cases repeated on duplicate specimens. Including some very exceptional results, the mean would rise to $9 \cdot 1 \times 10^{-12}$ grams per gram.

Of the St. Gothard rocks I have examined fifty-one specimens selected to be, as far as attainable, representative.[2]

Of these, twenty-one are from the central region, and their mean radium content is just 3·3. The portion of the tunnel from which these rocks come is closely coincident with Stapff's thermal subdivision of regions of low temperature.[3] This portion of the mountain offers the most definite conditions for comparison with the Simplon results. The region south of this is affected by water circulation; the regions to the north are affected by the high temperature of the granite.

We see, then, that the most definite data at our disposal in comparing the conditions as regards temperature and radio-thermal actions in the two tunnels appear to show that the steeper gradient is associated with the greater radium-content.

It is possible to arrive at an estimate of the downward extension of the two rock masses (assumed to maintain to the same depth their observed radio-activity), which would account for the difference in gradient. In making this estimate, we do not assume that the entire heat-flow indicated by the gradients is due to radium, but that the difference in radium-content is responsible for the difference of heat-flow. If some of the heat is conducted from an interior source (of whatever origin), we assume that this is alike in both cases. We also assume the conductivities alike.

Calculating on this basis, the depth required to establish on the radium measurements the observed difference in gradients of the Central St. Gothard and of the Simplon, we find the depth to be about 7 kilometres on the low mean of the Simplon rocks, and 5 kilometres on the high mean. There is, as I have already said, nothing improbable in such a downward extension of primitive rocks having the radio-activities observed; but as a different distribution of radium may, of course, obtain below our point of observation, the result can only claim to be suggestive.

Turning specially to the St. Gothard, we find that a temperature problem of much interest arises from the facts recorded. The north end of the tunnel for a distance of 2 kilometres traverses the granite of the Finsteraarhorn massif. It then enters the infolded syncline of the Usernmulde and traverses altered sediments of Trias-Jura age for a distance of about 2 kilometres. After this it enters the crushed and metamorphosed rocks of the St. Gothard massif, and remains in these rocks for 7½ kilometres. The last section is run through the Tessinmulde for 3 kilometres. These rocks are highly altered Mesozoic sediments.

I have already quoted Stapff's observations as to the variations of gradient in the northern, central, and southern parts of the tunnel. He writes: "They (the isotherms) show irregularities on the south side, which clearly depend on cold springs, they bend down rapidly, and then run smoothly inclined beneath the water-filled section of the mountain. Other local irregularities can be explained by the decomposition of the rock; but there is no obvious explanation of the rapid increase in the granite rocks at the northern end of the tunnel (2000 metres), and it is probably to be attributed to the influence of different thermal qualities of the rock on the coefficient of increase. For the rest these 2000 metres of granite belong to the massif of the Finsteraarhorn, and, geologically speaking, they do not share in the composition of the St. Gothard. Perhaps these two massifs belong to different geological periods (as supposed for geological reasons long ago). What wonder, then, if one of them be cooler than the other." (*Loc. cit.*, p. 30.)

Commenting on the explanation here offered by Stapff, Prestwich[1] states his preference for the view that the excess of temperature in the granite is due to mechanical actions to which the granite was exposed during the upheaval of this region of the Alps.

The accompanying diagram shows the distribution of temperature as given by Stapff, and the distribution of radium as found from typical specimens of the rocks. There is a correspondence between the two which is obvious, and when it is remembered that the increase in radio-activity shown at the south end would have been, according to Stapff, masked by water circulation, the correspondence becomes the more striking. The small radium values in the central parts of the tunnel are remarkable. The rocks of the Central St. Gothard massif are apparently exceptionally poor in radium.

At the north end the excess of radium is almost confined to the granite, the rock to which Stapff ascribed the exceptional temperatures. The radium of the Usernmulde is probably not very important, seeing that these sediments cannot extend far downwards. The principal local source of heat appears located more especially beneath the synclinal fold, for Stapff's table (*loc. cit.*, p. 31) of the gradients beneath the plain of Andermatt shows a rising gradient to a point about 2500 metres

[1] Schardt, *loc. cit.*

[2] I would like to express here my acknowledgments to the Trustees of the British Museum for granting me permission to use chips of the rocks in their possession; and especially to Mr. Prior for his valuable assistance in selecting the specimens.

[3] Trans. North of England Mining and Mec. Engineers, xxxiii., p. 25.

[1] Proc. R.S., xli., p. 44.

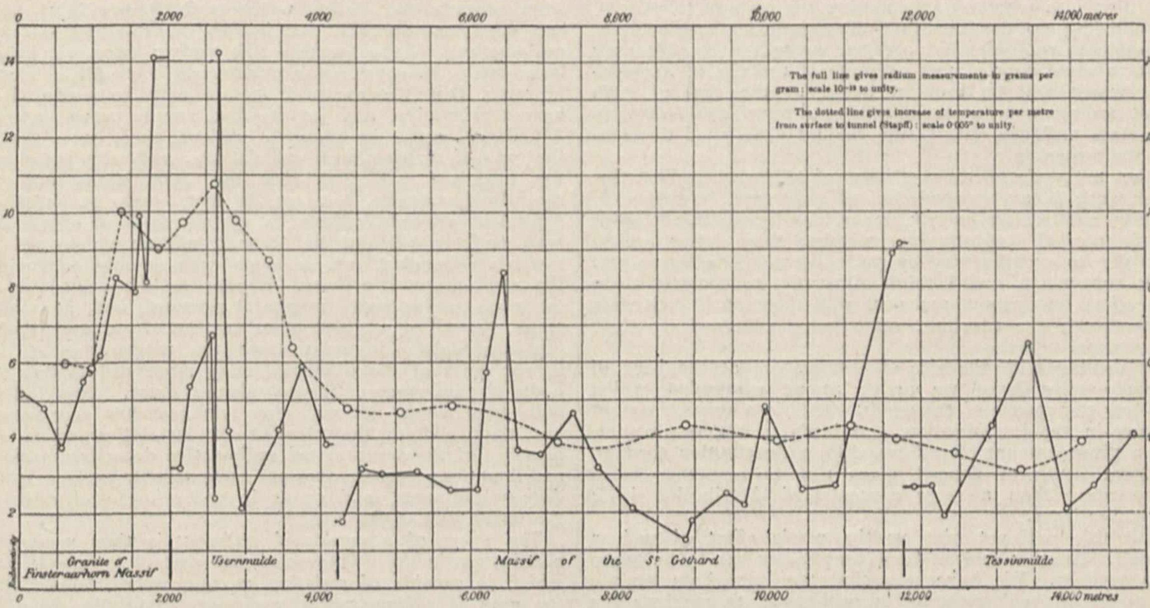
from the north entrance of the tunnel. It is observable that the radio-activity of the granite increases as it approaches the Usernmulde and attains its maximum (14·1) where it dips beneath the syncline.

The means of radium-content in the several geological sections into which the course of the tunnel is divisible are as follows :—

Granite of Finsteraarhorn	7·7
Usernmulde	4·9
St. Gothard massif	3·9
Tessinmulde	3·4

The central section, however, if considered without reference to geological demarcations, would, as already observed, come out as barely 3·3. And this is the value of the radio-activity most nearly applicable to Stapff's thermal subdivision of the region of low temperature.

If we accept the higher readings obtained in the granite as indicative of the radio-active state of this rock beneath the Usernmulde, a satisfactory explanation of the difference of heat-flow from the central and northern parts of the tunnel is obtained. Using the difference of gradient as basis of calculation, as before, we find that a downward extension of about six thousand metres would, if the outflow took place in an approximately vertical direction, account for the facts observed by Stapff. This depth is in agreement with the result as to the downward extension of the St. Gothard rocks as derived from the comparison with the Simplon rocks.



We are by no means in a position to found dogmatic conclusions on such results; they can only be regarded as encouragement to pursue the matter further. The coincidence must be remarkable which thus similarly localises radium and temperature in roughly proportional amounts, and permits us, without undue assumptions, to explain such remarkable differences of gradient. There is much work to be done in this direction, for well-known cases exist where exceptional gradients in deep borings have been encountered—exceptional both as regards excess and deficiency.

Radio-active Deposits and the Instability of the Crust.

At the meeting of the British Association held last year at Leicester, I read a note on the thermal effects which might be expected to arise at the base of a sedimentary accumulation of great thickness due to the contained radium.

The history of mountain building has repeated itself many times: ages of sedimentation, with attendant sinking of the crust in the area of deposition, then upheaval, folding up of the great beds of sediment, and even their over-thrusting for many miles. So that the mountain ranges of the world are not constituted from materials rising from below, save in so far as these may form a sustaining core, but of the slowly accumulating deposits of the ages preceding the upheaval.

The thickness of collected sediments involved in these great events is enormous, and although uncertainty often attends the estimation of the aggregate depths of sedimentation, yet when we consider that unconformities between the deposits of succeeding eras represent the removal of vast masses of sediment to fresh areas of deposition, and often in such a way as to lead to an underestimate of the thickness of deposit, the observations of the geologist may well indicate the minor and not the major limit. Witness the mighty layers of the Huronian, Animikean, and Keweenawan ages where deposits measured in miles of thickness are succeeded by unrecorded intervals of time, in which we know with certainty that the tireless forces of denudation laboured to undo their former work. Each era represents a slow and measured pulse in the earth's crust, as if the overloading and sinking of the surface materials induced the very conditions required for their re-elevation. Such events, even in times when the crust was thinner and more readily disturbed than it is now, must have taken vast periods of time. The unconformity may represent as long a period as that of accumulation. In these Proterozoic areas of America, as elsewhere on the globe and throughout the whole of geological history, there has been a succession in time of foldings of the crust always so located as to uplift the areas of sedimentation, these upheavals being sundered by long intervals during which the site of sedimentation was transferred and preparation made for another era of disturbance. However long deferred there seems to be only the one and inevitable ending, inducing a rhythmic and monotonous repetition surely indicative of some cause of instability attending the events of deposition.

The facts have been impressively stated by Dana: "A mountain range of the common type, like that to which the Appalachians belong, is made out of the sedimentary formations of a long preceding era; beds that were laid down conformably, and in succession, until they had reached the needed thickness; beds spreading over a region tens of thousands of square miles in area. The region over which sedimentary formations were in progress in order to make, finally, the Appalachian range, reached from New York to Alabama, and had a breadth of 100 to 200 miles, and the pile of horizontal beds along the middle was 40,000 feet in depth. The pile for the Wahsatch Mountains was 60,000 feet thick, according to King. The beds for the Appalachians were not laid down in a deep ocean, but in shallow waters, where a gradual subsidence was in progress; and they at last, when ready for the genesis, lay in a trough 40,000 feet deep, filling the trough to the brim. It thus appears that epochs of mountain making have occurred only after long intervals of quiet in the history of a continent."

The generally observed fact that the deposition of sediments in some manner involves their ultimate upheaval has at various times led to explanations being offered. I think I am safe in saying that although the primary factor, the compressive stress in a crust which has ceased to fit the shrinking world within it, has probably been correctly inferred, no satisfactory explanation of the connection between sedimentation and upheaval has been advanced. The mere shifting upwards of the isogeotherms into the deposits, advanced as a source of local loss of rigidity by Babbage and Herschel, need not involve any such loss so long as the original distance of the isogeotherms from the surface is preserved.

We see in every case that only after great thicknesses of sediments have accumulated is the upheaval brought about. This is a feature which must enter as an essential condition into whatever explanation we propose to offer.

Following up the idea that the sought-for instability is referable to radio-thermal actions, we will now endeavour to form some approximate estimate of the rise of temperature which will be brought about at the base of such great sedimentary accumulations as have gone towards mountain building, due to the radium distributed throughout the materials.

The temperature at the base of a feebly radio-active layer, such as an accumulation of sediments, is defined in part by radio-active energy, in part by its position relative to the normal isogeotherms, whether these latter are in turn due to or influenced by radio-thermal supplies or not. It is convenient, and I think allowable, to consider these two effects separately, and deal with them as if they were independent, the resultant state being obtained by their summation.

In dealing with the rise of temperature at the base of a radio-active layer we arrive at an expression which involves the square of the depth. This is a very important feature in the investigation, and leads to the result that, for a given amount of radium, diffuse distribution through a great depth of deposit gives rise to a higher basal temperature than a more concentrated distribution in a shallower layer.

But this will not give us the whole effect of such a deposit. Another and an important factor has to be taken into account. We have seen that the immediate surface rocks are of such richness in radium as to preclude the idea that a similar richness can extend many miles inward. Now, it is upon this surface layer that the sediments are piled, and as they grow in thickness this original layer is depressed deeper and deeper, yielding under the load until at length it is buried to the full depth of the overlying deposit. This slow and measured process is attended by remarkable thermal effects. The law of the increase of temperature with the square of the depth comes in, and we have to consider the temperature effect not merely at the base of the deposited layer, but that due to the depression and covering over of the radium-rich materials upon which the sediments were laid down.

The table which follows embodies an approximate statement of the thermal results of various depths of deposit supposed to collect under conditions of crustal temperature such as prevail in this present epoch of geological history:—

Thickness of sedimentary deposit		Resulting rise of isogeotherms		Weakening of earth's crust as defined by the rise of the geotherm at 40 kilometres
Kilometres		Kilometres		Kilometres
6	...	7·4	...	40 to 32·6
8	...	10·2	...	40 to 29·8
10	...	13·3	...	40 to 26·7
12	...	16·7	...	40 to 23·3
14	...	20·4	...	40 to 19·6

I have deferred to the conclusion of this address an account of the steps followed in obtaining the above results. It is clearly impossible, within the limited time allotted to me, to make these quite clear. It must suffice here merely to explain the significance of the figures.

The first column gives the depth of sedimentary deposit supposed to be laid down on the normal radio-active upper crust of a certain assumed thickness and radio-activity. From the rise of temperature which occurs at the base of this crust (due to the radio-activity, not only of the crust, but of the sediments) the results of the second column are deduced, the gradient or slope of temperature prevailing beneath being derived from the existing surface gradients corrected for the effects of the radio-thermal layer. The third column is intended to exhibit the effect of this shift of the geotherms in reducing the strength of the crust. I assume that at a temperature of 800° the deep-seated materials lose rigidity under long-continued stress. The estimated depth of this geotherm is, on the assumptions, about 40 kilometres. The upward shift of this geotherm shows the loss of strength. Thus in the case of a sedimentary accumulation of 10 kilometres the geotherm defining the base of the rigid crust shifts upwards by 13 kilometres, so that there is a loss of effective section to the amount of 30 per cent.[1]

As regards the claims which such figures have upon our consideration, my assumptions as to thickness and radio-activity of the specially rich surface layer are, doubtless, capable of considerable amendment. It will be found, however, that the assumed factors may be supposed to vary considerably, and yet the final results prove such as, I believe, cannot be ignored. Indeed, those who are in the way of making such calculations, and who enter into the question, will find that my assumptions are not specially favourable, but are, in fact, made on quite independent grounds. Again, a certain class of effects has been entirely left out of account, effects which will go towards enhancing, and in some cases greatly enhancing, the radio-thermal activity. I refer to the thickening of the crust arising from tangential pressure, and, at a later stage, the piling up and overthrusting of mountain building materials. In such cases the temperature of the deeper parts of the thickened mass must still further rise under the influence of the contained radium. These effects only take place, indeed, after yielding has commenced, but they add to the element of instability which the presence of the accumulated radio-active deposits occasions, and doubtless increase thermal metamorphic actions in the deeper sediments, and result in the refusion of rocks in the upper part of the crust.[2]

The effect of accumulated sediment is thus necessarily a reduction in the thickness of that part of the upper crust which is capable of resisting a compressive stress. Over the area of sedimentation, and more especially along the deepest line of synclinal depression, the crust of the globe for a period assumes the properties belonging to an earlier age, yielding up some of the rigidity which was the slow inheritance of secular cooling. Along this area of weakness—from its mode of formation generally much elongated in form—the stressed crust for many hundreds, perhaps thousands, of miles finds relief, and flexure takes place in the only possible direction; that is, on the whole upwards. In this way the prolonged anticline bearing upwards on its crest the whole mass of deposits is formed, and so are born the mountain ranges in all their diversity of form and structure.

We have in these effects an intervention of radium in the dynamics of the earth's crust, which must have influenced the entire history of our globe, and which, I believe, affords a key to the instability of the crust. For after the events of mountain building are accomplished, stability is not attained, but in presence of the forces of denudation the whole sequence of events has to commence over again. Every fresh accession of snow to the firn, every passing cloud contributing its small addition to the

[1] See Appendix B.

[2] Prof. C. Schmidt (Basel) has recently given reasons for the view that the Mesozoic schists of the Simplon at the period of their folding were probably from 15,000 to 20,000 metres beneath the surface ("Ec. Geol. Helvetiæ," vol. ix., No. 4. p 590). As another instance consider the compression of the Laramide range (Dawson, Bull. Geol. Soc. Am., xii., p. 87).

torrent, assists to spread out once more on the floor of the ocean the heat-producing substance. With this rhythmic succession of events appear bound up those positive or negative movements of the strand which cover and uncover the continents, and have swayed the entire course of evolution of terrestrial life.

Oceanic Deposits.—The displacements of the crust which we have been considering are now known to be by no means confined to the oceanic margins. The evidence seems conclusive that long-continued movements have been in progress over certain areas of the sea floor, attended with the formation of those numerous volcanic cones upon which the coral island finds foundation. Here there are plainly revealed signs of instability and yielding of the crust (although, perhaps, of minor intensity) such as are associated with the greater movements which terminate in mountain building. I think it will be found, when the facts are considered, that we have here phenomena continuous with those already dealt with, and although the conditional element of a sufficient sedimentary accumulation must remain speculative, the evidence we possess is in favour of its existence.

One of the most interesting outstanding problems of deep-sea physiography is that of the rates of accumulation of the several sorts of deposit. In the case of the more rapidly collecting sediments there seems no serious reason why the matter should not be dealt with observationally. I hope it may be accomplished in our time. For my present purpose I should like to know what may or may not be assumed in discussing the accumulation of radio-active sediments on the ocean floor.

As regards the rate of collection of the non-calcareous deposits, the nearest approach to an estimate is, I think, to be obtained from the exposed oceanic deposits of Barbados. In the well-known paper of Jukes Brown and Harrison [1] on the geology of that island, it is shown that the siliceous radiolarian earths and red clays aggregate to a thickness of about 300 feet. These materials are true oceanic deposits, devoid of terrigenous substances. They collected very probably during Pliocene and, perhaps, part of Pleistocene times. Now, there is evidence to lead us to date the beginning of the Pliocene as anything from one million to three million years ago. The mean of these estimates gives a rate of collection of 5 millimetres in a century. This sounds a very slow rate of growth, but it is too fast to be assumed for such deposits generally. More recent observations might, indeed, lead us to lengthen the period assigned to the deposition of these oceanic beds; for if, following Prof. Spencer,[2] we ascribe their deposition to Eocene times, a less definite time-interval is indicated; but the rate could hardly have been less than 3 millimetres in a century. The site of the deposit was probably favourable to rapid growth.

We have already found a maximum limit to the average thickness of true oceanic sediments; and such as would obtain over the ocean floor if the rate of collection was everywhere the same and had so continued during the past. If there is one thing certain, however, it is that the rates of accumulation vary enormously. The 1200 or 1500 feet of chalk in the British Cretaceous, collected in one relatively brief period of submergence, would alone establish this. Huxley inferred that the chalk collected at the rate of 1 inch in a year. Sollas showed that the rate was more probably 1 inch in forty years. Sir John Murray has advanced evidence that in parts of the Atlantic the cables become covered with Globigerina ooze at the rate of about 10 inches in a century. Finally, then, we must take it that the fair allowance of one-seventh of a mile may be withheld in some areas and many times exceeded in others.

Now it is remarkable that all the conditions for rapid deposition seem to prevail over those volcanic areas of the Pacific from which ascend to the surface the coral islands—abundant pelagic life and comparatively shallow depths. Indeed, I may remind you that the very favourable nature of the conditions enter into the well-known theory of coral island formation put forward by Murray.

The islands arise from depths of between 1000 and 2000 fathoms. These areas are covered with Globigerina ooze having a radio-activity of about 7 or 8. The deeper-lying deposits around—red clay and radiolarian ooze—show radio-activities up to and more than 50. From these no volcanic islands spring.

These facts, however, so far from being opposed to the view that the radio-activity and crustal disturbance are connected, are in its favour. For while those rich areas testify to the supply of radio-active materials, the slow rate of growth prevailing deprives those deposits of that characteristic *depth* which, if I may put it so, is of more consequence than a high radio-activity. For the rise in temperature at the base of a deposit, as already pointed out, is proportional to the square of the thickness; in reality the dilution of the supplies of uranium which reach the calcareous oozes flooring the disturbed areas is a necessary condition for any effective radio-thermal actions.

It might appear futile to consider the matter any closer where so little is known. But in order to give an idea of the quantities involved I may state that, if my calculations are correct, a rate of deposit comparable with that of the chalk prevailing for ten million years would, on assumptions similar to those already explained when discussing the subject of mountain building, occasion a rise of the deeper isogeotherms by from 20 to 30 per cent. of their probable normal depth.

In making these deductions as to the influence of radium in sedimentary deposits, I have so far left out of consideration the question of the time which must elapse in order that the final temperature-rise in the sediments must be attained. The question we have to answer is: Will the rate of rise of temperature due to radium keep pace with the rate of deposition, or must a certain period elapse after the sedimentation is completed to any particular depth, before the basal temperature proper to the depth is attained?

The answer appears to be, on an approximate method of solution, that for rates of deposition such as we believe to prevail in terrigenous deposits—even so great as 1 foot in a century, and up to depths of accumulation of 10 kilometres and even more—the heating waits on the sedimentation. Or, in other words, there is thermal equilibrium at every stage of growth of the deposit; and the basal temperature due to radio-active heating may at any instant be computed by the conductivity equation. For accumulations of still greater magnitude the final and maximum temperature appears to lag somewhat behind the rate of deposition.

From this we may infer that the great events of geological history have primarily waited upon the rates of denudation and sedimentation. The sites of the terrigenous deposits and the marginal oceanic precipitates have many times been convulsed during geological time because the rates of accumulation thereon have been rapid. The comparative tranquillity of the ocean floor far removed from the land may be referred to the absence of the inciting cause of disturbance. If, however, favourable conditions prevail for such a period that the local accumulations attain the sufficient depth, here, too, the stability must break down and the permanency be interrupted.

Upheaval of the ocean floor, owing to the laws of deep-sea sedimentation, should be attended with effects accelerative of deposition—a fact which may not be without influence. But although ultimately sharing the instability of the continental margins, the cycle of change is tuned to a slower periodicity. From the operation of these causes, possibly, have come and gone those continents which many believe to have once replaced the wastes of the oceans, and which with all their wealth of life and scenic beauty have disappeared so completely that they scarce have left a wreck behind. But those forgotten worlds may be again restored. The rolled-up crust of the earth is still rich in energy borrowed from earlier times, and the slow but mighty influences of denudation and deposition are for ever at work. And so, perchance, in some remote age the vanished Gondwána Land, the lost Atlantis, may once again arise, the seeds of resurrection even now being sown upon their graves from the endless harvests of pelagic life.

[1] Q.J.G.S., xlviii., p. 170.
[2] *Ibid.*, lviii., p. 354 *et seq.*

APPENDIX A.

Convective Movement of Uranium to the Earth's Surface.—The estimate of temperature given assumes (1) that the mass of igneous material is spherical, and (2) that its surface is kept at constant temperature, heat escaping freely. The first assumption is in favour of increasing the estimate of temperature, and probably would not generally be true, especially of a mass moving upwards. The second assumption tends to give a lower estimate of temperature, and is certainly misleading, as the surrounding materials are non-conducting, and must favour the accumulation of radio-active heat.

On assumptions (1) and (2) and on Barus' results for the thermal expansion of diabase between 1100° and 1500°,[1] and results of my own on basalt,[2] which are in approximate agreement, and assuming the mean excess of temperature to be 500° and the surrounding material to be at a fluid temperature, the force of buoyancy comes out at more than 60 dynes per cubic centimetre of the spherical mass. This is an under-estimate.

If we may assume that the Deccan Trap is indeed an instance of such an over-heated mass escaping at the surface, and that similar radio-active masses rising up from beneath at various times in the past may have affected the crust, we have at our disposal a local source of energy of plutonic origin which may account for much.

APPENDIX B.

Sedimentation and Rise of Geotherms.—The depth of the upper radio-active layer is, of course, unknown. We possess, however, the means of arriving at some idea of what it must be. The quantitative thermal conditions impose a major limit to its average thickness, and the indications of injected rocks suggest a minor limit.

It will be found that if $2{\cdot}6\times10^{20}$ calories is the heat output of the whole earth per annum, and if we assign only one-fifth of this amount to cooling due to decay of the uranium, then, on the assumption that the earth is no longer losing any part of its original store of heat, we have about 2×10^{20} representing radium heating. From this the allowance of terrestrial radium per square centimetre inwards is $2{\cdot}3\times10^{-8}$ grams. This would give a major limit. But it is almost certain that some of this radium is located in more deeply seated parts of the earth. If we take 10^{-8} as contained in the normal radio-active surface layer, and assume (what according to my experiments should not be far from the truth) that the average radio-activity is 3, we arrive at a thickness of 12 kilometres.

Some such mean value is necessitated by the evidence we derive from the radio-activity of igneous rocks. These rocks must in many cases be derived from considerable depths. Such outflows as the Deccan may indicate local sub-crustal conditions; so also may the eruptions of certain volcanic areas. But those extrusions which have attended mountain building, more especially its closing phases, appear to indicate general conditions, and involve the existence of such radio-active materials at considerable depths. If we assume a thickness for the radio-active part of the crust much less than the 12 kilometres, difficulties are met with on this line of reasoning.[3]

Proceeding now to the derivation of the results given in the table, p. 464. The equation $k\theta = qhx(D - x/2)$ (where θ is the temperature at the depth x, D being the total depth of the radio-active layer, q the radium per c.c. in grams, h the heat output of one gram of radium per second, k the thermal conductivity) is easily derived by considering the conditions of thermal flow in the layer, supposed to lose heat only at the surface.[4]

The aggregate depths of radio-active material in the several cases of sedimentary deposit assumed in my address amount to 18, 20, 22, 24, and 26 kilometres. I assume the mean radio-activity to be 3·5, and the average conductivity to be 4×10^{-3}. From this the basal temperatures are found, as due to radio-thermal actions. These temperatures are to be augmented by the temperatures proper to the several depths, which depend upon the conducted interior heat. To estimate these we require to apportion the observed average surface gradient (taken as 32 metres per degree) between radio-active effects in the upper layer and the flow of heat from within. The radio-thermal gradient comes out at about 75 metres; the inner gradient is accordingly 56 metres. Hence the total temperature at the base of each radio-active mass is obtained. But the geotherms proper to the several depths, 18, 20, &c., kilometres, under conditions prevailing elsewhere in the crust, are easily found from the value of θ for the normal layer (82° C.), and adding the temperature due to interior heat. From the difference of the temperatures we, finally, find the rise of the geotherms.

As conveyed in my address, I have found on several different values of the thickness and radio-active properties of the surface layer, results in every case showing large values for the rise of the geotherms. The data assumed above are by no means the most favourable.

[1] Phil. Mag., xxxv., p. 173.

[2] Trans. R.D.S., vi., p. 208.

[3] See p. 464, *ante*, and foot-note as bearing on the possible displacement of the geotherms.

[4] See Strutt, Proc. R S., lxxvii., p 482.

NOTES.

It is with deep regret that we learn of the sudden death of Prof. Alexis Hansky, whose work in solar physics at the Pulkowa Observatory has attracted so much attention. According to a letter from M. Tikhoff, which appears in the September number of the *Bulletin de la Société astronomique de France*, M. Hansky was drowned whilst bathing in the Black Sea at Simeise, in the Crimea, on August 11 (July 29 O.S.). The deceased astronomer commenced his practical work in solar physics by observing the total eclipse of 1896 at Novaya Zemlya, and at the time of his tragic death was engaged in the installation of a new observatory in the Crimea which had been given to him, and which he had handed over to the Pulkowa Observatory. By his death at the early age of thirty-six years, the study of solar physics has suffered a loss which it will be exceedingly difficult to repair.

The President of the Local Government Board has arranged for the making of the two following researches:—a chemical and bacteriological investigation, by Mr. C. G. Moor and Prof. R. T. Hewlett, as to the influence of softening and of other chemical processes on the purity of water supplies from the chalk as shown in actual experience and under experimental conditions, and an investigation by Prof. Sidney Martin, F.R.S., into the powers of production of disease possessed by certain streptococci and by the poisonous substances produced by them, in continuance of previous investigations by him on the same subject. These investigations complete the allocation of the scientific grant for the year 1908–9.

Prof. Robert Koch has been chosen to represent the German Government at the forthcoming International Tuberculosis Congress at Washington.

The annual conference of the Sanitary Inspectors' Association opened on Tuesday last under the presidency of Sir James Crichton-Browne, F.R.S.

The third International Philosophy Congress has been in session at Heidelberg during a portion of the past week. The next meeting will take place in 1912, at Bologna.

The arrangements for the fourth International Fisheries Congress, which, as has already been announced, is to be held at Washington from September 22–26, are now complete. An attractive itinerary has been arranged for the week following the sessions of the congress, and includes visits to New York City, Narragansett Bay, Wood's Hole, Boston, and Gloucester, at each of which places local committees and individual residents will provide demonstrations of fishery methods and incidental entertainment. The methods of oyster culture employed on the great New England beds, the pound-net fishery, the purse-seine fishery, inspection of fish markets and vessels, the methods of deep-sea research, and other matters relating to the fisheries will be shown. Special itineraries will be arranged for members who may desire to visit other fisheries and hatcheries, and letters of introduction will be furnished.

AN International Rubber Exhibition (lasting a fortnight) is to be held at Olympia, London, from September 14. The exhibits will consist wholly of objects of interest to members of the rubber and allied trades, and will comprise illustrations of the growth of the commodity and examples of the machinery employed in its manipulation. Rubber trees in all stages of their growth will be shown, together with the raw material obtained from them, and the varied forms into which it is manufactured. Demonstrations will be given in a laboratory, and growers, manufacturers, and others will have an opportunity of discussing questions relating to the industry at an international congress, to which delegates have been sent by many Continental countries. Borneo, Mexico, and other rubber-producing countries are taking part in the enterprise.

AT the third International Congress of the History of Religions, which is to be held on September 15–18 at Oxford under the presidency of Sir Alfred Lyall (Prof. E. B. Tylor, F.R.S., being honorary president), the following papers will be communicated to general meetings of the congress:—the address of the president; religious wisdom cultivated in old Israel in common with neighbouring peoples, by Prof. von Orelli; l'Influence religieuse de l'Astrologie dans le Monde Romain, by Prof. Cumont; Buddhistic religious art, by Prof. Macdonell; some ethical developments of pre-Christian Judaism, by Dr. Charles; totemism, by Dr. F. Boaz; and the Cretan religions, by Dr. Arthur Evans, F.R.S.

AMONG the papers to be read at the International Congress of the Refrigerating Industries (which, as has already been stated in these columns, is to take place in Paris on October 5–12 next) we notice the following:—the effects of low temperature, by Sir W. Ramsay, K.C.B., F.R.S.; liquid air, by Dr. L. A. Groth; the construction of cold stores, by Mr. Hal Williams; notes on methods and apparatus for ascertaining the heat, conductivity, and insulating properties of materials, by Mr. W. D. A. Bost; the refrigeration of, and transport of refrigerated, fruit, &c., Mr. C. M. Simons, Mr. F. W. J. Moore, and Mr. H. J. Ward; ice-making and ice-machinery, by Mr. T. F. Mead; new industrial applications of cold, by Mr. H. Birkett; the organisation of cold-storage transport on railways—refrigerator-cars, cold-storage warehouses, and charges, by Mr. T. N. Wylie; and the organisation of cold-storage transport by sea, by Mr. J. T. Milton.

A SOMEWHAT severe earthquake shock is reported to have been felt at Shemakha (Transcaucasia) at 8 o'clock in the evening of September 1. The direction was from south to north.

A MONUMENT to Hermann von Wissmann, the German African explorer, was unveiled at Lauterberg, in the Harz, on Friday last.

MR. F. J. SEAVER, assistant botanist of the North Dakota Agricultural College, has been appointed director of laboratories in the New York Botanical Garden.

DR. K. WEGENER has, according to *Science*, been appointed director of the Observatory of Samoa.

THE new building of the medical college of Western Reserve University (which will be devoted to experimental medicine) is to be dedicated on November 20, when an address will be delivered by Dr. W. H. Welch, of the Johns Hopkins Medical School.

THE College of Physicians, Philadelphia, has awarded the Alvarenga prize for 1908 to Dr. William T. Shoemaker, for his essay on "Retinitis Pigmentosa."

THE fourth field meeting (to Bisley) of the Cotteswold Naturalists' Field Club will be held on Tuesday next, when those attending will drive from Stroud to the Frith Quarry and Worgan's Quarry to study the richly fossiliferous Inferior-Oolite strata there exposed.

ARRANGEMENTS are being made for the holding of fungus forays under the auspices of the South-Eastern Union of Scientific Societies in the neighbourhood of Tunbridge Wells on October 9 and 10. Full particulars respecting the same may be obtained from the honorary secretary of the cryptogamic section, Dr. George Abbott, 4 Rusthall Park, Tunbridge Wells.

DURING the past week there has been much activity in France in the interests of aërial navigation. On September 5 the military dirigible balloon, the *République*, made the longest flight so far achieved by her. Leaving her headquarters at Chalais-Meudon at 8.40, she crossed Paris, and proceeded by way of Senlis and Pont St. Maxence to Compiègne, which was reached at 12.30. Circling here, without stopping she returned to Paris by a slightly more eastern route, and was back at Chalais-Meudon by 3.10. The journey, which thus lasted six hours and a half, is estimated at between 180 and 200 kilometres. The balloon attained a height of 650 metres, and of the 420 kilograms of ballast with which she started, 190 kilograms remained at the end of the journey. At Le Mans, Mr. Wilbur Wright made at 7.30 in the morning a flight of 19m. 48 2-5s., which is within a few seconds of the "world's record" of Mr. Farman, namely, 20m. 9 3-5s. The distance covered is estimated at 22 kilometres. In attempting a second flight, which lasted 3m. 21s., at a height of 12 to 15 metres, a violent gust of wind drove the aëroplane so near the trees on the edge of the ground that a sudden turn had to be made, in the accomplishment of which the end of the left wing came in contact with the earth and was broken. On September 6 M. Delagrange made a flight lasting 29m. 53 4-5s., and covered a distance of 24 kilometres 727 metres. He was compelled to alight from want of fuel, having started with only 24 litres. This flight constitutes a new "world's record," both for time and distance.

ACCORDING to the *Pharmaceutical Journal*, an important step is being taken by the Commonwealth Government in regard to the adoption of uniform food standards throughout Australia. Under the present system each State fixes its own standards, the result being considerable variance, and consequent annoyance and expense to manufacturers and importers. Now that a Commonwealth analyst has been appointed, the way is cleared for federal action, and the proposal is made for a conference of Commonwealth and State expert authorities with the object of discussing the basis of united legislation.

IT is stated in the *Lancet* that out of 50,000 pupils in the primary schools of Milan 47,000 are more or less the victims of buccal maladies, mainly affecting the teeth, and steps are being taken to bring about a more satisfactory condition of things. To this end the Associazione per la Scuola, composed of representatives of the family and of the teaching profession, the Istituto Stomatologico Italiano, and the municipality, are acting in concert to make a periodical inspection of the primary schools, beginning with the coming scholastic year 1908–9. This inspection is to be carried out by the "Commissione d'Igiene," presided over by Dr. Ambrogio Bertarelli, and composed of Dr. Bordoni-Uffreduzzi, Dr. Clerici, and other eminent consultants and practitioners of the Lombard school. Under the auspices of this commission the inspection will be performed by the specialists attached to the Istituto Stomatologico, who will communicate to the parents or guardians of the children concerned the stomatological condition of each, illustrated with appropriate diagrams. A small fee will be expected from the well-to-do families of the children inspected, while the service will be rendered gratuitously to those of humble means. At the end of the year statistical tables of the cures effected and of the results obtained will be presented to the municipality.

OUTBREAKS of American gooseberry mildew having occurred in various parts of Essex, and the matter having been brought under the notice of a committee of the Essex County Council, the county inspectors have been authorised to enter premises where the disease is believed to exist with the view of specimens being sent to the county laboratories to be reported upon.

SILVER medals are this year offered by the Industrial Society of Mulhouse for the synthesis of a gum possessing the properties of Senegal gum, and for a handbook treating of the drugs used in the dyeing and printing industries; a medal of honour is offered for an economical substitute for dried egg-albumen, or for a decolourised blood-albumen for the same purpose. Other awards will be given for papers on the colouring matter or on the carmine in cochineal; the theory and manufacture of alizarin reds; the composition of aniline black; the transformation of cotton into oxycellulose; the composition of colouring matter and synthesis of a natural colour, various mordants, bleaching processes, and colours, &c. Papers, &c., must reach the Président de la Société Industrielle de Mulhouse, Alsace-Lorraine, before February 15, 1909. Further details may be obtained on application.

A CANAL of the width of rather more than 1000 feet is to be constructed through the island of Mühlenwerder, in the Elbe, where the Mühlenfeut joins the river, by which Hamburg will be enabled to use a considerable part of the island for future harbour construction, and to leave the waterway from the mouth of the Elbe to Harburg independent of Hamburg's shipping. Hamburg will in consequence be able to construct harbour basins independent of the part of the Elbe belonging to Prussia. The deepening and widening of the lower portion of the Elbe in 1896 resulted in the river being available for the increasing traffic and the larger dimensions of the vessels, but the improvements then made are no longer sufficient; hence the present proposal, the cost of the carrying out of which is estimated at 6,000,000*l*.

THE April number (vol. ii., part i.) of the Records of the Indian Museum contains a notice of the retirement of Lieut.-Colonel Alcock from the office of superintendent of the museum. Colonel Alcock came to India in 1886 as a member of the Government Medical Service, and after two years' professional work on the N.W. frontier he was gazetted surgeon-naturalist to the Indian Marine Survey; his appointment to the superintendentship of the museum took place in 1893. A minute of the trustees records the value of his services to the museum and to biological science generally.

WE have to acknowledge the receipt of a copy of the report of the Indian Museum for 1906–7, wherein attention is directed to the embarrassment caused by the smallness of the staff when two or three of its members are absent on leave. It is, however, hoped that a revised scale of pay will attract a better class of men to the posts filled by the non-gazetted members of the staff. Much satisfaction will be felt at the addition to the staff of a natural-history collector, and at the permission granted to the superintendent to visit various parts of India for the prosecution of faunistic and bionomical researches.

THE ravages of the coffee disease are so fresh in the memories of the inhabitants of Ceylon that it is no wonder the appearance of the bleeding disease of the cocoa-nut

trees caused considerable alarm in the colony. Fortunately, however, they are now better prepared to resist the attack of an epidemic caused by a parasitic fungus than they were twenty-five years ago. For one thing, they have a resident official mycologist who was able at once to tell them what steps to take to prevent the spread of the disease, and the growers were ready, if not eager, to take them. The illustration shows a young cocoa-nut tree destroyed by the disease (*Thielaviopsis ethaceticus*, Went.). The first appearance consists of a rusty or dusky bleeding patch on the stem, which is subsequently followed by others. As soon as the crown or "cabbage" is affected the tree dies, as in the figure. A bleeding patch is seen just above the level of "podion's," or boy's, head, and there are two other smaller ones higher up. Young trees succumb much sooner than old ones. The remedy recommended by Mr. T. Petch consists in cutting out the diseased areas with a chisel, searing the cavities, and applying hot coal-tar.

EARWIGS are generally supposed to make but very occasional use of their complexly folded wings. In the August issue (vol. iii., No. 2) of the *Journal of Economic Biology* Mr. W. E. Collinge records, however, that on many evenings in June and July last a considerable number of these insects was observed on the wing, apparently in the neighbourhood of Berkhampstead. Individuals—all males—ranging in number from seven to eleven flew into a single lighted room on three evenings, thus indicating that many more must have been on the wing out-of-doors.

AN article treating of the cotton varieties in Egypt is contributed by Mr. W. L. Balls to the *Cairo Scientific Journal* (July). The author, discussing the mixed nature of the crop, attributes it not only to the number of varieties, but also to the developments arising out of interplanting and natural crossing. With regard to the origin of the Egyptian cottons, while it is possible to indicate the source of one or two, such as Abassi and Yannovitch, the source of most of the varieties is indeterminable. Also it is noted that unless the original strain is pure, there must be continual breaking away from the type. A scheme for improving the crop is foreshadowed in connection with an experiment station that is being founded by the Khedivial Agricultural Society. The plan consists in obtaining pure varieties by selection or crossing, and the maintenance of such pure varieties by special precautions.

THE current number of the *Kew Bulletin* (No. 7) opens with a short account, communicated by Mr. G. Massee, of the fungus *Naemospora crocea*, that causes "die-back" of peach shoots. The genus falls in the group of "fungi imperfecti," as only the conidial reproductive stage is known. Conidial infection is limited to shoots of the year's growth. Mr. A. L. Simmons, adding to the list of Lepidoptera taken in the gardens, records the discovery of pupæ of the spurge hawk-moth and the capture of a swallow-tail butterfly. There are also published analyses of the tuberous roots of the new Ecanda rubber plant *Raphionacme utilis*, and an article on the cultivation of the date-palm in Mesopotamia.

MR. P. S. SPARKMAN, an Englishman settled in San Diego County, California, devoted his life to the study of the Luiseno tribe of Indians, among whom he lived. Before his murder in May, 1907, he had completed a dictionary of the native dialect and an account of the culture of the tribe, the latter being now issued as a part of vol. viii. of the University of California memoirs on archæology and ethnology. The culture of the tribe presents no features of special interest, that of the neighbouring and allied tribes having been already illustrated in other issues of the same series. Their food largely consists of various kinds of acorns, for the crushing of which a stone mortar is required. This is formed by chipping in a rock a slight cavity, round which a basin-shaped basket is fixed with pitch to prevent the contents from flying out when struck by the pestle. With constant use this cavity becomes deeper and deeper, until the basket, being no longer necessary, is removed. The tribe supplies an excellent example of that curious institution, the Couvade. The father for some time after the birth of a child is obliged to take as much care of himself as the mother does. He must not take cold, lest the health of the infant might be endangered; he cannot drink cold water in winter; he must eat the same diet as that prescribed for the baby. If the child dies, its mother attributes her loss to his neglect of these precautions. We have also a full account of the initiation rites, and the writer has been lucky enough to procure a full record of the exhortation addressed by the person in charge of the rite, who carefully explains, with appropriate warnings, the rules of life and etiquette which the boys and girls are expected to obey. Morals, in fact, merely mean the observance of the long-established customs of the tribe, and do not include those higher rules of conduct which the codes of more advanced societies prescribe.

THROUGHOUT Africa are played games, usually between two players, which consist of transferring counters from one hole to another; formerly these have been classed by ethnologists under the Nubian name of *mangala, mankalah*, &c., but Captain R. Avelot (*Bull. Soc. d'Anth.*, Paris, 1908, p. 9) distinguishes three types, for which he adopts the names of *tshéla* for the games of skill, *tab* for games of the backgammon group, and *ouri* (*uri*) for games of mathematical combinations. The author mentions the distribution of the three types, and gives a map of the spread of hole games, more particularly of *uri*, as well as a list of the native names for these games. He concludes that *uri* may be considered as absolutely African, which appears to have arisen among a Hamitic tribe in the neighbourhood of Ethiopia, whence it has been spread (*a*) by the Fulah across south Sahara to Senegambia, and thence to Guinea and Gabon; (*b*) by Nubian slave-merchants, who took it down the Nile; (*c*) by the Jaggas, who, skirting the south of the equatorial forests, carried it to Angola; (*d*) by a Negro-Hamitic people it proceeded south as far as Manica. The hole games have nothing to do with cup-markings. Although the author in this and his earlier paper (*Bull.*, 1906, p. 267) gives numerous references, he has overlooked S. Culin's paper, "Mancala, the National Game of Africa," Report U.S. Nat. Mus. for 1894 (1896), p. 597, and Flinders Petrie's remarks in "Egyptian Tales," 2nd series, 1895, p. 136.

THE lake dwellers of Lower Dahomey, who have been studied by Major-Surgeon Gaillard (*l'Anthropologie*, xviii., p. 99), do not present many analogies with the inhabitants of aquatic pile-dwellings of Malaysia and New Guinea. In some cases the village in the water is opposite one of the same name on the land, and there is evidence in favour of the view that the natives were driven to build on the lake to escape the depredations of the Dahomians, who could not cross the water on account of the fetish customs. The lake-dwellers are not attached to their condition, and during the existing state of political security have returned to the ordinary agriculture of the district, though still remaining fisher-folk, and a great number have built ordinary land houses, but these are accused by those who remain faithful to the pile-dwellings of being incapable fishermen. Fishing grounds are free to all. The miserable condition of their houses is due to a lack of forethought and a passion for tobacco and alcohol.

THE Corps of Mining Engineers of Peru has issued a *Boletin* (No. 57), by Mr. E. A. L. de Romaña, on the tin deposits of Bolivia and prospecting for tin in Peru. Covering 100 pages, with twenty-eight illustrations, it is the most complete memoir on the subject, and is of special interest at the present time, when growing uneasiness prevails that many of the world's best tin deposits are becoming exhausted, and that the price of the metal may possibly become quite prohibitive for many industrial purposes before long. In Bolivia it is evident that the prospects are good. That country now ranks second only to the Federated Malay States as a tin-producing district, and it is probable that the production will continue to increase. Careful exploration has been made in Peru in the provinces of Huancané and Chucuito, which are the nearest to the Bolivian tinfields, but with the exception of some samples of stanniferous lead ore found at Vilque Chico, no indications of tin were discovered. Another *Boletin* (No. 56) issued by the Corps of Mining Engineers of Peru deals with the problem of the irrigation of the valley of Ica.

THE records of the Mysore Geological Department (vol. vii.), which have just been received, contain general and special reports of work done from July, 1905, to June, 1906. The year was an eventful one in the history of mining in Mysore owing to the attention devoted to the mineral resources by European and native capitalists. Chief among the ores sought were those of manganese and chromium. Other minerals which engaged attention were magnesite, asbestos, and chromium. The new gold-field near Lingadhalli gave good indications. The value of the gold raised in the Kolar area during the year was 2,274,786*l*. The results of the work of the Geological Survey during the year are given in special reports by Mr. H. Kelsall Slater and Mr. P. Sampat Iyengar. The former made a geological survey of 180 square miles in the Kadur district. The boundary of the granite extending from Tarikere westward was mapped, and found to conform generally with the base of the hills. The schists composing the hills are invariably associated with the occurrence of gold. Numerous old workings were discovered, and the area deserves careful prospecting, as it bids fair to prove another valuable goldfield. In the Mysore district the felsite and porphyry dykes of Seringapatam were mapped. In the Tumkur district traces of gold were found, and the native workings of grey corundum are described. Reports are also given on the geology of the Srinivaspur and Kadri Taluks, and of parts of Challakere and Sira Taluks. An appendix is devoted to a detailed description of important felsite and porphyry dykes in the neighbourhood of Seringapatam. When polished they yield very handsome ornamental and building stones.

IN his "Studies on the Thermodynamics of the Atmosphere," published under the auspices of the Weather Bureau, U.S. Department of Agriculture, Prof. F. H. Bigelow collects nine papers which he had contributed during 1906 to the *Monthly Weather Review*. They deal with a variety of subjects, including the meteorological conditions characteristic of different parts of cyclones and anticyclones in Europe and America during summer and winter. The temperature gradient at different levels receives special attention. The last four papers deal with a waterspout, or waterspouts, seen near Cottage City, Mass., in August, 1896. In most of the papers, even those relating to the waterspout, there is a rich profusion of statistical data, conversion factors, and mathematical formulæ. There are sixty-seven tables and forty illustrations, including ten full-page photographs of the Cottage City waterspout at different stages of development. A large number of conclusions are also drawn. The papers provide evidence of industry and imagination, and the conclusions and observational data will no doubt receive the critical examination of meteorologists.

A CORRESPONDENT has written to point out that Prof. von Thán, to whose memory a monument is to be erected at Ó-Becse, Hungary (see NATURE, August 27), held the chair of chemistry from 1862 in the University of Budapest, and not that of the University of Vienna.

OUR ASTRONOMICAL COLUMN.

DISCOVERY OF A COMET, 1908*c*.—A telegram received from the Kiel Centralstelle announces that a new comet was discovered by Prof. Morehouse, of the Drake University Observatory, Des Moines, Iowa (U.S.A.), on September 1. At 8h. 40m. (Yerkes M.T.) on that date the position of the comet was

R.A. = 3h. 20m., dec. = 66° 15′ N.,

and it was reported as moving rapidly in either a south-east or north-west direction. It was also said to possess a long, conspicuous tail.

A second telegram states that this object was observed by Prof. Thiele at Copenhagen on September 3, its position at 10h. 29·6m. (Copenhagen M.T.) being

R.A. = 3h. 19m. 43s., dec. = 67° 14′ 42″ N.,

so that the motion is north-west. This observation gave the magnitude of the comet as 9·0. The present apparent path lies through Cassiopeia towards Cepheus, and the comet does not set below the horizon in London; it reaches the zenith about 4 a.m., and should therefore be an easy object for telescopic observation.

The comet was quite easily found with a 3¼-inch equatorially-mounted finder at South Kensington on Friday night, September 4, and in the 10-inch refractor, with a power of 100, appeared as a very diffuse, nebulous patch with scarcely a trace of any stellar nucleus.

LARGE SUN-SPOTS.—The large sun-spots illustrated in these columns in our issue of August 13 have again appeared round the eastern limb of the sun. They were first re-observed at South Kensington on August 27, and have since been visible to the naked eye. On August 29 a new, large spot followed, and on September 1 the large group, two large single spots, and a smaller group were to be seen on the disc. For the actual epoch of solar activity the disc is, therefore, displaying a remarkable amount of spotted area.

RECENT METEORS.—Some interesting notes on meteors recently observed are published by Mr. Denning in No. 400 of the *Observatory* (p. 350, September). Mr. Denning remarks on the favourable conditions for meteor observations that obtained during the present summer, and states that on fifteen dates between July 18 and August 8 he observed 204 meteors during 20½ hours. Of these, more than half were traced to known radiants, forty-two of them being Perseids. During the period July 26 to August 7, the Perseid radiant moved from 25°, +53°, to 41°, +56°; a late Perseid was seen, on August 17 at 9h. 25m., which left a fine streak, and was directed from 51°, +58°; another, of the first magnitude, was seen on August 19d. 9h. 44m., and its direction was from a point at 56°, +60°.

For the period June 25 to August 10, Mr. Denning received duplicate observations of ten meteors, for which he gives the particulars of the real paths.

D_3 (HELIUM) ABSORPTION IN THE SOLAR SPECTRUM.—In a letter to the *Observatory* (No. 400, September, p. 353) Captain Daunt reports that he believes he observed D_3 dark in the solar spectrum when making visual spectroscopic observations of the large sun-spot group which was near the eastern limb on August 1. The line had much the same appearance as that shown on the photograph taken by Mr. Nagaraja at Kodaikánal last year, running as a fairly fine dark line, somewhat thickened in the centre, right across the group. Although the sun was getting low at the time—between 5 p.m. and 6 p.m.—Captain Daunt believes that the line seen was not of atmospheric origin, for he was unable to see it anywhere else on the disc, and it stopped short a little way on either side of the penumbra.

THE SPECTRUM OF THE NEBULA HV 15 CYGNI.—The spectrum of the Milky Way nebula HV 15 Cygni was photographed by Prof. Max Wolf with the Waltz reflector on August 3. An exposure of 3½ hours was given, and the resulting spectrum shows the light-source to be gaseous.

By far the brightest line is that at the violet end of the spectrum, λ 373; the line at λ 434, the band at λ 500, and the lines at $\lambda\lambda$ 369, 397, and 411 are also present, but faint, their intensities being in this order. Possibly there is also a line at λ 360, but this is doubtful. Prof. Wolf hopes that by having his mirror re-silvered he will be able to obtain a much stronger spectrum (*Astronomische Nachrichten*, No. 4271, p. 379, August 29).

THE PARIS OBSERVATORY.—M. Baillaud's first report as director of the Paris Observatory gives an account of the work performed during 1907, and follows its predecessors in general form. Among the records of a vast amount of routine work performed there are one or two points of general interest which call for special remark.

During 1907 the "cercle meridien du jardin" was employed solely for the study of recent improvements to the instrument, and the report gives the results at some length. The automatically registering micrometer has given unhoped-for precision; the difference of personal equation amongst the observers is practically absent, and shows no variation with the magnitude or with the amount or direction of the motion of the observed object. The mean error of a passage is reduced to ± 0.03s. instead of the ± 0.05s. obtained by practised observers using the electric method and ± 0.07s. with the eye and ear method. It is hoped to complete the tenth fascicule of the "Atlas photographique de la Lune" during the current year, and it appears necessary that, in order to complete the work satisfactorily, two more fascicules must be issued.

The new stellar spectroscope, of which M. Baillaud gives an illustrated description, is used, in conjunction with the equatorial *coudé*, for the determination of radial velocities, and, with its greatest dispersion, gives a spectrum in which, at Hγ, each millimetre includes four Ångström units. The time service and the *carte du ciel* work have been carried on as usual, and for the latter full statistics are given showing the progress made.

INSTITUTION OF MINING ENGINEERS.

THE nineteenth annual general meeting of the Institution of Mining Engineers, which was held in Edinburgh on September 2–4, was largely attended, and was altogether a most successful gathering, the papers, discussions, and excursions being all of more than ordinary interest. Dr. R. T. Moore (Glasgow) was elected president for the ensuing year. The annual report showed that the membership was considerably more than 3000. It was announced that owing to the death of the secretary, Mr. M. Walton Brown, various changes had been found necessary. The headquarters of the institution would be moved from Newcastle to Westminster, and Prof. L. T. O'Shea (Sheffield University) was appointed honorary secretary and Mr. P. Strzelecki assistant secretary.

Of the five papers on the programme, the first read was by Mr. Henry Hall, H.M. Inspector of Mines, on coal-dust and its treatment with calcium chloride. He dealt first with the history of coal-dust in relation to colliery explosions. The first reference to the matter was in a report by John Buddle in 1803, but it was not until 1874 that it began seriously to be argued that coal-dust could of itself cause a colliery explosion in the absence of fire-damp. At the present time coal-dust is regarded as the chief agent of destruction. Experiments made by the author showed that the quantity of coal-dust deposited day by day in a mine is much less than is usually thought. When once the roads have been made clean it is easy to keep them so. Watering with the view of laying the dust is impracticable where the rocks are friable shales, as it tends to cause accidents from falls of roof and side. Calcium chloride promises to obviate the difficulty. The application of the solution, or, better still, of the dry powdered salt, is effective for three months. The discussion was well sustained. Mr. H. M. Cadell suggested that a cheaper hygroscopic material, such as common salt, might be tried. Mr. Bennett Brough mentioned that calcium chloride was being successfully used in Washington on macadamised roads to obviate the dust nuisance. Mr. W. C. Blackett stated that calcium chloride had proved efficacious in a Durham colliery.

The next paper read, that by Mr. G. B. Walker, on the practical use of colliery rescue apparatus, embodied a set of rules for the use of such apparatus. He was of opinion that the course that would be adopted in this country was to have central rescue stations maintained by the coal-owners' associations. In the discussion it was suggested that there was a danger of the possibilities of rescue apparatus being exaggerated. Mr. W. E. Garforth, however, strongly supported the views expressed in the paper, and Mr. C. E. Rhodes believed that, apart from the humanitarian aspect of the question, there was great use for the apparatus in saving property in mine fires.

The paper by Mr. John Gemmell on the Wemyss coalfield contained much interesting historical detail compiled from the journals of the second Earl of Wemyss (1610–1679), who devoted careful thought to the development of the coal seams on his estates. The review of the present condition of the mines contained much information of value. A diamond bore has just been put down on the estate to the enormous depth of 4534½ feet. Temperature observations were made, the lowest reading taken being at a depth of 3955 feet, where the temperature was 92°.2 F., giving an average thermal gradient from the surface of 1° F. in 87½ feet. In the discussion Mr. Brough emphasised the value of the temperature observations in this bore-hole, as it was probably the deepest in Great Britain. The temperature increase was lower than the average of the observations collected by the British Association Underground Temperature Committee. Mr. J. S. Dixon suggested that this discrepancy could be explained by the cooling action of the flow of water encountered at depths of 1577 feet and 1827 feet. Papers by Mr. J. G. Thomson on the deep diamond boring at Balfour Mains, Fifeshire, and by Mr. William Caldwell on the working of oil shale at Pumpherston, were taken as read, and the proceedings terminated with the usual votes of thanks. On September 3 the members visited the Wemyss collieries and the Pumpherston oil works and shale mines, and on September 4 there was a steamer excursion to the Kyles of Bute.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

DUBLIN.—In connection with the meeting of the British Association, the following honorary degrees have been conferred:—*D.Sc.*, Mr. Francis Darwin, F.R.S., Sir David Gill, K.C.B., F.R.S., Dr. William Napier Shaw, F.R.S., Captain Henry George Lyons, F.R.S., Prof. Horace Lamb, F.R.S., Prof. Charles Scott Sherrington, F.R.S., Prof. Ernest Rutherford, F.R.S., Prof. Archibald Byron Macallum, F.R.S., Dr. Albert Kossel, and Dr. Ambrose Arnold William Hubrecht; *M.D.*, Sir Thomas Lauder Brunton, Bart., F.R.S.; *LL.D.*, Sir James Augustus Henry Murray.

THE new building of the engineering laboratory of the Heriot-Watt College, Edinburgh, will be opened by the Earl of Rosebery on September 16.

THE Board of Education has issued (Cd. 4288) the first part of "Statistics of Public Education in England and Wales, 1906–7–8." The present part is confined to educational statistics; the second part, which will appear later, will deal wholly with financial statistics. The number of technical institutions in England recognised by the Board during 1906–7 was 31, and the number of teachers therein 521. The Board defines a technical institution as one giving an organised course of instruction in day classes, including advanced instruction in science or in science and art, and provided with a staff and equipment adequate for the purpose. The number of students who attended these institutions at any time during the year was 2655 (including 325 girls and women), and 1446 of these attended a full course of instruction. Of the 2330 boys and men attending, 542 were under seventeen years of age, and 469 were twenty-one years of age or more. The number of evening schools and classes in England recognised by the Board for the education of persons already engaged in some occupation which takes up the greater part of their time was 5368 in 1906–7. These classes varied very widely in character and scope; 29,946 teachers were employed in them, and 687,681 students attended during the year, and the Board paid grants on account of 515,897. There were in the same year 676 secondary schools in England recognised by the Board as eligible for grants as compared with 600 in the previous year. These schools accommodated 62,712 boys and 50,877 girls, the numbers in the preceding year being respectively 60,353 and 44,681.

THE regulations which deal with the position of *agrégé* in the Paris faculty of medicine and the joint faculty of medicine and pharmacy have, according to the Paris correspondent of the *Lancet*, recently been modified by the Minister of Public Instruction. The new regulations will not, however, come into force until the commencement of the scholastic year 1909–10. The qualifying examination consists of three sections:—(1) anatomy, physiology, physics, chemistry, and natural science; (2) medicine; and (3) surgery and obstetrics. In the first and third sections special branches may be taken according to the particular branch to which the candidate has devoted himself. The course of the examination is as follows:—(1) a written essay in anatomy, physiology, and histology; (2) a *viva voce* examination, lasting three-quarters of an hour, in general pathology; (3) a clinical examination; and (4) an examination in practical pathological anatomy. Once a candidate has been declared qualified he maintains his position for life, and all candidates who were qualified in examinations held previously to November, 1907, are dispensed from the above-mentioned examinations. For admission as *agrégé* the following tests have to be passed:—(1) The candidate must hand in his testamurs and other documents (*titres*). (2) He must give a lecture of one hour's duration without an assistant or notes. Four hours are allowed for the preparation of this lecture. (3) Practical work.

PROF. JOHN W. GILMORE, of the Pennsylvania State College, has been chosen president of the College of Agriculture and Mechanic Arts of Hawaii, situated at Honolulu, which was opened on Friday last.

THE report for 1908 of the president of Yale University states that plans for the immediate future at the University involve the development of courses in regional geography until there are instructors who are authorities on the geography of each of the continents. This will eventually necessitate the erection of a separate department of geography, which will not only offer courses, but will also conduct explorations in the less known parts of the world, particularly those parts where the character of the physical features has been a prominent factor in the life of a race, such expeditions being in charge of officers of the department, and including advanced students.

MR. A. H. MACKENZIE, of the University of Aberdeen, has been appointed professor of science and manual training at the Allahabad Training College for Secondary Teachers.

AN exhibition of the work of teachers and pupils of Indian schools of general education is to be held in Mysore on October 6–12 next. Five classes of exhibits are to be arranged for, namely, infant and primary schools; secondary schools for boys; secondary schools for girls; collections of objects suitable for school museums; and records of teachers' work. A number of English exhibits are to be sent by the English Board of Education and the Director of Public Instruction, Madras.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 31.—M. Bouchard in the chair.—Concerning *Trypanosoma congolense*: A. **Laveran.** Details are given of experiments on goats. In one case the animal acquired complete immunity against *T. congolense*, but this immunity did not extend to infection by *T. dimorphon*. In the second case, immunity against *T. congolense* was also attained, and experimental inoculations with *T. dimorphon* are in progress.—Pfaff's problem: A. J. **Stodolkievicz.**—Periodic functions: P. **Cousin.**—The temperature of dissociation of ammonia and carbon monoxide: Herman C. **Woltereck.** Ammonia, carefully purified from moisture and traces of organic matter, was passed through a Jena glass tube, the temperature of which was controlled by a Le Chatelier pyrometer. The first traces of dissociation were observed at 620° C.; the lower temperatures noted by other investigators are probably due to the presence of traces of impurity. Carbon monoxide commences to dissociate between 570° C. and 580° C.—The white disease of the oak and *Erysiphe quercus*: M. **Boudier.**—The action of human serum on *Trypanosoma pecaudi*. The differentiation of *T. pecaudi* and *T. gambiense*: A. **Thiroux** and L. **d'Anfreville.** From experiments on apes it is concluded that human serum exerts a preventive and curative effect as regards infection with *T. pecaudi*, and this effect falls off very slowly.

NEW SOUTH WALES.

Royal Society, June 3.—Mr. W. M. Hamlet, president, in the chair.—The viscosity of water: Richard **Hosking.**—Note on a cupriferous porphyrite and quartz veins in the Nelligen district: Dr. H. I. **Jensen.** The author briefly describes a curious basalt formation between Nelligen and Braidwood which contains inclusions of schist, limestone, reef quartz, and quartz porphyry, and in addition small bunches of native copper and copper ores. Unlike the Bumbo basalts, this basaltic rock contains no copper at all except in the vicinity of the other inclusions. It is inferred that the copper, in common with the other xenoliths, has been torn out of a mineral vein along which the magma found egress to the surface. A number of quartz veins which cut out in both directions, or in depth, occurring in the same district, are attributed to pneumatolytic processes in the period in which the ancient palæozoic rocks underwent metamorphosis.

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