THURSDAY, SEPTEMBER 8, 1887.

HYDROPHOBIA.

THE culmination of scientific knowledge in any special direction frequently appears to the casual observer as a sudden and unforeseen event, although actually the result of a combination of well-ascertained facts accumulated during many years. The introduction of rabies from among the inexact to among the more exactly known diseases has been however so rapid as to very fairly substantiate this popular belief. It is now scarcely more than three years ago since the self-sacrificing labours of M. Pasteur helped us to pass from superstition to accurate knowledge of the real nature of rabies or hydrophobia, and this passage from the pre-scientific to the scientific epoch of the subject was actually perfectly abrupt. Nearly the same thing may also be said of the discovery by Koch of the *Bacillus tuberculosis*, for in both cases the scientific grasp of the subject undoubtedly commenced with the discovery how the virus might be isolated for purposes of experiment. While, however, this is strictly the case with tuberculosis, it is but partially true for rabies, for though there cannot be a shadow of doubt that the micro-organism which is the virus of rabies will soon be demonstrated in pure cultivations, this one factor is yet wanting to place it on the same level as that of tuberculosis. This difficulty in obtaining pure cultures, though a serious defect in our information, is yet most interesting, for it affords a distinct interpretation of several facts in the etiology of the disease, which, as we shall see directly, proved obstacles to early inquirers, and which yet at the same time, when viewed in the present light of science, are most encouraging to those who are anxious to see this miserable evil extinguished for ever.

All observers, notably Mr. Dowdeswell, are agreed that a micrococcus can be demonstrated in the tissues of the spinal cord of animals affected with the disease, but unless we accept the doubtful results of Fol no one has yet succeeded in cultivating this micrococcus. Those familiar with the difficulties of "rearing" pathogenic organisms will readily understand this obstacle in a disease with such a long incubation period as rabies.

M. Pasteur at the outset of his investigations attempted to solve the problem in this direction, but fortunately for science soon abandoned it in view of the probability that the virus would best be dealt with by endeavouring to obtain it in quantity from the central nervous system, since from the symptoms it evidently there produced its greatest effect, and so might be expected to be more especially present. He therefore made an emulsion by crushing in sterilized *bouillon* portions of the central nervous system, specially the spinal cord. With this emulsion he inoculated the disease from animal to animal by injecting a small quantity of it beneath the dura mater. By this simple procedure he established the first of his most important discoveries, viz. the real incubation period of the disease. At the same time too, as is usual in instances of a genuine scientific advance, the one important discovery led to a further one, since he has thus presented science with an infallible means of determining whether an animal had really suffered from the disease or not.

To the public this test has already been of the utmost value, since, as is well known, the characteristic lesions in the alimentary canal, &c., being absent or but feebly marked in the early stages of the disease, the possibility of thus giving a definite opinion in cases of doubt by the aid of Pasteur's method has frequently been the means of affording the utmost relief to the minds of those who have been victims to the lingering dread of hydrophobia. We are not aware however that the slightest public expression of gratitude has ever been expressed in this country towards that experimental science which in M. Pasteur's hands has led to such an important result, or that we, who by reason of rabies being endemic among us are profiting, and will profit, enormously by the light thus shed on the subject, have acknowledged our indebtedness to him in any way.

From his researches M. Pasteur was led on to formulate certain deductions which might be accepted as the logical consequence of the theory of the disease thus shown by him to be indubitably zymotic.

In the first place, M. Pasteur considered it probable that he could attenuate the virus which he had just discovered the possibility of handling with certainty, and we may add safety. This he soon accomplished by the method of drying.

It might naturally be supposed that, having proceeded thus far, he would have been led to attempt protection from, and prevention of, the evil effects of the disease by inoculating with this attenuated virus. Indeed it is unfortunately the popular belief that he did do this, and that his efforts to cure the malady are conducted upon this plan.

As a matter of fact, however, M. Pasteur, with a much wider prescience of the facts and theories of zymotic disease in general, considered that the well-known arrest of development which happens to virulent organisms as a consequence of their growth in tissues or in artificial culture media was due to the production by their metabolic processes of katalyzed substances whose presence was inimical to the active growth of the microbe, and that therefore these substances might be regarded and used as antidotes to the virus. Acting on this assumption, he proceeded to endeavour to protect animals by injecting into them considerable quantities, not of the virus, as generally supposed, but of these antidotal substances, or possibly of one alone. Of course, knowing that as a rule attempts to isolate in a state of chemical purity such products usually failed or seriously altered them, and having already ascertained that the baleful influence of the virus could be abrogated or rendered nugatory by the process of drying, he proceeded to employ the simple method of injection emulsions of dried spinal cords. The injection of such emulsions was naturally performed understanding that the drying would probably also impair to a certain extent the protective value of the antidotal substance; to insure, therefore, the introduction into the animal of as large a quantity as possible he considered it advisable to inject emulsions on successive days from spinal cords which had been dried for shorter and shorter periods, till ultimately, having commenced by inoculating from cords which had been dried for fourteen days, and which had been proved to be perfectly innocuous, he arrived at a cord which had been dried for only twenty-four hours. Of course the introduction of such virulent matter as this had no effect on systems already armed by the previous inoculations of antidotal substance. It is greatly to be regretted that owing to general ignorance of this fundamental principle of M. Pasteur's method the many and violent discussions upon his treatment have been rendered absolutely useless. We trust, therefore, we may be excused for having dwelt thus at length upon it.

As soon as M. Pasteur had demonstrated to the French Academy that he could secure the *protection* of dogs by injecting them in the manner above described, he was induced to attempt the *prevention* of the disease in man by similar injections after the bite. This attempt soon developed into a regular practice, from the numbers of patients who flocked to Paris seeking treatment in consequence of the unhappy prevalence of rabies in those European countries which had neglected to provide for its easy extermination by suitable legislation.

From the Report of the Committee commissioned by the Local Government Board to inquire into M. Pasteur's treatment, it would appear, as might have been expected, that a comparatively large proportion of M. Pasteur's patients were bitten by dogs which were not rabid; but it is also evident from the same Report that when deductions have been made for these cases the death-rate among the remainder was far lower than even the lowest estimate ever formed of the mortality from hydrophobia among persons bitten by reputedly rabid dogs.

While this gratifying result was accomplished, it was at the same time evident that the method was by no means perfected, and the lamented death of Lord Doneraile (from the bite of a tame fox which had been infected by a dog), affords an illustration of this, for the deceased nobleman was subjected to treatment within a few days after receiving the virus. But while the inoculations did not prevent a fatal issue, there seems good reason to believe that they notably modified the distressing features of the malady, for in a brief account before us it is stated that the inability to swallow fluids only appeared twenty-four hours before death, and there was at no time spasm produced by swallowing moist solid food. The same gratifying modification appears to have also been present in another case that recently was observed in St. George's Hospital. Should this modification prove to be general, M. Pasteur will have deprived the malady of its worst tortures.

Before leaving the consideration of this part of the subject it is to be noted that the virulent opposition with which M. Pasteur's efforts in the cause of humanity were met went to the length of charging him with having actually caused the death of some of his patients by his inoculations. This charge, though not supported by any exact evidence whatever, was also inquired into by the above-mentioned Committee appointed by the Local Government Board. Indeed, they had a special opportunity of doing so, for one of the laboratory servants of Mr. Horsley (who carried out the experiments for the Committee), being bitten most severely by a rabid cat, died six weeks later (the usual incubation period of the disease) with the paralytic form of hydrophobia rather than the excitable form. Rabbits inoculated from the spinal cord of this man died with the shortest possible period of incubation. As the Committee point out, this would seem to have lent colour to the idea that the inoculations themselves were fatally virulent, had not similar instances of short incubation periods been occasionally observed to follow inoculation from similarly rabid animals. Stress was also laid upon the mode in which the man was inoculated—namely, by what M. Pasteur called the intensive treatment, and which he adopted in cases of very severe injury. But the whole question was dismissed by M. Pasteur altering this mode of treatment in order that there should not even be the semblance of the possibility of such an accident.

It seems to us that, in England at any rate, there is quite another view to be taken of this question of rabies and its scientific prevention—in fact, that its complete extermination should be ensured in preference to efforts to treat it after it has attacked anyone.

The data upon which legislation should be based are now fortunately at hand. The House of Lords recently appointed a Select Committee, under the chairmanship of Lord Cranbrook, the President of the Privy Council, to inquire into the whole question of the social bearing of the disease, and the means which have been adopted to get rid of it in foreign countries. The Report of that Committee is published, and we have been permitted in addition to inspect the evidence laid before it. This evidence is a most instructive comment upon the manner in which the facts of modern science are sometimes treated as being of only equal value with the most absurd misstatements dictated by charlatanism and abandoned self-interest.

As a whole, however, the Report is one with which we have good reason to be satisfied in many ways, for it recognizes the great value of the simplest means of preventing the spread of the disease, viz. the muzzle.

Those of us who remember the senseless anti-vivisectionist opposition which met the police edict enforcing this salutary measure in London, will not be surprised of course to find in the evidence before the Committee the same thing repeated, but, as was inevitably the case, deprived this time of all its deceptive influence.

For the experience of the working of the muzzle in London, where it brought the number of deaths from hydrophobia down from 27 in 1885 to 0 in the last quarter of 1886, and indeed we believe we may also say the first six months of 1887; the experience of its working in Nottingham, where the cases of rabies varied directly in number according to the way in which the muzzle regulations were enforced; of Prussia, where the disease is almost extinct, being one-fiftieth part of that in Great Britain; of Scandinavia, where it is absolutely extinct—all disproved the baseless theories and assertions of those who, under the guise of pseudo-zoophilism, endeavour to perpetuate in man and the lower animals the torments of this horrible disease. As will doubtless have been already surmised, the whole of this factious opposition to the above-mentioned beneficent legislation came from the small clique of anti-vivisectionists who were unhappily represented on the Committee itself in two of its members, viz. Lords Mount-Temple and Onslow.

The Lords' recommendation of the muzzle, however, is marred by one defect, and that a very serious one.

Here is a disease, a zymotic disease, the virus of which, as we have just seen, is most difficult of isolation, and evidently easily destroyed by ordinary conditions when it has left living tissues; a disease, too, which is fortunately infrequent compared to many others, and again fortunately one which would become extinct if not kept in existence by transmission from dog to dog; a disease, in short, which needs nothing but the firm administration of the most ordinary rules of preventive medicine to be destroyed utterly,—and yet, in deference to the professional agitation before mentioned, the Lords' Committee only recommend the use of the muzzle when the disease is "prevalent." If this means, and it is capable of being interpreted in two ways, that the Lords' Committee think the muzzle should be applied only when the disease is epidemic, nothing more regrettable can be imagined. To the scientific mind it seems almost incredible that a legislative body should hesitate to grasp the opportunity, the easiest ever offered, of eradicating a disease so painful and utterly incurable when once the symptoms have declared themselves; but here unfortunately is the example we referred to above of scientific fact overridden by vulgar prejudice. For this disease, acknowledged by all who have studied it to be transmitted solely by inoculation of one animal by another, is endemic in Great Britain, is paramount in the manufacturing districts and great cities; and yet the Lords' Report, instead of recommending the universal application of the muzzle, which would abolish the evil from these its centres, is content to leave it to the local authority—Heaven save the mark!—to apply the remedy when the disease has already made sufficient havoc (!) as to call for its suppression. It is sad, too, to see that this view, which we must call narrow, runs through the whole Report, but it is gratifying to find that that Committee, at any rate, fully appreciates the high worth of M. Pasteur's invaluable test of the disease.

In conclusion the Lords say that, should M. Pasteur's method of treating the disease be found of value, provision should be made for its introduction into England. While heartily concurring in this recommendation, we cannot but feel grieved that the necessity for it in England should be permitted to exist; for in this country, like Scandinavia, the introduction of the disease can be prevented; so that if proper measures were taken England would enjoy the same complete immunity from it that Sweden does at the present day. With reference to the adoption of M. Pasteur's mode of treatment into this country, a most fundamental difficulty arises at the outset, viz. that we have no public laboratory where investigations of this kind could be carried on for the nation, and that therefore an institution of the kind would have to be established for this and kindred subjects of inquiry. At the present time there is, unfortunately, little hope that this want—which we have before so frequently pointed out is nothing short of a national disgrace—will be adequately met; and, as a matter of fact, questions of this sort are usually decided at the Brown Institution, the nation being thus lamentably dependent upon the assistance of a private charity.

Oddly enough this necessity has just been provided for in France by the institution of a *quasi*-private laboratory—the Pasteur Institute. We are, however, strongly of the opinion that questions of this kind are of an Imperial character, and as such should be dealt with by the central Government in a properly-fitted institution.

Hydrophobia from time immemorial has been the most dreaded of all diseases, and justly; but no doubt this dread has been intensified by ignorance of its causation, an ignorance which, having existed for more than 2000 years, has just been dissipated to an enormous extent by the scientific labours of M. Pasteur. This advance is, of course, a source of vexation to the misanthropic anti-vivisectionists, who are shamefully endeavouring to bolster up the exploded theory of spontaneous generation in order to hamper the efforts of preventive medicine to stamp out the disease, regardless also of the evidence from countries where it has been so rooted out, and where, owing to its importation being prevented, it has never appeared again.

And while we have made this great step forward in our knowledge of the nature and etiology of the disease, we have at the same time learnt, thanks again to M. Pasteur, how to protect animals from its ravages, how to prove or disprove its existence in the absence of clinical or anatomical evidence, and, although this is still *sub judice*, apparently how its fatal effects may be warded off in the human being, and, if not successfully prevented, possibly ameliorated.

Finally, a most satisfactory outcome of this increase in our scientific knowledge is the revelation to us that by the adoption of certain legislative means we may destroy the evil once and for all.

POPULAR BOOKS ON BIRDS.

Ocean Birds. By J. F. Green. With a Preface by A. G. Guillemard, and a Treatise on Skinning Birds, by F. H. H. Guillemard, M.D. With Illustrations by Frances E. Green. 4to, pp. viii.–93. (London: R. H. Porter, 1887.)

Bird Life in England. By Edwin Lester Arnold. 8vo. (London: Chatto and Windus, 1887.)

TWO books deserving the above title are before us. It is well known that some of the most interesting works on ornithology have been written by men who do not profess to be scientific naturalists, but who exhibit an intelligent acquaintance with their subject and also possess a faculty of description the want of which adds so much to the dullness and heaviness of style with many more ambitious writers. Anyone who has made an ocean voyage knows full well that the hours are often apt to hang heavy on the hands of the passengers; and if this is true on board a steamer, it is much more true in the case of a sailing-vessel. Mr. Green therefore has compiled a volume which aims at giving assistance to voyagers in the southern oceans, providing short descriptions of the species of sea-birds most commonly met with; and as the author has travelled much by sea himself, it may be taken for granted that he knows the wants of an ornithological inquirer on board a vessel, and has done his best to supply the information. A "Glossary of Terms," and a chapter on the preparation of bird-skins, have been furnished by the author's friend Dr. Guillemard, whose excellent account of the voyage of the *Marchesa* is one of the most readable of modern books of travel. The work is divided into three parts, the first treating of the petrels, the second of the frigate-birds, gannets, and tropic birds, and the third of the gulls and terns. Mr. Green has given a very correct account of all the best-known species belonging to these groups, and for a second edition he may find a few useful notes on some of his marine friends in the volume published by the Royal Society on the Transit of Venus Expedition to Kerguelen Island. One at least of the notes here published is given by Mr. Green, but only as an extract from our volume of "Aves" in "Cassell's Natural History." The illustrations which accompany the work may be sufficient to identify the various species represented, especially the albatrosses, but they are rather roughly done, and that of the flying petrel is nothing but a caricature. No figures taken from mounted birds are ever satisfactory, and Miss Green's illustrations are no exception to the rule.

Mr. Arnold's little work will rank with any that we know of for pleasant reading, either from a sportsman's or an ornithologist's point of view. Some of the descriptions of game and wild-fowl shooting are exceptionally good, and carry with them a scent of the moor and the sea. Despite an acquaintance with several standard works on birds, the author seems to cling with respect to some of the more pretentious but second-rate books which pass muster as histories of British birds. It is, however, somewhat of a treat to find His Royal and Serene Highness the Prince of Mantua and Montferrat (!) spoken of under his original title of Groom Napier, though we should never call him a "first-class" authority. Many well-known names are wrongly spelt throughout the book, and these shortcomings should be corrected in a subsequent edition, when we should also like to see that Seebohm's excellent "History of British Birds" has come under the author's ken. It is to works on natural history like Mr. Arnold's, where real instruction is conveyed in elegant English, so that the acquisition of knowledge is rendered pleasant and easy, that we owe so much of the interest which has of late years been awakened in scientific pursuits; and we should be captious indeed were we to point out small errors in a book the perusal of which has given us so much enjoyment. Not the least useful feature of the work is a chapter by Mr. Brodie Innes on "Grouse Moors and Deer-Forests."

Should the works under review pass into a second edition, we should be glad to point out to the authors certain emendations which have occurred to us, of too little moment, perhaps, to mention in a review, but which would add somewhat to the finish of the volumes.

R. BOWDLER SHARPE.

OUR BOOK SHELF.

First Lessons in Science; designed for the use of Children. By the Right Rev. J. W. Colenso, D.D. (London: Ridgway, 1887.)

THIS book was written more than a quarter of a century ago for the use of a class of natives of the diocese of Natal, who were learning to read English. Since then the greater part of it has been rewritten in order to adapt it to the necessities of European children. As far as possible the earlier lessons are written in words of one syllable, so that they are well fitted for the use of those for whom they are intended.

The object of the work is to furnish the readers with useful information concerning the things around them, in place of the usual childish stories contained in the first books of English; at the same time presenting only such facts as ought, according to the good Bishop, and we quite agree with him, to be known by everyone. We venture to think that in this respect the native students under Bishop Colenso's care were much better off, having these lessons in their possession, than the boys and girls of our own schools who were learning English at the same time.

By far the greater part of the book is devoted to astronomy, to which subject it forms really an admirable introduction. This of course necessitates the introduction and explanation of many geometrical and optical terms, all of which are put forth in the best possible way. The physical features, and orbital and apparent motions of all the members of our system, including comets and meteorites, are fully considered, as are also the apparent motions of the stars.

The reasons are also given why the observed place of a heavenly body should be corrected for refraction, parallax, aberration, precession, and nutation. Kepler's laws and the law of gravitation also come in for a fair share of attention.

Some of the figures should be brought up to date. We are told that the earth is 96,000,000 miles from the sun, and that between forty and fifty minor planets are known; whereas the distance of the sun is between 92,000,000 and 93,000,000 miles, and no less than 268 minor planets are now on our lists.

It is to be regretted that books of this kind, written in clear, simple language, are not more appreciated by those responsible for the selection of reading-books for our elementary schools.

LETTERS 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. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

On the Constant P in Observations of Terrestrial Magnetism.

IN regard to the quantity P, depending on the distribution of magnetism in a pair of magnets employed for measuring terrestrial horizontal force, for the calculation of which Prof. Harkness, in NATURE for August 18, p. 366, gives a simplified expression, may I be allowed to mention that a yet more simple modification of the ordinary formula has been used in the Greenwich observations since the year 1878, in which, the difference between A and A_1 being small,

$$P = (\log A - \log A_1) \times \frac{r_1^2 r^2}{(r_1^2 - r^2) \text{ modulus}}.$$

When the two distances employed are, as is usual, always the same, the factor becomes a constant, being, for $r = 1{\cdot}0$ foot and $r_1 = 1{\cdot}3$ foot, $= 5{\cdot}64$. The advantage of the form is that as, in the calculation, the logarithms of A and A_1 are first arrived at, their difference multiplied by 5·64 at once gives P. Mention of this is made in the introduction to the Greenwich Magnetical Observations for 1878, and in those for some following years, although omitted from the more recent volumes.

WILLIAM ELLIS.

Royal Observatory, Greenwich, August 24.

The Svastika on English Walls.—The Solar Eclipse of August 19.

I GREATLY fear that practical builders will be uncourteous enough to smile at Mrs. Murray-Aynsley's idea (NATURE, August 18, p. 364) that the S-shaped iron bars seen on the walls of houses are fire-emblems or survivals of sun-worship. They are common enough in every county of England and elsewhere; in fact, wherever the scamping of jerry builders or the lapse of time has caused walls to give way or bulge outwards. The bolt in the centre is not merely to hang them up, but is the end of a long and strong iron bar passing right through the building and attached to a similar curved brace on the other side, or at any rate fixed to some firm unyielding part of the masonry. The curved shape is simply chosen as that which embraces and gives support to the greatest area of brick or stone surface without the necessity of having a solid, continuous plate.

A propos of sun-worship, it is sad to reflect how much good a little of it might have done in inducing that august but capricious luminary to show himself to the thousands who looked in vain for him on the morning of the recent eclipse. He seems to have shone in splendour in longitudes east of the Urals, where his worshippers abound, but to have hid himself in anger from nearly the whole of unbelieving, scientific Europe.

At Twer, between St. Petersburg and Moscow, where I was myself, the early dawn was beautifully clear; but first a dense ground-mist enveloped us, and then, when enough wind sprang up to clear this away and give us a glimpse of the sun about six-tenths eclipsed, a heavy bank of rain clouds came up and put an end to all hopes of observation. The commencement of totality was pretty well marked by a sudden intense gloom, not, however, greater than (if even as great as) a London fog.

At Berlin placards were extensively posted up a little later in the day stating that "in consequence of the unfavourable weather the eclipse was postponed until the next day." This might have been believed in France or Ireland, but it is harder to take in the Teuton than the Celt. H. G. MADAN.

Eton College.

Large Meteors.

A PEAR-SHAPED fireball, rivalling Venus in brilliancy, passed over Cardigan and Radnor in Wales on August 21 at 11h. 2m. It was observed by Mr. D. Booth at Leeds, and by the writer at Bristol; but the two paths, though likely to be very accurate as regards the *direction* of flight, are somewhat discordant in the beginning and end points. The radiant of the fireball was at 264° + 61° in Draco, and agrees with the two following showers:—

1871 August 20–25	... ...	264° + 64°	Tupman.
1887 August 14–23	... ...	264 + 62	Denning.

The meteor referred to appears to have been observed at Bristol much earlier in its track and when considerably higher in the atmosphere than when noticed at Leeds. The mean of the two places gives a height of 80 miles over a point 6 miles east of Aberystwith to 45 miles above a place 7 miles west of Rhayadergwy. The earth point was near Hay, Herefordshire.

It would be important to hear if this fine meteor was observed at any stations in the Midlands, in Wales, or on the south-east coast of Ireland. As seen from Leeds it passed through *Scutum Sobieski*, and at Bristol close to the star *ι Draconis*.

Another fine meteor about equal to Venus was observed here on August 30 at 14h. 25m. It left a bright streak in its path of 18° from 19° + 27° to 5° + 14°. Radiant at 46° + 43° near *β* Persei. W. F. DENNING.

Bishopston, Bristol, August 31.

Colliery Explosions and Atmospheric Pressure.

THERE are few questions so much in need of a satisfactory solution as the relationship which exists between colliery explosions and changes of atmospheric pressure. Before anything was known of the weight, and variations in the weight, of the air, before the barometer was discovered, miners had learned to connect the state of their working-places with weather changes. The old pits were very shallow, the workings very limited, and the ventilation practically left to take care of itself, so that it is not difficult for us to understand the effect of temperature rather than pressure on the atmosphere of the mine. "Trefoil damp," "pease bloom damp," &c., sufficiently indicate the summer prevalence of the danger; in winter "the damps were scarcely felt or heard of." In the early part of the present century Mr. John Buddle, the Newcastle viewer, having watched his barometer and the mining reports became strongly of opinion that "accidents from fire-damp always occur with a low barometer." Faraday and Lyell's Report on the Haswell disaster of 1844 dwelt upon the importance of officials taking into account the variations of the barometer in the management of mines. Since then numerous public Commissions and private inquirers, English and foreign, have investigated the connexion supposed to exist between the exudation of gas and a *falling* barometer. The earlier decisions may be said to favour Mr. Buddle's opinion, but of late years there appears to be a tendency to declare that the effect of a low or falling barometer has been considerably over-rated—that in reality it has little or no influence. Under whatever conditions of pressure explosions formerly occurred, it is perfectly clear from the experience of recent years that disasters take place, as a rule, when there is an excess and not when there is a deficiency of pressure.

Mr. Dobson's Report to the British Association in 1855 showed from a large, though imperfect, number of observations that up to the year 1854 accidents from fire-damp were most frequent in the summer months June and July, the minimum at the end of January; the results being taken to prove indisputably the general dependence of explosions upon the seasons of the year.

In the papers communicated to the Royal and to the Meteorological Societies between 1872 and 1874 by Messrs. Scott and Galloway, it was however shown from 1369 accidents in twenty consecutive years that the maximum occurred at the end of January, the minimum in the middle of September.

These very different results may be regarded as indicating the great revolution which has taken place not only in the time at which explosions occur, but also in the conditions of mining operations. Pits are now of enormous depths, with most extensive galleries, and the ventilating appliances are of the most elaborate description. Possibly these changes have modified very greatly the effect of weather variations. It must be remembered that gas exists in mines under two quite distinct conditions, that in the goaves and waste places being free and in direct contact with the air, while the gas occluded in the solid coal or imprisoned in faults is not in direct contact with the atmosphere. In the former case it is generally agreed that the accumulations of gas expand or contract with the changes of atmospheric pressure. In the latter case we know that the gas exists in the coal at a pressure of many atmospheres, so that it is highly improbable that it is affected directly by the rise and fall of the barometer. Indirectly, however, it would seem that a very important effect results, but in direct opposition to the idea that it escapes only with a falling barometer.

Serious explosions are almost exclusively confined to deep mines, where the management is perfect, and where every care is taken to insure safety. Mystery surrounds each disaster, and it is left to individuals to trace them to coal-dust, gas, or some other favourite theory. Fortunately the illiterate manager has given way to a different order of men, and from the interest taken by mining engineers there is reason to believe that much of the uncertainty which at present envelops the question will be removed before long. Barometers are now common to all mines, and they are studied with more or less interest by the officials. For years past it has become clear to them that there is no apparent connexion between the escape of gas and a falling barometer: the firemen "in ordinary cases can forestall the barometer by from twelve to twenty-four hours." This conclusion, based upon the ordinary observations of officials during their daily routine of duty, has been confirmed by more precise and carefully-planned systems of collecting information.

Following the Seaham disaster of September 1880 (when the centre of an anticyclone was over the northern counties), Mr. Corbett arranged hourly observations, day and night, for several months, showing the atmospheric pressure, the measurements of gas which had escaped into the workings, and by means of water-gauges the movements of the gas in parts of the workings sealed from contact with the air. The water-gauges indicated an out-bye pressure as much as 33, 35, 41, and 48 hours before the barometers began to fall, while gas in measurable quantities was to be found many hours before the mercury gave signs of falling. On the Continent somewhat similar observations have been made at Saarbrück and Karwin. The Austrian inquiry showed that "where after a rapid rise of the barometer it continued to rise slightly, or remained stationary for some time at its maximum, a gradual increase in the volume of gas in the air would set in; or if, after a rapid fall in the barometer, it continued to fall gradually, or remained stationary at its lowest point, a decrease in the quantity of gas would become apparent."

Evidently, therefore, from these researches the greatest danger is not, as a rule, to be apprehended when the barometer is low or falling, and this is supported by actual disasters, the majority taking place under anticyclonic conditions of pressure. While Mardy, Pendlebury, Penygraig, Seaham, and many others add to the verdict, it will suffice to deal with some explosions of the present year, and see if they do not bring home to us a new view of the natural forces at work far down below the surface of the earth.

From the simultaneous observations made at 6 p.m. on Friday, February 18, the Meteorological Office reported:—"The barometer is now rising in all parts of the United Kingdom, and an anticyclone is apparently advancing from the westward." An hour later thirty-nine lives were lost in an explosion in the Rhondda Valley. The anticyclone continued on its course to the Continent, and by the morning of Wednesday, February 23, when so much damage was wrought by the earthquake, the centre had reached Southern Europe. On March 1 the anticyclone was a little further north, and over the neighbourhood of the Chatelus Mine, near St. Etienne, where ninety lives were sacrificed. Still moving northward, the night of March 4–5 found the highest barometer readings over Belgium and the Netherlands, when 144 miners perished at Quaregnon, near Mons.

In the last week of May another anticyclone moving from south to north was marked by the loss of one life at Darcy Lever on the 25th, three lives near Wigan on the 26th, and seventy lives at Udston, near Glasgow, on the 28th.

An anticyclone over Western Germany on the night of June 7–8 marked about sixty deaths at Gelsenkirchen. As this area moved to the westward, a slight earthquake was felt near Strasburg on the 11th, and a severe one in La Vendée on the 15th.

Clearly Mr. Buddle's strong opinion is not applicable to the second half of the century. The knowledge that gas is found escaping with a rising barometer, and that so many explosions take place as indicated, has led mining officials to blame the mercury for not falling even before the gas begins to escape, their idea being that pressure has actually decreased, but that barometers are many hours before taking up the changes. The idea may be dismissed as an erroneous one. The cause must be sought for in another direction, not the direct action of variations of atmospheric pressure on the gas as it leaves the coal, but the effect on the earth's crust and indirectly on the occluded gas. Whatever be the true cause of earthquakes, there seems to be no reason to doubt that fluctuations of atmospheric pressure cause undulations of the earth's crust. Prof. Darwin, taking a probable estimate for the elasticity of rocks, has calculated that with a range of two inches of the barometer we are at least three or four inches nearer the earth's centre when the instrument stands very high than when it is very low, and concludes: "It may be that the incessant straining and unstraining of the earth's surface is partly the cause of earth-tremors, and we can at least understand that these strains may well play the part of the trigger for precipitating the explosion of the internal seismic forces." The seismological records of Japan show that earthquake shocks are twice as numerous under the predominant anticyclone of the winter months, as they are in the summer with lower pressure. As a result of the discussion of earthquakes in Jamaica, Mr. Maxwell Hall concludes that "at the time of an average earthquake shock the barometer is a little above its average height. This is due to the circumstance that the winter months, December, January, and February, when the barometer is above its monthly average, are more liable to shocks than other months of the year; and that the hours from 8 p.m. to 2 a.m., when the barometer is above its diurnal average, are similarly more liable to shocks than other hours of the day." Explosions of fire-damp follow a similar rule; they are most numerous in the winter months, when the range of pressure is greatest, and usually when the barometer is very high. Allowing for the flexure of the earth's surface, we can conceive that with the downward movement under increasing pressure the pent-up gases are forced into the workings of our deep mines; it may be indeed these movements cause infinitesimal fissures in the coal-seams through which the gas passes into the workings at a time when it has been customary to believe there was least danger. There is some degree of probability in this from the fact, so frequently noted in great explosions, that there is a suddenness in the appearance of the gas which is not a common experience in shallow workings.

Taking into consideration all the recorded facts, they point to the conclusion that far greater weight should be attached to a period of high atmospheric pressure than has hitherto been deemed necessary. In any future discussion of this important subject it is to be hoped further evidence will be forthcoming, and that instead of endeavouring to connect every disaster with a low barometer, the distribution of pressure as a whole be taken into account.

The influence of coal-dust upon explosions has not been touched upon, but it may be remarked that the dry atmosphere of an anticyclone renders the dust more inflammable than the dampness of a low-pressure system, so that there is a double reason for giving closer attention to mines under anticyclonic conditions.

HY. HARRIES.

MEASUREMENT OF SPECIFIC HEAT.

HAVING regard to the comparatively large experimental error introduced by thermometers into specific heat measurements, a null method appeared to be desirable. The following method occurred to me about two months ago, but not having access to a physical laboratory, I have not been able to practically test its accuracy.

Two exactly similar calorimeters (A and B) are taken, each containing a coil of thin Pt wire of resistance R, so arranged as to be completely immersed in the liquid. A contains a mass, M (including water equivalent), of water; B the same mass of substance the specific heat of which is being measured. The wires are arranged in bridge fashion, so that the ratio of the currents flowing through the two wires may be made to take any value.

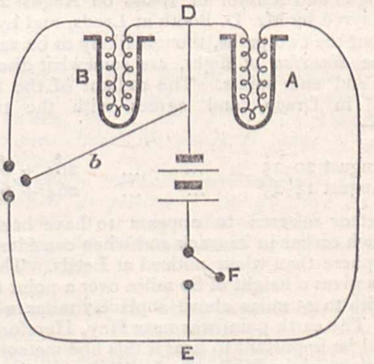

FIG. 1.

A differential thermometer (not indicated in the sketch, for sake of clearness) shows the least difference of temperature between A and B. Probably the most delicate and convenient arrangement is to use two thin Pt wires balanced in the arms of a bridge, using a very sensitive galvanometer.

First consider the calorimeter B containing the substance. It receives a quantity of heat, H, from a current, C, flowing through a resistance, R, for a time, *t*. Hence

$$H = \frac{C^2Rt}{J} = \theta MS$$

(where θ is the rise of temperature, and S the mean specific heat for that interval).

Similarly in A, containing water,

$$H^1 = \frac{C_1^2Rt}{J} = \theta M.$$

For by the conditions obtained θ is the same in both A and B. Hence $S = \left(\frac{C}{C_1}\right)^2 = \left(\frac{r_1}{r}\right)^2$, where r_1 and r are the resistances of the two circuits. It is obviously unnecessary to make the resistances, and the masses of liquids, equal, but the equation is thus simplified. If a smaller mass of water, m, be taken, then $S = \frac{m}{M} \cdot \left(\frac{r_1}{r}\right)^2$, thus increasing the delicacy of the method.

Since in the adjustments a considerable amount of time would be necessary to allow the calorimeters to attain thermal equilibrium after each trial, the following modification may prove more simple and more practical:—

The calorimeter B is arranged so that by a switch-key, C, the current can be diverted through a wire of exactly equal resistance, b, so that the current is the same by either path. The resistance from D to E is the same either way. The key F is pressed down for a time, t,

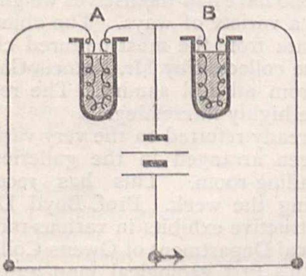

FIG. 2.

until the needle is largely deflected; then the current is switched from B and passed through A alone, until the needle is just brought back to zero, in total time, T. Then, neglecting for the present the slight error due to cooling,

$$\text{in A} \ldots \theta M = \frac{C^2RT}{J}, \text{ in B} \ldots \theta MS = \frac{C^2Rt}{J}, \therefore S = \frac{t}{T}.$$

Since T and t can both be made large, this should give very accurate results. It is evidently especially applicable to the measurement of the rate of increase of specific heat with temperature, since the liquids may have any initial temperature.

In conclusion, I may say that I should not have published this method in such an incomplete state, and unsupported by experiment, but I noticed to-day (September 5) that Profs. Stroud and Gee intend to read a paper before the British Association on "A Null Method in Electro-Calorimetry," and it is possible this may refer to a similar method.

GEORGE N. HUNTLY.

THE HESSIAN FLY.

I AM sorry to say that reports from correspondents acquainted with the attack of the Hessian fly show its presence now in an almost continuous line along the northern and eastern coast from Cromarty on the Moray Firth in Scotland down to Kent.

I have this morning received specimens of the *puparia* from the parish of Urquhart, in Morayshire, the most northerly locality from which I have at present received the so-called "flax-seeds."

The amount of presence varies very much. In the locality above mentioned (that is, the district from Aberdeen to Cromarty), the traces of attack are reported as to be found from 25 to 30 miles inland, but the injury slight, not more than one straw in fifty being affected, and the grain of fair quality. It is severe in some parts of Perthshire, and is found also in the eastern counties adjacent.

In East Lothian, Haddington, and Berwickshire attack is only reported from a few places at present, and in Northumberland from one locality.

Beginning again on the two sides of the Humber the attack widens much in area as it is traced south. It passes through Lincolnshire and Cambridgeshire, touching an easterly part of Northamptonshire, till it extends over the district commonly known as the eastern counties, including besides great attack in Hertfordshire, and some in Bedfordshire; and it also occurs in Kent.

In the southerly or westerly parts of England it occurs at Lymington and Petersfield in Hampshire, and to a considerable extent near the College of Agriculture, Downton, near Salisbury; and I have one report of it from near Bridgwater, and it also occurs at Goring Heath, Oxfordshire.

The above localities are where I know of its presence from specimens sent to myself, or, in a few cases, from information given me by correspondents whom I know to be acquainted with the appearance of the *puparium*, and the characteristics of the attack.

It very likely may occur elsewhere, but I am only just giving a general sketch of extent of infested area from personal knowledge.

It strikes me as a very curious point that the attack should so markedly cling to the sea-side, excepting in a few isolated instances, or where the inland area is continuous with the sea-side district.

It is very satisfactory to observe that although the season has been so altogether *extraordinarily* favourable to various kinds of insects affecting corn-stems, yet that in very many instances reported to me the injury caused to wheat by Hessian fly has been slight

On this fact I venture to think we may ground a hope that, either from the varieties of wheat which we use being kinds suited to do what is called "resist" attack, or from circumstances of our cultivation, we may find that our wheat at least does not suffer as much as in some other countries.

Also the enormous prevalence of the two stem attacks caused respectively by the corn sawfly (*Cephus pygmæus*), and by the dipterous fly, the *Chlorops tæniopus* (attacks which far exceed in amount any which have been brought under my notice as caused by these insects), give a hope that the climatal circumstances which usually prevail here will have an effect in checking the attack of the *Cecidomyia destructor*, as well as the above-named crop pests, as we see that all three kinds have been exceptionally thriving in the exceptional heat and drought.

It is unnecessary to point out to your highly informed and thinking readers that the statements now appearing of the *Cecidomyia destructor* having been a corn pest in this country for many years have not the slightest foundation.

ELEANOR A. ORMEROD.

THE BRITISH ASSOCIATION.

MANCHESTER, *Tuesday Evening.*

ABOUT the success of the Manchester meeting there seems to be only one opinion. In mere numbers—the most popular gauge of success—it has by several hundreds surpassed all former meetings; the number of tickets sold very closely approaches 4000. As a natural result, the amount of money collected and available for the purposes of research is unprecedentedly great, as will be seen by the list of grants which have been allotted to the various Committees. The great increase in attendance over all former years is to a considerable extent due to the large number of foreign visitors, who have formed a marked and prominent feature of the present meeting. In the proceedings of nearly every Section the representatives of foreign science have taken an active part, with the result that the time of the whole meeting has been more intensely scientific than in the case of any previous meeting. This has been especially shown in the case of the important discussions which had been arranged for, and which most of them bore the character of real debate; the only exception, we believe, being the case of electrolysis, in Sections A and B, the "discussion" consisting mostly of the reading of a series of papers. Quite otherwise, however, was it with the discussions on heredity, introduced by Prof. Lankester, and on the cell theory, introduced by Prof. Schäfer, in Section A—discussions in which the subjects were threshed out very thoroughly. To some extent it is generally conceded that the great mixture of foreigners has to some extent solved the problem of an International Scientific Congress, which in any formal way is generally considered impracticable. Their presence here has certainly added a stimulating variety to the meeting, and the honour has been duly appreciated by the Corporation and citizens of Manchester. The foreigners have all been hospitably entertained as guests, and there have been not a few special entertainments got up for their special behoof. At the great dinner to be given to-morrow by the Mayor and Corporation nearly half of the guests will be foreigners. One of the pleasantest gatherings of the meeting was at a little dinner given on Sunday night by a few of the biologists to a select few of their foreign co-workers, especially botanists, at which De Bary delighted everybody present.

The number of papers read at this meeting has been quite comparable with its other exceptional features. Sections that have never split before have been compelled to split now. Biology, though it has thrown off Section H, has this year split into two sub-sections,—Botany and Physiology,—and there is even some fear, perhaps hope, that these divisions may become permanent. On Saturday every Section met except E, and to-morrow the majority will have to sit close up to the General Committee-meeting. Out of all this rush of papers no doubt some good comes, but most of those interested in the welfare of the Association admit that it would be well to moderate it, or perhaps still more completely to organize it. For one thing the custom of reading one paper in several Sections is greatly to be deprecated, and this year it has been carried still further than ever, greatly to the indignation of those Sections which had to submit to hearing the story retold. This came to a crisis in Section E, where an eminent geologist, who condescended to read a paper to some extent already given to his own Section, was told in almost so many words that Section E had no time to listen to geological lectures. Here indeed the battle between the geologists and geographers was fought out, greatly it was thought to the discomfiture of the former, who are loth to think that there is anything worthy of the name of geography outside of their own lines.

In spite of the persistently unfavourable weather, the public lectures have been quite successful. The lecture on the rate of explosions by Prof. H. B. Dixon kept a large audience intensely interested from beginning to end; and nothing could be more striking and instructive than his experiments, some of which were on a gigantic scale. Equally attractive was the lecture to working men on Saturday evening on electricity by Prof. George Forbes. The biggest audience of any, however, assembled in the Free Trade Hall on Monday evening to listen to Sir Francis De Winton's lecture on exploration in Central Africa. The audience was evidently a popular one, and the lecturer had the warmest reception. Unfortunately, the lantern used to show maps and pictures on the screen was rather a failure. Distinctly popular as it was, probably even the specialists were glad to get a convenient summary of recent work in Central Africa, pleasantly conveyed.

The address of the President of the Association, Sir Henry Roscoe, was very numerously attended. Sir Henry was evidently audible all over the place, and his reception, as might have been expected, was enthusiastic.

Manchester is rather badly off for excursion places, and on Saturday, we believe, quite as many people spent the day in Manchester as elsewhere. Indeed, most of the Sections were so busy with work that they had no time to think of play. The little dredging excursion was joined in by about fifty men, who all seemed highly satisfied with the results, in spite of the weather. One of the most popular of the coming excursions will be that to the Isle of Man from Friday to Tuesday, under the guidance of Prof. Boyd Dawkins. There is also some talk of an excursion to the Lakes, but the weather does not encourage holiday enterprise in so notoriously rainy a region.

One popular and distinctly useful feature in connexion with the present meeting has been the Anthropometric Laboratory which has been established in connexion with Section H, under the care of Dr. Garson and Mr. Bloxam. It has been very largely frequented by the members of the Association, who have had themselves weighed, measured, and tested in a variety of ways. The object, we believe, is to obtain data from the most cultured classes to compare with those collected by Mr. Francis Galton, at South Kensington, from all and sundry. The result, it is expected, will be highly interesting.

We have already referred to the very varied exhibition which has been arranged in the galleries around the attractive reading-room. This has received various additions during the week. Prof. Boyd Dawkins shows some very instructive exhibits in various rooms belonging to the Geological Department of Owens College, including William Smith's first geological map and an autograph letter. Another exhibit deserving mention is the collection of wax models, illustrating vertebrate morphology and embryology, shown by Prof. His, of Leipzig, on behalf of Dr. A. Ziegler, of Freiburg. Naturally the model of the Manchester Ship Canal has attracted much attention, but not more than the great variety of interesting anthropological exhibits, which include the collection of casts and photographs from Egyptian monuments contributed by Mr. Flinders Petrie.

Great complaints have been made of the way in which the Press has reported the proceedings of the meeting. This may partly be due, no doubt, to the fact that Parliament is still sitting and takes up much of the space of the papers; but is also to be ascribed to a larger extent to the fact that ordinary reporters are hardly equal to the work of the British Association. When there are printed abstracts the matter is simple enough, but when discussions have to be reported the failure is almost absolute. This is certainly to be regretted, as it could not but be of the greatest service to have such discussions widely circulated. Surely it is quite worth while for the Sections to organize adequate reporting arrangements for their own sake.

The next meeting, at Bath, will be presided over by Sir Frederick Bramwell. In 1889 the Association will meet in Newcastle, and it is expected that an invitation will come from Leeds for 1890.

The following is the list of grants which have been made this year by the Association:—

A.—*Mathematics and Physics.*

Ben Nevis Observatory	£150
Electrical Standards	80
Magnetic Observations	15
Standards of Light	100
Electrolysis	50
Solar Radiation	10
Differential Gravity Meter	10
Uniform Nomenclature in Mechanics	10

B.—*Chemistry.*

Silent Discharge	10
Properties of Solutions	25
Recording Water Analysis Results	10
Influence of Silicon on Steel	20
Methods for Teaching Chemistry	10
Isomeric Naphthalene Derivatives	25
Action of Light on Hydro-acids	20

C.—*Geology.*

Sea Beach near Bridlington	20
Geological Record	50
"Manure" Gravels of Wexford	10
Erosion of Sea Coasts	15
Erratic Blocks	10
Underground Waters	5
Palæontographical Society	50
Volcanic Phenomena of Japan	50
Pliocene Fauna of St. Erth	50
Carboniferous Flora of Lancashire and West Yorkshire ...	25
Volcanic Phenomena of Vesuvius	20

D.—*Biology.*

Zoology and Botany of the West Indies	100
Flora of Bahamas	100
Development of Fishes, St. Andrews	100
Marine Laboratory, Plymouth	100
Migration of Birds	30
Flora of China	75
Naples Zoological Station	100
Physiology of the Lymphatic System	25
Marine Station at Granton	50
Peradenyia Botanical Station	50
Development of Teleostei	15

E.—*Geography.*

Depth of Frozen Soil...	5

F.—*Economic Science and Statistics.*

Precious Metals in Circulation	20
Value of the Monetary Standard	10

G.—*Mechanics.*

Investigation of Estuaries by Models	200

H.—*Anthropology.*

Effect of Occupations on Development	25
North-Western Tribes of Canada	100
Prehistoric Race in the Greek Islands	20
Anthropological Notes and Queries	50
Total	£2025

Captain Sir Douglas Galton (one of the general secretaries) submitted the following report of the Council to the General Committee at the meeting held on the 31st ult.:—

The Council have received reports during the past year from the general treasurer, and his account for the year will be laid before the General Committee this day. Since the meeting at Birmingham the following have been elected corresponding members of the Association: Dr. Finsch, Dr. O. W. Huntington, Dr. A. König, Lieut. R. Kund, Prof. Leeds, Prof. H. Carvill Lewis, Prof. John Trowbridge. The Council have nominated Mr. Oliver Heywood a vice-president of the meeting at Manchester. An invitation for the year 1889 will be presented from Newcastle-upon-Tyne; but the invitations from Melbourne and Sydney have been withdrawn. The following resolutions were referred by the General Committee to the Council for consideration, and action if desirable:—(*a*) "That the Council be requested to consider the question of rendering the reports and other papers communicated to the Association more readily accessible to the members and others by issuing a limited number of them in separate form or in associated parts, in advance of the annual volume." The Council, after careful consideration of the question, are of opinion that a certain number of copies of the more important reports presented to the Sections of the Association should be kept in stock and sold separately, the number of copies printed and the price of each report to be fixed by the secretaries after communication with the officers of the several Sections. (*b*) "That the Council be requested to consider the advisability of selling publicly the presidential addresses." The Council have considered the question, and are of opinion that it is desirable that printed copies of the addresses of the president and the presidents of Sections should be stitched together and sold; that a number of copies, not exceeding 1000, should be printed; and that these should be placed on sale, at the price of one shilling, through agents or otherwise, as may be considered most suitable. (*c*) "That the Council be requested to consider the advisability of calling the attention of the proprietor of Stonehenge to the danger in which several of the stones are at the present time from the burrowing of rabbits, and also to the desirability of removing the wooden props which support the horizontal stone of one of the trilithons; and in view of the great value of Stonehenge as an ancient national monument, to express the hope of the Association that some steps will be taken to remedy these sources of danger to the stones." The Council have carefully considered the question, and having had the advantage of perusing the detailed report recently prepared by a deputation of the Wilts Archæological and Natural History Society on the condition of the whole of the stones constituting Stonehenge, are of opinion that the proprietor should be approached with the expression of a hope that he will direct such steps to be taken as shall effectually prevent further damage. (*d*) "That the Council be requested to consider whether a memorial should be presented to Her Majesty's Government, urging them to undertake and supervise agricultural experiments, and to procure further and more complete agricultural statistics." The Council have considered the question, and are not prepared to memorialize the Government on the subject. The question of the re-arrangement of the journal has been brought before the Council by Mr. J. B. Martin, and, after careful consideration, the Council are of opinion that it is unnecessary to print in each number of the journal the list of the papers read on the previous day; also that it would be well to place the list of officers of each Section at the head of the list of papers to be read in that Section. The Council wish to obtain the sanction of the General Committee to these alterations. The Council having considered a letter addressed to them by Mr. R. H. Scott, are of opinion that it should be an instruction to the secretaries of all committees, other than committees of Sections, to send notices of all meetings to each member of a committee, and that the draft report of the committee should first be sent in proof to each member, and then submitted to a meeting of the committee specially called for the purpose. The Corresponding Societies Committee, consisting of Mr. F. Galton (chairman), Prof. A. W. Williamson, Sir Douglas Galton, Prof. Boyd Dawkins, Sir Rawson Rawson, Dr. J. G. Garson, Dr. J. Evans, Mr. J. Hopkinson, Prof. R. Meldola (secretary), Mr. W. Whitaker, Mr. G. J. Symons, and General Pitt-Rivers, having by an oversight not been re-appointed at Birmingham last year, the Council have requested these gentlemen to continue the work of their committee, and now nominate them for re-election, with the addition of the names of Mr. W. Topley, Mr. H. G. Fordham, and Mr. William White. In accordance with the regulations the five retiring members of the Council will be Mr. W. Pengelly, Sir Richard Temple, Dr. De La Rue, Sir F. J. Bramwell, and Mr. J. C. Hawkshaw.

The Council recommend the re-election of the other ordinary members of the Council, with the addition of the gentlemen whose names are distinguished by an asterisk in the following list:—Capt. W. de W. Abney, F.R.S., Sir R. S. Ball, F.R.S., W. H. Barlow, F.R.S., W. T. Blanford, F.R.S., W. Crookes, F.R.S., Prof. G. H. Darwin, F.R.S., Prof. W. Boyd Dawkins, F.R.S., Prof. J. Dewar, F.R.S., *Sir James Douglass, F.R.S., Prof. W. H. Flower, F.R.S., Dr. J. H. Gladstone, F.R.S., Lieutenant Colonel H. H. Godwin-Austen, F.R.S., Prof. O. Henrici, F.R.S., Prof. J. W. Judd, F.R.S., J. B. Martin, F.S.S., Prof. H. McLeod, F.R.S., Prof. H. N. Moseley, F.R.S., Admiral Sir E. Ommanney, C.B., F.R.S., Prof. W. C. Roberts-Austen, F.R.S., *Prof. Schuster, F.R.S., *Prof. H. Sidgwick, *Prof. Schäfer, F.R.S., W. T. Thiselton-Dyer, C.M.G., F.R.S., Prof. T. E. Thorpe, F.R.S., *H. Woodward, F.R.S.

Sir H. E. Roscoe, M.P., moved, and Sir R. W. Rawson seconded, the adoption of the report. The motion was adopted.

The Chairman submitted the treasurer's report, which stated that the receipts for the past year were £5081 6*s.* 3*d.*, including a balance of £1869 brought forward at the Birmingham meeting. The disbursements included the sum of £1186 18*s.* in respect of grants in aid of scientific research. The balance in hand was £1718. The report was adopted.

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY EDWARD SCHUNCK, PH.D., F.R.S., F.C.S., PRESIDENT OF THE SECTION.

IT is, I can assure you, with a feeling of extreme diffidence that I take the chair to-day as President of the Chemical Section at this meeting of the British Association. When I look round me and see the many distinguished men who are prepared to take part in our proceedings I cannot but very strongly feel that the Council's choice might have fallen on a worthier representative of chemical science than myself. Having in the course of my career bestowed more time and attention to technical matters than to purely scientific subjects, and having moreover arrived at a time of life when active participation in work of any kind must necessarily be drawing to a close, you must not expect from me the accurate knowledge of the present state of chemical science and the questions that are at this moment presenting themselves for solution such as would naturally be required from anyone occupying the post which I have on this occasion the honour to hold. The marvellously rapid progress of chemistry during the last twenty years has made it difficult for the most industrious cultivator of the science to keep abreast of the knowledge of the day, and for a *dilettante* like myself one may say it is next to impossible. I confess myself painfully conscious of my defects in this respect, and I shall therefore have to claim the indulgence of the Section should questions arise on which I am unable to speak with authority, or even to discuss with advantage.

Considering, however, how efficiently I am supported by the gentlemen with whom I have the honour to be associated, and to whom I am sure in any case of difficulty I may appeal for assistance, I trust to be able to perform the duties of my office without discredit. I will not, however, trouble you with merely personal questions, which are always more or less tedious, but proceed with the few remarks which I wish to make, and which, if not new or instructive, may perhaps serve to entertain you during the time usually devoted to addresses of this kind.

I think you will hardly expect me, even were I fully competent to do so, to review the progress of chemistry during the last half-century, for the time at my disposal would be too short and the result at my hands, I fear, unsatisfactory. I shall prefer to call attention in a few words to the chemistry of other days as I knew it, and the chemistry of the present time as known to us all, and to point out what I consider to be the chief characteristics of each. I shall then, with your permission, point out a few of the directions in which, in my opinion, the chemistry of the future will probably be developed, and in this undertaking I shall perhaps be more successful than in the other; for to discuss the history of science requires exact knowledge; but in speculating on its future the imagination comes into play, and to imagine is easier than to describe.

When I first entered on my studies, exactly fifty years ago, chemistry could hardly be called a science—it was rather a collection of isolated facts unconnected by any consistent theory covering the whole field. Most of the important elements were known, but of the exact proportions in which they combine together we were ignorant. The law of definite proportion had been generally accepted, but so imperfect were the data then at our disposal that we may say the law was rather taken for granted than proved. The atomic theory of Dalton as explaining this law had also been adopted by chemists; but it is not unlikely that this theory, then in its infancy, might by the vigorous onslaught of a man of Berthollet's acumen have been upset, and we should then have been left entirely without a guide through the bewildering labyrinth of facts. Of any connexion between chemistry and physics there was in those days no question; of any but the most superficial notions regarding the effects of heat, light, and electricity on chemical substances we had no conception. The idea that chemistry could have any bearing on or connexion with physiology or pathology would have been ridiculed as absurd. I cannot think of the then state of organic chemistry without feeling amused. The state of this branch of chemistry could hardly perhaps be called chaotic or rudimentary, for, after all, what had been done had been well done and neatly done, but the assemblage of facts of which it consisted was devoid of systematic arrangement; it resembled a cabinet of curiosities, the components of which stand in no recognizable relation to one another, or a miscellaneous collection of books placed in an orderly manner on shelves, but without any attempt at classification. As to the genesis of organic compounds, what would now be called absurd notions prevailed. I distinctly remember eminent chemists maintaining that no strictly speaking organic body, even of the simplest constitution, could possibly be formed without the intervention of the so-called vital force. The fact, then recently discovered by Wöhler, of the artificial formation of urea from inorganic substances, was considered as something almost miraculous—*i.e.* as a phenomenon the like of which would never again recur. Without, however, entering into further details, I think I may, without fear of contradiction, assert that the main distinction between the chemistry of fifty years ago and the chemistry of the present day consists in this, that whereas formerly the science dealt chiefly with qualitative reactions, it now occupies itself principally with quantitative determinations. To have established the fact that every chemical phenomenon may be represented in figures, denoting either number, measure, or weight, such figures, when once accurately determined, remaining constant and unchanged through all time—this seems to me the crowning glory of modern chemistry. It is the firm establishment of this principle that has transformed the face of chemistry and has changed it from a mere descriptive into an exact science.

In justice to our predecessors it should, however, be remembered that this principle, though more fully developed in our own day, was not for the first time set up in quite recent times. The labours of Dalton, conducted on quantitative lines, were performed in this city of Manchester in the early part of this century. At the same time Berzelius was engaged in analyzing the most important inorganic compounds and establishing the fact, not previously recognized, strange as it may now appear, that every well-defined substance has a definite chemical composition. But going still further back, we come to the alchemists. Now alchemy, if it has any logical basis at all, is founded on quantitative notions as regards matter. All metals, the alchemists said, consist of sulphur, salt, and mercury (these terms signifying not so much elements in the modern sense as qualities) in various proportions; hence their convertibility. Take copper, remove from it a certain proportion of its sulphurous constituent, and add more of the mercurial, and you have silver; repeat the process with silver, and gold results. At the time of which I speak, though much important analytical work had been done by Berzelius, Rose, and others in organic chemistry, though the veteran Chevreul had led the way in placing organic chemistry on a quantitative basis, and the composition of the most important organic compounds—thanks to the labours of Liebig and his method of organic analysis—had been ascertained, still quantitative determinations were not considered of such paramount importance as at present. In fact, scientific thought did not run in that direction, but satisfied itself, for the most part, with the study of qualitative reactions. It was still possible to see memoirs by eminent chemists containing not a single quantitative determination. Strange as it may seem, two able chemists, Boettger and Schoenbein, were living until quite recently who worked and obtained valuable results without resorting to the balance, the instrument which of all others seems the most indispensable to the chemist of to-day. The balance was indeed universally employed in my younger days, but no other instrument, properly so called, was ever seen in the laboratory. The spectroscope was not yet invented; the polariscope had not come into use; volumetric analysis was still in its infancy. Even the thermometer was but seldom used. What a different picture does the laboratory of the present day present, with its instruments of precision and its various appliances for effecting quantitative determinations of all kinds!

Whether the universal prevalence of, and exclusive attention to, quantitative methods in chemistry has been an unmixed good may be doubted. Who has not run with a weary eye over the long array of figures, the never-ending tables of which some modern memoirs seem to consist, and not longed for some mere description—were it only regarding trivial matters—to relieve the monotony and fix the subject treated of on the memory? That quantitative determinations given in quite precise terms may occasionally be entirely futile may be seen on referring to the history of alchemy. One of the later alchemists professes to have converted 5400 parts by weight of copper into 6552 parts of silver by the action of 1 part of a metal-improving substance—philosopher's stone (Kopp, "Die Alchemie"). Here we see the quantitative method applied to a purely chimerical process, elaborated from the depths of the experimenter's inner consciousness, and of no value whatever. Much of what is at the present day carefully worked out and presented to the world in numerical form may, like this statement of the alchemist, pass away and be forgotten. This may possibly be the case with the numerous carefully-made analyses of water which we now meet with, and which we would gladly exchange for a few decided qualitative tests of its hygienic properties. In the case of air and water it is not the minuteness of the noxious matter which causes doubts to arise, but the absence of any decided and undoubted chemical characteristics of the impurities present. It is probable that a refined sense of taste, uncorrupted by the luxurious indulgences which civilization has introduced, would be able to detect differences in drinking-water which might escape the attention of the most consummate analyst.

Whatever objections may, however, be entertained to the application of quantitative methods in natural science, to the exclusion of others, it is certain that important results have flowed from their adoption, insomuch that we seem to have arrived at the conclusion that the expression of quantitative results is the be-all and end-all of science; that all differences are merely quantitative; that there is no such thing as mere quality. The whole philosophy of our age is expressed in this one proposition: All differences within the sphere of our experience are quantitative. It is the basis of Darwinism, if I am not mistaken, and underlies many of our political and social theories. Of course it is a mere assumption if stated generally, for the phenomena that admit of purely quantitative expression are few in number compared with those that do not; but then it is surmised, and with some degree of probability, that the vast region outside the quantitative sphere will in time come to be included within it. The past history of science seems to render this likely in the future. The science of chemistry has so far, however, presented an insuperable barrier to the general adoption of this view, and will continue to do so as long as the so-called elements remain what we now admit them to be—indestructible, immutable, inconvertible. It is possible to denote all the known properties of gold and silver, their atomic weight, specific gravity, hardness, malleability, action towards heat, light, and electricity, in precise numbers with reference in each case to a certain standard; and yet we cannot say that silver minus a little of this, plus a little of that, constitutes gold—the two elements are essentially and radically distinct. Unless we admit with the alchemists that by taking away a little of A and adding a little of B we can convert one metal into another, one element into another, the quantitative method must fall short of its complete development in chemistry. Numerous attempts have, therefore, been made to show the theoretical probability, even if it should not be possible to prove it experimentally, of the so-called elements being really compound bodies, or at least of their containing a basic matter common to all. My predecessor in this chair has endeavoured to show, in the brilliant address delivered to this Section on the occasion of its last meeting, that the barrier hitherto presented to us by the intractability of our present elements may be overcome, and has adduced experimental illustrations in favour of his views of the compound nature of the elements. Mr. Crookes has called to his aid the doctrine of evolution, which has proved so valuable an instrument in the hands of the biologist, maintaining that the elements, like the species of plants and animals, were gradually evolved by some process of condensation from a primordial matter called by him protyle, each step in the process being represented by a distinct element. This is doubtless taking very safe ground, for if the process of evolution was the same in the inorganic as it is supposed to have been in the organic world, the process can never be repeated, and we shall, therefore, never be in a position to illustrate it experimentally. I may, however, have misunderstood what Mr. Crookes meant to convey, and, if so, must apologize for the dulness of my apprehension. Granting, however, the possibility of our resolving our present elements, were it in theory only, into modifications of one basic material out of which they have been evolved, the question would still remain to be answered, What has caused this primordial matter to be split up into groups and forms having distinct and opposite qualities? and when this question is answered, if it can be answered even in a problematical way, then other questions would arise, until by degrees we should arrive at the confines of physical knowledge and find ourselves in the region of metaphysics, where scientific reasoning ceases, and thinking for scientific purposes becomes unprofitable. Excursions into this region would indeed be very useful if on returning to physical regions we could every time bring back with us an instrument as potent and far-reaching as the atomic theory has proved to be, a theory which still remains the basis of all our reasoning in chemistry; but then the atomic theory has been quite an exceptional instance. Metaphysical speculation, such as the *Naturphilosophie* of the Germans has dealt in, has, generally speaking, been utterly barren in natural science.

I will not on the present occasion dwell on the vast addition made to the number of useful and beautiful substances by chemists during the last fifty years. Their number is legion, and their mere description fills volumes, whereas half a century ago a dictionary of moderate size would have sufficed for the purpose. Among these newly-discovered substances none are more remarkable than the metals rubidium, cæsium, thallium, indium, gallium, the existence of which was revealed by the spectroscope, and which, indeed, would have probably remained unknown but for the labours of Bunsen and Kirchoff in perfecting and applying that instrument.

I must not, however, omit all reference to a department of chemistry which has been, one may almost say, created within the time to which I am referring—I mean that of synthesis. When I began to study chemistry we only heard of analysis; of synthesis, so far at least as regards organic bodies, we only dreamt as a remote and unattainable region. The only instance then known of the synthesis of an organic substance was that of urea by Wöhler. Synthesis was, indeed, supposed to be an essentially vital process effected under the influence of the vital force, and quite outside the sphere of the chemist. Since then what marvels have we not seen? Alizarin and purpurin, the colouring-matters of madder, have been prepared artificially by Graebe and Liebermann, indigo by Baeyer, not to mention bodies of simpler constitution obtained by comparatively less complicated processes. We are honoured to-day by the presence of Prof. Ladenburg, who has succeeded in artificially preparing coniin, the alkaloid to which hemlock owes its poisonous properties; the first natural alkaloid, indeed, which has been obtained artificially. Looking back at what has been achieved, I think we may entertain the confident anticipation that all the most important organic bodies, acids, alkaloids, and neutral substances will, in course of time, be obtained in a similar manner, though of one thing we may be pretty sure—viz. that we shall never succeed in forming any really organized matter as distinct from organic. The term organic matter is in fact only employed for the sake of convenience, and as an expression handed down to us from former days, since so-called organic compounds are subject to the same laws with regard to composition as the bodies which we name mineral or inorganic, but organized matter such as we find constituting the vessels of plants and animals is a different thing. The protoplasm contained in the vegetable and animal cell is something very distinct from the same matter after the death of the organism, but the difference between living and dead matter is not of a chemical nature. In referring to chemical synthesis I cannot refrain from expressing regret that so little has hitherto been done in the artificial production of minerals with a view to elucidating the processes by which they were formed in Nature, but it is possible that more has been done in this direction than I am aware of, since this is a department of chemistry with which I am not familiar. It is certain that inorganic chemistry generally does not now receive the attention which it formerly did. The exclusive devotion to the chemistry of the carbon compounds which we find in most of our laboratories at the present day may, however, be accounted for when we see the brilliant results to which the study of those compounds has led.

After these few remarks on the development of chemistry during the last fifty years, of which I know a little, it may seem presumptuous on my part in the presence of some of the most eminent chemists of our day, whose opinions must be of infinitely more value than mine, to say anything about the future of our science and the direction it will probably take. Nevertheless, trusting to your kind indulgence, I will venture on some speculations in this direction, which, if they do not instruct the younger members of the Section, may serve to amuse their seniors, and at all events will refer to subjects on which some thought is well bestowed.

As regards the future of chemistry, the question has frequently suggested itself to me as it has doubtless done to others —Will chemical science go on expanding and developing during the next few generations as it has done in the course of the last hundred years? Will discovery follow discovery, and fact be added to fact, until the record occupies not a few volumes only, but a whole library? Will systematic chemistry, *i.e.* the history and description of all possible combinations of the elements, have any limits? I am inclined to answer in the negative. All human institutions pass through the same phases; they have their rise, they culminate, and decay; and I do not see why the science of chemistry should form an exception. Moreover, it is a natural law that whatever develops rapidly also declines rapidly, and the development of systematic chemistry since the commencement of this century has been perfectly unprecedented. I think it probable that in the course of time, at the rate at which we are now progressing, nearly all possible compounds will have been prepared, all the most important chemical facts will have been discovered, and pure chemistry will then be practically exhausted, and will be in the same condition as systematic botany and mineralogy now are. New compounds will now and then be discovered, just as new plants and new minerals now are, but nothing further will be brought to light that will affect the theories at which we shall then have arrived, whatever they may be. All the material with which the science has to deal having then been brought together, what will happen? Will chemical science cease? Will chemists, satisfied with past achievements, cease to work, confining themselves to practical questions and the history of the days gone by? I think not. The science will continue to develop, but in other directions than those previously pursued. The exhaustion of systematic botany has not put an end to botanical science, for vegetable physiology has opened a wide field to the botanist, one that will take a long time to explore thoroughly. To indicate the directions which chemical science will take in its various applications to other departments of knowledge, as, for instance, in connexion with the study of the physical properties of matter, or in elucidation of the chemical processes whereby minerals have been formed, or those through which geological strata have passed in bygone ages, would not be within my competency, as I should have to touch on subjects with which I am not familiar; but I may be permitted to refer by a few words to a subject, with which, by reading at least, I have become better acquainted, and which seems to me to offer a wide field to the investigator who shall come well provided with physical and chemical knowledge to its cultivation. I allude to the processes whereby the substances constituting the various organs of plants and their contents are formed, and those again to which the decomposition and decay of vegetable matter are due; a subject as to which our knowledge is quite elementary, but which, it seems to me, admits of an extension and development of which we have at present not the least conception.

De Saussure, it is well known, first discovered the fact that plants under the influence of light absorb carbonic acid and give off oxygen, the inference of course being that the carbonic acid and the water present are decomposed, the carbon of the former and the hydrogen of the latter going to form the various organic constituents of the plant, while the oxygen or a part of it is set at liberty and poured into the atmosphere. The facts as they stand are simply these: what the plant requires for its subsistence is carbonic acid, water, nitrogen in some form (presumably that of a nitrate), certain bases—potash, lime, magnesia, iron oxide, and phosphoric acid. Out of these it constructs the whole of its organic frame, its cells and their contents, rearranging the elements of which its food consists in such a manner as to convert inorganic into organic matter, *i.e.* changing bodies in which the affinities of the atoms are thoroughly satisfied into such as contain them in a state of more or less unstable equilibrium, and therefore liable to alteration when their atoms are allowed to act in accordance with their natural affinities. More than this we do not know; our ignorance of the several steps or stages of the process, if there are any such steps, is complete; all that has been added to the general statement just given is mere speculation. Yet it is impossible to remain satisfied with the present state of our knowledge on the subject. Accordingly numerous attempts have been made to bridge over the chasm which separates the inorganic and organic worlds, not indeed to show that the change does not involve the creation, as was once supposed, of new matter—for this was proved long ago —but to exhibit in its details the hidden mechanism which produces it, but hitherto without success. We know that light is essential to the process of assimilation in plants, since this does not proceed in the dark, but this fact does not help us to an explanation, for light in this case is a mere stimulant, and never produces the same or similar effects outside the vegetable organism. Liebig and others have attempted to show that the process of assimilation in plants commences with the formation of some simply constituted body, such as oxalic or formic acid, with the elimination of oxygen, out of which by condensation and further separation of oxygen more complex bodies, such as sugar, fats, &c., are formed; but there is not the slightest evidence at present in favour of this view. The first product of assimilation that is distinctly recognized is starch, a highly complex organic, we might almost say an organized body, which appears at once with all its characteristic properties, like Minerva springing fully armed from the head of Jove. If we are to adhere to the facts so far observed, we must conclude that the plant does not proceed as we should do in the laboratory, beginning with the more simply constituted compounds and advancing to the more complicated, but that the reverse process is the one actually adopted, the supposed intermediate products being more probably the results of retrogressive metamorphosis. This conclusion is, however, so much opposed to ordinary chemical views that one cannot feel surprised at the constantly repeated attempts to clear up the question. There can be no doubt indeed that much here remains to be done and to be discovered.

Intimately connected with this subject is that of chlorophyll, the green-colouring matter of leaves, which is always found wherever the process of assimilation in plants is going on, and nowhere else, and is therefore doubtless an essential factor in the process. What part it plays in this process is, in my opinion, still unknown. Its action is probably in part chemical, in part physical, and this adds, it may be, to the difficulty of understanding it. It is generally supposed that it is chlorophyll which by its direct action on the carbonic acid and water with which it comes into contact leads to the formation of organic matter with elimination of oxygen. But this is, I think, a mere assumption—an error due, like many others, to a mistaken use of terms. The chlorophyll of chemists is simply an organic colouring matter, like alizarin or indigo, but being in the vegetable cell intimately associated with other matters, vegetable physiologists have attributed to the action of one, and that the most obvious, constituent what is really due to the complex, perhaps even to some quite other constituent of the complex. It is impossible to conceive that the chlorophyll of chemists can be endowed with the remarkable and exceptional properties attributed to it by physiologists; it is a chemical entity, nothing more. It may indeed be said that chlorophyll only acts as it is stated to do when inclosed within the vegetable cell, but this merely amounts to saying that its action is not merely chemical, but is controlled by the vitality of the cell, which, I suppose, means the action of the protoplasm. If chlorophyll is the agent whereby the decomposition of carbonic acid and water is effected, how, it may be asked, is the agent itself produced? It does not come from without; the plant must be able to form it in the first instance. We are told by vegetable physiologists that the Coniferæ when raised in total darkness from seeds produce chlorophyll. In light or in darkness I am convinced it is the same; the plant forms chlorophyll as a means to an end. What the end is we know; it is the assimilation of carbon and hydrogen to form organic matter. How does the chlorophyll assist in attaining this end?

In propounding a new theory in reply to this question I venture to claim your indulgence, such as has been accorded to some of my predecessors and others who at these meetings of the British Association have been permitted to make statements and use arguments of a novel or paradoxical character, which, if they effect nothing else, at least afford a relief to the usual routine of scientific reasoning. My experiments on chlorophyll have led me to infer that the constitution of that body is much less simple than it is generally supposed to be. I do not mean by this that chlorophyll is a mixture in the usual sense; everyone who has paid any attention to the subject knows that ordinary chlorophyll consists of several colouring matters, some of which are yellow, not to mention fatty matters which are unessential. What I mean to say is this, that the pure green substance, the chlorophyll *par excellence*, does not belong to the same class of bodies as alizarin or indigo, but contains three elements, each of which is essential to its constitution, one being a basic nitrogenous colouring matter, the second a metal or a metallic oxide, the third an acid, the three together constituting green chlorophyll. The basic colouring matter is a body of very peculiar properties; it is the phyllocyanin of Fremy; the metal may be iron or zinc, the acid I will suppose to be carbonic acid. Now the plant having formed its colouring matter, the metallic oxide being present in some form or other, and the carbonic acid being supplied by the atmosphere, all the necessary conditions co-exist for the formation of chlorophyll. The compound is an unstable one; it easily parts with its carbonic acid, giving it up to the protoplasm or whatever the agent may be that effects its actual decomposition under the influence of light. The advantage of this arrangement would consist in this, that the carbonic acid would be presented in a more condensed state to the agent which effects its decomposition than if it were merely contained in a watery solution, but more loosely combined, and therefore more easily accessible than if it were united to a strong base such as potash or lime. The carbonic acid having been disposed of, the other two constituents would be in a state to take up fresh quantities of carbonic acid, and so on. Chlorophyll would therefore act as a carrier of carbonic acid in the plant, just as hæmoglobin serves to convey oxygen in the animal economy. Numerous objections may of course be raised to the theory of which I give an outline; I only throw it out as a tentative explanation, showing that the function of chlorophyll may be, in part at least, chemical, and that we need not suppose it to be endowed with the marvellous and exceptional powers usually ascribed to it. Other and more probable explanations will doubtless suggest themselves when this difficult subject has been more thoroughly worked out. Eventually, too, it will be found, I imagine, that physical forces as well as chemical affinities play a part in this as in every other process of the vegetable economy. In the case of cholorophyll there can be no doubt that the green colour and the peculiar behaviour towards light have something to do with its action, but on this point it is not necessary for the chemist to pronounce any opinion. I may take this opportunity of mentioning the important experiments of Sachs and Pringsheim on the optical properties of chlorophyll in their relation to assimilation in plants, as they are probably not so well known to chemists as to botanists.

What I have said may serve to show that the very first steps of the process whereby organic or organized matter is formed in plants are hardly understood. We understand still less the further steps leading to the production of the more complex vegetable bodies—acids, alkaloids, fatty matters. Granted that we were able to trace the formation in the plant of a compound of simple constitution, such as oxalic or formic acid, how far should we still be from understanding the building up of such compounds as starch, albumen, or morphia? The syntheses so successfully and ingeniously carried out in our laboratories do not here assist us in the least. We know the steps by which alizarin is artificially produced from anthracene; but can anyone for an instant suppose that the plant commences in the same way with anthracene, converting this into anthraquinone, and having acted on the latter first with acid, then with alkali, arrives at last at alizarin? Indeed the plant never contains ready-formed alizarin at all. What we observe from the commencement is a glucoside a compound of alizarin and glucose, which, so far as we see, is not gradually built up, but springs into existence at once. When we think of the complicated process by which indigo is produced in the laboratory with the various substances and appliances required, and then see how in the minutest seed-leaves of a plant like woad a still more complex substance, indican, is found ready-formed, we stand confounded at the simplicity of the apparatus employed by the plant, and are obliged to confess that we have no conception of the means whereby the end is attained. The same difficulties occur in other cases, and it will therefore probably be conceded that the synthetic process carried on in plants, from the first step to the last, are not in the least understood.

It might be supposed that after all the labour and attention bestowed on the inorganic constituents of plants we should know something of the part played by these constituents in the processes of assimilation and nutrition, but here the obscurity is as great as elsewhere. We know by experiment that certain inorganic matters—potash, lime, magnesia, iron oxide, phosphoric acid—are essential to the growth of plants; but of their mode of action, or of the reason why certain plants require potash salts, others lime, and so on, we know nothing. Phosphoric acid is no doubt an essential constituent of the protoplasm of the plant; but why cellulose, of which the various organs chiefly consist, should require mineral matters, which do not enter into its composition, for its formation and building up is still a mystery.

The department of chemistry which relates to the decomposition of organic and organized matters presents problems almost as difficult of solution as those relating to their formation and building up; that is to say, the phenomena observed do not apparently obey the same laws as those prevailing in the inorganic world. When I began my chemical studies the difference in this respect between mineral and organic compounds was less clearly seen than at present. The conversion of alcohol into acetic acid, the putrefaction of animal and vegetable matter were thought to be simply due to oxidation; they were phenomena, it was supposed, exactly similar to the rusting of iron, the tarnishing of metals, the fading of colours. That a third body was required to initiate and continue the process of decomposition, that organic matter in contact with purified air would remain unchanged for any length of time—was not known nor suspected. I am not quite sure whether spontaneous decomposition—*i.e.* the splitting up of a complex body without the intervention of an external agent—might not at that time have been considered possible. In order to explain the phenomena of fermentation, the decomposition of sugar into alcohol and carbonic acid, for instance, we had only the theory of contact—devised by Berzelius and Mitscherlich, the latter of whom used to expatiate on the subject at great length in his lectures. When this ghost of a theory was laid by Liebig, who suggested an intelligible explanation of the phenomena in accordance with the facts then known, it was felt to be quite a relief, as affording a resting-place—if only a temporary one—for the mind. The brilliant researches of Pasteur, which have thrown so much light on the action of the insoluble organized ferments, I need only refer to, as they are so widely known, even outside scientific circles; and since also investigations such as his cannot be discussed without some reference to biological questions, which cannot be entered on now. I will confine myself therefore to a few remarks on the unorganized or soluble ferments, one of which I had occasion to examine when engaged in investigating madder and its colouring matters. These ferments, the type of which is diastase—a substance found accompanying starch in the seeds of plants—are soluble in water, perfectly neutral, devoid of all definite form, and though apparently inert, able when acting within the sphere in which Nature has placed them to cause changes and decomposition of the most profound character. Their action excludes everything in the shape of vitality, and yet it is as mysterious and unaccountable as anything that the vitality of the organized ferments is capable of effecting. Indeed, in vegetable, and especially animal, organisms they seem expressly intended for the attainment of certain ends necessary for the well-being, or even the existence, of the organism, insomuch that it has been supposed, with some show of reason, that it is to bodies of this class existing within the cells of organized ferments, but not separable by any means at our disposal, that the changes produced by the latter are really due.

A great deal of attention has been paid to the product and results of fermentation, but very little hitherto to the *modus operandi* of the ferments themselves, and yet this seems to me to offer a wide field for interesting research, especially in the case of those of the soluble class, which are easily prepared, and can be manipulated in the laboratory like any chemical substance without the tedious precautions and preliminary operations necessary in the case of the organized ferments. In what way, it may be asked, do these soluble ferments produce the effects peculiar to them? Is the action essentially chemical, or is it due to physical causes as well? Is the quantity of fermentable matter acted on by a certain quantity of ferment unlimited in amount, or are there limits to that amount somewhere? Does the ferment itself undergo any change during the process of fermentation, or is it the same afterwards as before and capable of acting on fresh quantities of fermentable matter? When a ferment is replaced by a strong mineral acid, the products of decomposition being the same, is the *modus operandi* in both cases alike, or must a different explanation be in each case sought? These questions have never been satisfactorily answered, and await solution. I know of only one attempt to show what actually takes place during a process of fermentation set up by a soluble ferment.

The experiments of Wurtz (*Comptes Rendus*, xci. 787), on papaïn, the soluble ferment of *Carica Papaya*, led to the conclusion that the fibrin on which it is made to act combines in the first instance with the ferment itself, the latter after the hydration of the fibrin is completed being again set at liberty, and then able to act on fresh quantities of fibrin. Thus, according to Wurtz, the action is found to be the same as that of chemical agents, properly so called, such as sulphuric acid, of which minute quantities may exert a hydrating action in consequence of the transitory formation of compounds which are constantly being produced and again decomposed.

There is another question referring to these soluble ferments to which in the present state of our knowledge it is impossible to frame a probable answer, viz. why does it so frequently happen that each ferment exerts a specific action, an action peculiar to itself, this being in fact, in the absence of any marked chemical characters, the only means by which they can be distinguished one from the other? Why does one ferment act on starch only, while the function of another consists in the hydration of fibrin, that of another in the decomposition of a glucoside, and so on? In accordance with the explanation of Wurtz, we should say that a specific ferment is one capable of combining only with the body on which it is to act, and with no other. I was led to ask this question when engaged in the examination of the colouring matters of *Rubia tinctorum*. The root of this plant, the madder of commerce, contains glucosides, which, though coloured, are quite devoid of tinctorial power. Nature has at the same time placed in the root a peculiar ferment, which, coming into contact with these glucosides at a certain temperature, effects their decomposition, splitting them up into glucose and true colouring matters. Now this ferment is a body *sui generis*, and cannot be replaced by any other ferment that I have tried; its action is specific. Why Nature should have deposited this body in the recesses of the plant for the express purpose of acting on certain glucosides and forming colouring matters, the object of which, so far as the economy of the plant is concerned, can only be guessed at, is difficult to understand. One is inclined in such a case to revert to the old-fashioned doctrine that some natural processes were devised for the use and delectation of man. It is quite certain in the case of madder that had it not been for its peculiar ferment erythrozym, the valuable tinctorial properties of the root, which have for centuries been applied in the production of that splendid dye Turkey red, would have remained unknown perhaps to the present day, since the only efficient substitute for the natural ferment is a strong mineral acid, and such acids and their uses were unknown in former days.

I am inclined to think that some of the younger chemists and physiologists of to-day may live to see the time when all the at present mysterious and unaccountable processes going on in the organisms of plants and animals, including those of fermentation, will be found to obey purely physical and chemical laws. To the biologist it may seem derogatory to the dignity of his science to have the principle of vitality, which has so long reigned supreme, dethroned and replaced by hard, unbending law. Such, however, is not the opinion of that distinguished botanist Sachs, who says, referring to this very point:—"Der Organismus selbst ist nur die aus verschiedenen Theilen bestehende Maschine, die durch weitere Eingriffe äusserer Kräfte in Bewegung gesetzt werden muss: von ihrer Struktur hängt es ab, welchen Effekt diese äusseren Kräfte an ihr bewirken. Es würde einen sehr niedrigen Horizont wissenschaftlicher Bildung verrathen, in diesem Vergleich eine Herabsetzung des Organismus sehen zu wollen, denn in einer Maschine, wenn auch nur von Menschenhänden gemacht, liegt das Resultat tiefsten und sorgfältigsten Nachdenkens und hoher Intelligenz, soweit es ihre Struktur betrifft, und wirksam sind in ihr schliesslich dieselben Naturkräfte, welche in anderer Combination die Lebenskräfte eines Organs darstellen. Die Vergleichung des organischen Lebens mit unorganischen Processen kann nur dann als eine Erniedrigung des ersteren gelten, wenn man so thöricht gewesen ist, die letzteren als etwas Niedriges und Gemeines aufzufassen, während die unbegreifliche Grösse und Durchgeistigung der Natur in beiden Fällen sich gleichartig offenbart" (*Vorlesungen über Pflanzenphysiologie*). The time may be far distant when these views of the great botanist shall be universally accepted; but they will, I think, sooner or later prevail.

The little known territory which separates the domains of chemistry and physiology will, in my opinion, offer a wide and interesting field for research, after that of pure chemistry shall have been exhausted or lost its interest. Most important problems connected with life and its relation to the inorganic world there await solution, and I confess that I am inclined to envy the young investigator who, coming provided with an ample store of chemical and physical knowledge, shall apply himself to the solution of these problems. The pleasures derived from the successful pursuit of such studies belong to the highest and purest that we are able to conceive. I can, however, only repeat what has so often been said before, and what the young man of science should not forget, that a life devoted to research only involves no material rewards; it certainly never secures wealth, sometimes not even honour nor fame. Looked on with indifference or even dislike by the State, the Church, and the public at large, all that the man of science can certainly look forward to at the close of his career is the addition at his hands of a few stones to the vast edifice of truth, and the consciousness of having attained a higher stage of intellectual insight.

You may probably expect me, before I conclude, to make some reference to technological matters, to the various chemical arts and manufactures for which the Manchester district is noted. At the last meeting of the British Association in Manchester a report on the condition at that time of manufacturing chemistry in the South Lancashire district, by Sir Henry Roscoe, the late Dr. Angus Smith, and myself, was laid before the Chemical Section. A similar report showing the progress made in chemical technology since that time would have been interesting. Great changes have taken place during the period that has elapsed, especially as regards the alkali trade, and quite a new branch of industry has been developed, that of the coal-tar colours. A description of these new features of our chemical industry with statistics of production would therefore have been acceptable. The idea of a report had however to be given up on account of the difficulty of obtaining reliable information as to details, and in these matters it is the details only which are interesting, the general features of the subject being well known. It can hardly be a matter for surprise, I think, that our manufacturers, considering the active competition to which they are exposed, and the disadvantages under which they labour in consequence of the exclusiveness of foreign nations, should be loth to furnish information which would benefit their rivals in trade. Several interesting papers on branches of chemical industry by gentlemen well versed in them will, however, be read before the Section, and these will, to a great extent, make up for the want of a general report. In the Chemical Section of our Jubilee Exhibition, too, you will see a very fine collection of chemical products, more extensive and beautiful, perhaps, than any previously brought together, and these will give you a good idea of our industrial activity. It would have been interesting to witness step by step some of the processes employed in the manufacture of these various products, but this, I am sorry to say, must not be expected generally.

To some it may seem that this Jubilee Exhibition shows the manufacturing industry and prosperity of this district at least at their highest state of development; that they are now at their meridian, and in the future are doomed to decline. If this be so—and there are certainly indications which seem to favour this view—it would be well for those whose visits here are only occasional to take especial note of the present state of things so as to be able to compare their impressions when they next visit us with those now received, since gradual changes in communities, as in individuals, are more patent to casual observers than to those who are always on the watch.

From some points of view the signs of the times are certainly not encouraging. It should not be forgotten that the manufacturing prosperity of this district depends to a great extent on the ample supply of a product which is brought to us at some cost from tropical and semi-tropical countries to be re-exported in the shape of manufactured goods. A political convulsion abroad, and this, unfortunately, is a casualty that may at any time be expected, or even the determination on the part of other nations to starve us out, however short-sighted such a determination might be, might cut off our supplies and disable us permanently as we were partially disabled twenty-five years ago. If to this be added the fact that foreign nations are becoming increasingly hostile and exclusive commercially, we cannot feel surprise at the dismal forebodings entertained and the confident predictions of decline uttered by some who claim to know all the facts. I ought to apologize for alluding to so gloomy a subject on the occasion of this to a great extent festive gathering, but then men of science like to look at a question not only from a hopeful but from every point of view. Fortunately on this question they are not called upon to pronounce any opinion one way or the other.

Should this be the last time that Manchester shall entertain the British Association in the day of its prosperity, I can only say with the German poet—

> "Schliesst den Kreis und leert die Flaschen]
> Diese Sommernächte feiernd,
> Schlimme Zeiten werden kommen,
> Die wir Auch sodann ertragen."

Whether in prosperity or adversity I feel sure that this city will always endeavour to entertain its visitors to the best of its ability. On the present occasion I may with confidence on the part of the chemical world of Manchester offer to the many friends from near and far who honour us with their presence at this meeting a most hearty welcome.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY HENRY WOODWARD, LL.D., F.R.S., V.P.G.S., PRESIDENT OF THE SECTION.

SINCE I received the friendly intimation from Prof. Bonney your distinguished and able President of last year, that the Council of this Association had done me the honour to select me to occupy the Presidential chair of this Section which he had vacated, I have been greatly exercised as to what subject to choose for the brief address with which it has now become customary to open the session. Not that there is any lack of materials ready to hand for the purpose—on the contrary, the subjects embraced by geology are now so varied and extensive that the effort to focus them in a single mind is ever becoming a more difficult task to accomplish, and demands the literary skill of a Lyell or a Geikie to marshal and arrange them from year to year in a manner suitable for presentation to you at our annual gathering.

Foremost in interest must necessarily be that which relates to our home affairs, and in this I have been most kindly favoured by Dr. A. Geikie, the Director-General of the Geological Survey of Great Britain, who sends me a brief notice of the progress of the Survey for 1886, taken partly from his Annual Report as Director-General and partly from information supplied by the office through the kindness of Mr. William Topley, our Recording Secretary. The following is the statement which I have received :—

The survey of the solid geology of England and Wales was completed at the end of 1883, and the field staff has since been occupied in surveying the drift deposits, making at the same time such revisions of the ordinary (solid) geology as may be necessary. In the north and east of England the drift and solid have been surveyed at the same time. The areas examined in the earlier days of the survey, in the south, centre, and west of England, and in Wales, were done for the solid rocks only.

In order to meet the great need for a general map of England and Wales on a moderate scale, one is being engraved by the Survey on the scale of 4 miles to 1 inch (1 : 253440), and will be issued in fifteen sheets.

A few of the survey memoirs relate to large areas, and give complete descriptions of the formations therein exposed, but most of the memoirs are explanations of special sheets of the map. A series of monographs is now in preparation giving full descriptions of special formations. Mr. Whitaker has charge of that on the Lower Tertiaries ; Mr. H. B. Woodward and Mr. C. Fox-Strangways are preparing the Jurassic memoir, the former taking the rocks south of the Humber, and the latter those of Yorkshire ; Mr. Jukes-Browne is writing the Cretaceous monograph ; and Mr. Clement Reid that on the Pliocene beds.

In Scotland some advance has been made in mapping the important and complicated area of the north-west Highlands. The surveyors there were chiefly engaged between Loch Stack and Ullapool, subsequently completing the area about Durness and Eriboll. The other parts of Scotland now being surveyed are the north-eastern and western side of the Grampians, all south of the latter having been already completed.

Ireland is entirely surveyed with the exception of a small area in Donegal, which will probably be completed this year. This district is of interest from its resemblance to the north-west Highlands, and from the problems which it presents as to the origin of the crystalline schists. The recent discovery of organic remains amongst the Donegal schists adds additional interest to this inquiry.

The publications of the Survey during the past year are as follows : England and Wales, six sheets of the map, two sheets of horizontal sections, three of vertical sections, and six memoirs ; Scotland, three maps and one memoir ; Ireland, two maps and six memoirs.

The next matter which has arisen since our last meeting relates to our colonies, and comes to us in the shape of a message from the retiring President of the Association, Sir William Dawson, who has embodied his ideas in a letter to the President of the Royal Society (Prof. Stokes), copies of which have been sent also to all the learned Societies. To the former I am indebted for a copy accompanied by a favourable report thereon from the Royal Society of Canada.

As the object of this communication is one in which I am sure we, as Englishmen, must all feel a hearty sympathy, appealing as it does to our patriotism in its widest sense, as well as to our devotion for and interest in the science of geology, I feel I shall not need to apologize for introducing it to your notice here.

We are invited by it to enrol ourselves, as geologists, in a Federal Union, composed of all our brethren at home, in our colonies, and in all the dependencies of the British Crown. Nor are we to stop here, for when this has been satisfactorily accomplished it is suggested that we should invite our English-speaking cousins of the great United States, with whom we are already in such close alliance upon so many objects of common scientific interest, to join our Geological Confederation, and, having thus obtained an overwhelming majority, we are to proceed—without armies or vessels of war—to extend our peaceful conquest over every country on the habitable globe, urging and persuading those countries which have not established geological surveys to do so forthwith, and inviting those which have surveys of their own to join our British Association Geological Union. And when all has been accomplished in this direction our exertions as a confederacy may well be extended to secure the mapping of all those outlying regions of the earth's surface at present imperfectly known or still geologically unexplored.

Suggestions such as these could hardly come at a more fitting and appropriate moment, for are we not now on the eve of the completion of the geological surveys of the British Islands? if such a task can ever be said to be completed which has occupied the attentive study of so many able geologists during the last eighty years or more, and from the very nature of the case must always require additional research and revision.

India, Africa, and our colonies may all hope for future assistance from the many geological students now being trained in our schools and colleges, who may not be required in the near future for home surveys, and must needs go further afield to win their title of admission to the ancient and honourable order of "Knights of the Hammer."

This idea of scientific federation was referred to by Prof. Huxley in his Presidential Address to the Royal Society in 1885, and subsequently by the present President (Prof. G. G. Stokes) in November last.

If we would devise a scheme by which we might from time to time, recognize in a suitable manner—whether by corresponding membership, or honorary fellowship, or by medals and awards—as Prof. Huxley has suggested, the good scientific work being done by members of the many Societies in our distant colonies of Canada, South Africa, Australia, New Zealand, and elsewhere, that would indeed be a step in the right direction, and would doubtless prove most helpful and encouraging to all our fellow geologists abroad.

The Geological Society of London, no doubt, to some extent covers this ground; but it should be noticed that in the view of this Society, our colonies and other dependencies are not, and I think rightly, recognized as foreigners, that designation being employed for those who are not in any sense subjects of the Queen.

As a consequence, the geologists of our colonies are not looked upon as eligible for honorary connexion with the Geological Society, and though in the distribution of the medals and awards their work is no doubt noticed, yet that is now so important and extensive that it might be desirable to secure for it a more specific and extensive recognition than has hitherto perhaps been possible.

Might we not also through the home influence we could bring to bear by means of this great Section of the British Association succeed in inducing our practical colonial Governments to see the enormous commercial as well as scientific gain that must eventually accrue to themselves if they would, with wise liberality, continue to completion their much-needed geological surveys, instead of (as has too often happened) abandoning the work before its end has been attained, or making its maintenance from year to year contingent on the chance discovery of gold, or the successful boring for coal or water—results not always to be attained within twelve months by a geologist in a new country, however good he may be, unless he have a fairy godmother or a divining-rod at his command?

If by means of our confederation such useful and helpful works can be inaugurated, we shall have fulfilled an object well worthy the initiation of Sir William Dawson, and of all those whose names may be connected with so laudable an undertaking.

Nor need such a development of the work of this Section interfere in any way with the labours of the "International Geological Congress," which occupies a distinct field of its own; for whatever we might accomplish in carrying out the suggestions put forward by Sir William Dawson would really be in effect to second and support—not to hinder—the work of that most useful body of geologists.

Our next topic relates to foreign affairs.

The International Geological Congress, which met in Bologna in 1881, and in Berlin in 1885, will hold its next meeting in London in 1888. This year the Committee of the Congress on Geological Nomenclature will meet during the Association week at Manchester. Prof. Capellini, of Bologna, is the President of this Committee, and Prof. Dewalque, of Liége, is the Secretary. Its object is to discuss various questions respecting the classification and nomenclature of European rocks, and to report thereon to the Congress in London.

It is quite certain that a large number of Continental and American geologists will be present in London next year, and it rests with English geologists to determine whether the meeting shall be as successful as those which have preceded it. The Berlin Congress left the arrangement in the hands of a small Committee of English members (Messrs. Blanford, Geikie, Hughes, and Topley), and advantage will probably be taken of the presence of so many geologists in Manchester to further the organization of the English meeting.

The occasion of the Congress visiting London next year should also be a sufficient reason to enlist new members here, and it is to be hoped that a very cordial reception will be accorded to all those who come from abroad to attend the meeting. It ought to be a great success, and deserves our warmest sympathies and co-operation.

Geology seems, at present, to be passing through what may not inaptly be termed a transitional or metamorphic period in its history, when old-established ideas are rapidly melting away, and under fresh influences are crystallizing out into quite other forms.

"New lights for old" is the popular cry both in science and politics, and, like the Athenians, nothing delights us more than to hear tell of some new thing.

If the proposition lately made by Prof. Judd, the President of the Geological Society in London, in his recently delivered Anniversary Address, holds good, that mineralogy is the father of geology, it seems not improbable that, like Saturn's offspring, our science is in danger of being devoured by its reputed parent; for certainly mineralogy, in the form of petrology, has of late years most largely occupied the geological field, whilst palæontology, once the favourite child of geology, is in its turn threatened with imminent extinction, as a separate study, by biology, which, without any substantial gain, now replaces, *in name only*, the hitherto better known sciences of botany and zoology.

Indeed, could the views so eloquently put forward by Prof. Judd be maintained, mineralogy itself would have to be added to the list of sciences included under biology. But notwithstanding the well-known aphorism of Linnæus—

"Lapides crescunt; Vegetabilia crescunt et vivunt;
Animalia crescunt, vivunt, et sentiunt"—

the growth of the first is of a totally different nature from that which takes place in the last.

Minerals, or more properly crystals, increase or grow in size by additions to their *external surfaces* of molecules of matter identical with themselves. They are therefore as a rule homogeneous throughout, almost rigid, and remain under ordinary circumstances unchanged irrespective of time.

Plants and animals, on the contrary, increase by intussusception, or the taking of matter within their tissues. Their bodies are *not homogeneous*, and they exhibit all the various phenomena of growth and decay.

We stand, then, still like "watchers on the threshold," not yet admitted beyond the veil. We are not prepared to include minerals in the study of living beings, nor are we, I submit, any nearer the solution of the problem, What is life? whether we call it "*vitality*" or "*vital force*," nor can we produce it like "electricity" or "electrical force" by the aid of mechanics. That it has existed ever since our planet became habitable by living organisms is beyond doubt; and since life first dawned it seems equally certain that this "vital force" was never at any time extinguished, but, like the sacred flame of Iran, its light has always gladdened our earth with its presence.

I have already referred to the vastness and diversity of the domain which Geology claims as her own; indeed, we might, if so disposed, pursue our subject in its *cosmical aspect*, and, inviting the astronomer and the physicist to our aid, proceed to consider the evolution of our earth and its subsequent history as a part of the solar system.

Or, taking up *geognosy*, we might inquire into the materials of the earth's substance and the chief rocks and minerals of which its crust is built up.

Should *dynamics* charm us, then we may study the various agencies by which rocks have been formed and altered, and the frequent changes in relation to sea and land which the terrestrial surface has undergone in former times.

Does *rock-architecture* attract us? It is ours to inquire how the various materials of the earth came to be arranged as we find them—whether wrought by living agents, or ejected by volcanic forces, or laid down quietly by water.

Or is *chronology* the object of our study? Then our task will be to investigate the well-marked succession of the stratified rocks and the sequence of events which they record.

Again, we might prefer the *physiographical aspect of geology*, embracing the history of the features of the earth and the causes which have brought about its varied conditions of continent and ocean, of mountain and valley, hill and plain, making up that grand diversity of surface which constitutes its scenery.

Yet more, it is within our domain as geologists to investigate *the past life of the globe* through all its successive changes and to trace it from its earliest dawn in pre-Cambrian times down to its grand development at the present day.

One result of the very vastness of this kingdom is that there is a tendency amongst its subjects to form into separate constituencies, and these in an incredibly short time evolve languages of their own, so that, unless this fissiparity can be successfully arrested, we shall speedily repeat the story of "the confusion of tongues," and our geological tower, which once promised by our combined labours to reach grandly heavenwards, may soon cease from building altogether.

This incoherence in our body politic may, I think, be traced to that great development by the microscope in mineralogical geology and petrology, which has no doubt been necessitated by the investigation of those remote pre-Cambrian or Archæan rock-masses in the north-west Highlands, Shropshire, the Malverns, South Wales, Cornwall, and the west of Ireland, whose fossils, if they ever existed, have been entirely obliterated[1] by the changes which their matrix has undergone, and whose very stratification has been lost by metamorphic action. In such investigations some of our ablest geologists have now been for long occupied with the best possible results, and Bonney, Callaway, Cole, Davies, Geikie, Hicks, Hull, Judd, Lapworth, Peach, Sorby, Teall, and many others have been labouring most zealously on these most ancient sediments, barren though they be of life forms, and often destitute of bedding.

It is refreshing, however, to find Prof. Judd at times abandoning volcanoes, and turning his attention most successfully to lizard-hunting with Prof. Huxley in the Elgin sandstones, or studying the micro-organisms in the cores from the Richmond boring or the valley of the Nile; to see Dr. Hicks leaving his patron St. David far behind, and digging for bones in the pre-Glacial caves at St. Asaph. Prof. Lapworth, too, we see avoiding Cape Wrath, and discoursing on the beauties of Canadian Graptolites and the Cambrian rocks at Nuneaton.

Thus there is still a bond of union connecting stratigraphical geology and palæontology and a common ground of interest whereon all geologists may meet. It should then be our endeavour not to dissociate ourselves or our interest from any subject of geological inquiry, but to maintain the union between all branches of our science and with all workers in whatever field they may labour, adopting for our motto the ancient maxim, "*Vis unita fortior est.*"

Especially should we adhere to the study of palæontology, seeing that it is indissolubly connected with one of the earliest chapters in the history of our science. Indeed, through the evidence afforded by organic remains, William Smith (better known by the title given to him by Prof. Sedgwick, "the father of English geology") was led to those remarkable generalizations as to the identification of strata by means of their contained fossils, which have exercised so great an influence over our own science during the past ninety years, and is still the guiding principle on which our classification of the sedimentary rocks is based. What Wollaston has done for mineralogy and crystallography, William Smith initiated for stratigraphical geology; and we cannot overlook our obligation to Smith whilst we reverence the work of his distinguished contemporary, Wollaston.

Palæontology, or the study of ancient life forms, stands somewhat in relation to geology as the science of archæology does to history, or as zoology and botany to physical geography. But, whereas the investigator of recent living forms deals with entire organisms and can study both their morphological and their physiological history as well as their geographical range, the palæontologist has too often to deal with imperfect remains, many of which have no exact modern representative, and has, in consequence, to look for and seize upon minute characters for his guidance, which the worker on recent forms would probably neglect as too trivial for even specific diagnosis.

The palæontologist, if he would succeed, must in fact be a trained zoologist or botanist, as the case may be, and an accomplished geologist also; such combinations of qualities like those possessed by the earlier race of "naturalists" are less frequently to be met with at the present day. They represent amongst us the same class of men as the "general practitioner" does in medicine; they are the all-round good scientific men, but not "specialists."

Biology, or the study of living things, has now become so vast a field that everyone is compelled to take up some special subject, and in striving to master it he makes his reputation as an authority on this or that group of organisms.

There is much to be said in favour of such a method of working, but I hold that everyone who so elects to spend his life must first of all pass through a thorough grounding in general biology, and should on no account take up special work until he has mastered thoroughly the general principles of scientific classification and the various types of organized beings, otherwise he will be for ever viewing all Nature with distorted vision, seeing, in fact, "men as trees walking." If as a student he shall have been nurtured wholly on the anatomy of the *sole*, all objects will be viewed from the stand-point of that one-sided fish. If the cockroach has engrossed his youthful studies, all nature will swarm with *Periplaneta orientalis*.

We have to guard against the starting of *student-specialists*. They must begin by being "general practitioners" if they are ever to do any good in the world of science, and after serving their time in a museum or elsewhere, then by all means let each follow his own "bent" and devote himself to some particular group, as did Davidson to the *Brachiopoda*, to the exclusion of all else.

It is the absence of "all-roundness" which has retarded more than any other thing the constant interchange of ideas between zoologists, botanists, and palæontologists, without which science languishes. Biologists as a body do not care to look at or study fossils; they see neither form nor beauty in the petrified fragments of a plant or animal such as would induce them to study these more closely, and they turn to the exquisitely perfect specimens of recent objects in their cabinets with a sigh of relief. But *Nemesis* is at hand created by our modern system of extreme biological training. The student of to-day is averse to the systematic work of both zoology and palæontology in our museums, and, technically inclined, craves for nothing so much as to be allowed to embed some interesting embryo in paraffin and cut it into 10,000 slices.

As a consequence our museums will suffer unless we can revive amongst our students a taste for and a love of general natural history; such, we mean, as the *taste for Nature* which excited the enthusiasm of Charles Kingsley and stimulated the zeal of Charles Darwin. We cannot all sail round the world as did Banks and Solander, Darwin, Huxley, Hooker, Wyville Thomson, Moseley, and so many other naturalists, though the mere act of travelling has now become so ridiculously easy that our own Association awoke one morning in Montreal, and may, for aught we know, find itself some day in Sydney or Melbourne! But we can fully appreciate Nature in a dredging expedition or feel her influence on a moor or mountain, in a quarry or down a mine.

What we want for our students in these high-pressure days are less frequent attendance in the examination room and a more frequent examination of Nature in the field. Our professors must take their men more often afield, and show them how to collect specimens and familiarize them with the aspects of natural objects as seen *without microscopes*, and they will return to their studies with far better and keener eyesight after their own *macroscopic* vision has been enlarged by contact with Nature.

Whoever then takes up the study of fossils must also be well acquainted with the structure of living animals and plants; he may also be expected to go on adding to his store of biological knowledge—but as some division of labour is absolutely essential, the man who pursues palæontological research must be prepared to concentrate all his energies to the elucidation of these extinct organisms, studying, but not occupying himself in describing, recent forms.

In order, however, to work satisfactorily at any particular group of extinct organisms, his eyes and his understanding must go through a long and careful training before he will be able to interpret correctly the appearances presented by the specimen before him, and to avoid the fallacies by which he is liable to be misled, arising out of the necessarily imperfect materials and their different modes of preservation in the matrix.

He must learn to distinguish between a suture and a fracture, and to know when a specimen has been distorted by cleavage or other mechanical cause, or altered by mere difference of mineralization. Such deceptive appearances have too often led to the multiplication of species, and even the creation of spurious genera.

Thus occupied in the investigation of ancient life forms, he will in truth be only writing the first chapters on the botany or the zoology of the earth, and whilst his carefully obtained results are of the greatest importance to the speculations and conclusions of the geologist they are equally essential to and a part of biological science.

My friend Dr. Traquair has recently thus expressed, in relation to his own subject, what I have attempted to make more general:—"The man who satisfactorily investigates the structure or determines the systematic position of a fish or reptile *preserved in stone* is as much a zoologist as he who describes a similar creature *preserved in spirits*, though with this difference, that the former task is in some points rather the more difficult, seeing that we have *only the hard parts* to go upon, and these generally in a crushed, fragmentary, or scattered condition. And," he adds, "without a genuine interest in, as well as a thorough knowledge of, recent biology, no one can hope to produce work of any value in palæontology."

[1] Traces of fossils are said to have been met with in Donegal, and I have just received evidence of Trilobites in the Upper Green Llanberis slates at Penrhyn, hitherto considered unfossiliferous!

Of course the value of all palæontological work, as of all zoological or botanical work, must depend entirely upon the care and exactness with which the work is performed.

Time, the great assessor of all human labours, will sit in judgment upon them and pronounce by their durability or instability the comparative value of each.

It appears to me that to the careful palæontological worker, as to the careful archæologist, the greatest merit is due if he succeed correctly in deciphering the too often fragmentary and blurred remains of a bygone age, and giving us in the present an accurate interpretation of a page from the life-history of the past.

Then, too, there is the geological aspect of palæontology. And here I may state that one of the charges made by a brother zoologist against us is "that we use fossils *merely* as counters by which to record the progress of geological time."

As well might exception be taken that the milestones along a turnpike road had been used by a traveller to calculate the length of his journey.

But omitting the word *merely* (for fossils have been made to give up many secrets to the investigator besides their age), I gladly accept the charge as conveying a great and important truth.

Do not the historian and the antiquary use the coins and medals dug from the ruins of the dead and long past dynasties of the world as sure guides in the chronology of the human period?

And may not the geologist also use "the medals of creation" as Dr. Mantell aptly called them—coined in no counterfeit mint, as the best and most trustworthy guides to enable him to establish the chronology of the stratified rocks of the earth?

Great then as is the benefit which zoology has derived from palæontology in enabling the zoologist to learn the earliest appearance in time of each group of organisms, and the modifications in structure, so far as we are enabled to ascertain them, which each may have undergone from the ancient to the modern period—it may be doubted whether even this valuable aid equals the service performed to stratigraphical geology by the careful study of organic remains—in enabling us to write the chronology of the rocks over so large a portion of the habitable globe.

Without fossils stratigraphical geology would be as unsatisfactory as it would certainly be uninteresting; with their aid it becomes, both in the field and in the cabinet, one of the most attractive and delightful of studies.

Owing to the very nature of sedimentary deposits, being of necessity either lacustrine, estuarine, or marine in origin, our knowledge of the ancient land surfaces of the globe is necessarily very limited, but we know much concerning its old marine areas. These are the more constant and widespread, and it is mainly upon these deposits, and not so much upon the more limited evidences of ancient land surfaces, that our chronology has been based.

Of the antiquity of cave-folk and their contemporary Mammalia we may expect to hear the very latest utterances from Prof. Boyd Dawkins and Dr. Hicks. The former is also to be congratulated upon his renewed work on the Mammalia in the Palæontographical Society's volume for 1886 (just issued). Prof. O. C. Marsh has added a further contribution to American palæontology in the shape of a memoir describing and figuring sixteen new species of Mesozoic mammals from the Upper Jurassic rocks in Wyoming, on the western slope of the Rocky Mountains. Mr. Lydekker has just completed Part V. of his most useful and much-needed Catalogue of the Fossil Mammalia in the British Museum, containing the Sirenia, Cetacea, Edentata, Marsupialia, and Monotremata.

The fossil birds remain to be catalogued. In the Reptilia it is refreshing to see Prof. Huxley once more taking up the pen and writing upon *Hyperodapedon* and *Rhynchosaurus* in his old vigorous and earnest style. We can only regret that his health precludes him from continuous labour, to the no small loss of science. Prof. Marsh shortly promises us his memoir on the Sauropoda, the plates of which are progressing rapidly to completion.

Our late veteran chief, Sir Richard Owen, although retired from active official duties, contributes a paper on *Galesaurus planiceps*, a Triassic saurian from South Africa, and a further memoir on *Meiolania* from Lord Howe Island.

Prof. Seeley and M. Louis Dollo are both occupied with Dinosauria, the former from the Cape (whence he has also detected part of a mammalian skeleton in the Triassic rocks), and the latter is adding to our knowledge of Iguanodon and other forms from the Wealden of Bernissart.

In the Amphibia, Prof. Dr. Herman Credner has added a most valuable paper on the development of *Branchiosaurus*, a small Labyrinthodont from the Keuper of Saxony, in which he has been able successfully to trace the development through a long series of individuals of a water-breathing naked larva of the Palæozoic epoch into an air-breathing adult form, clad in a strong coat of mail.

In fossil ichthyology, A. Wettstein has been occupied in the study of the Eocene fishes of the Glarus slates, and in his recent memoir he shows that out of one fish (*Anenchelum*), so constantly distorted by slaty cleavage, Agassiz had made no fewer than six species. The fish is now found to be identical with the living "scabbard-fish," *Lepidopus*; and the author reduces the forty-four species of Glarus fishes to twenty-three, and adds four new ones. Among the latter is the first fossil *Remora* yet met with, named *Echeneis glaronensis*. Its first dorsal is modified as a sucker, exactly as in the living *Remora*.

Baron Zigno, of Padua, has figured and described the first entire *Myliobatis* hitherto discovered in the Eocene of Monte Bolca.

M. Louis Dollo records the occurrence of two skeletons of *Carcharodon heterodon* in the Eocene of Boom, Antwerp, one measuring 7 metres, and the other nearly 9 metres in length. They are now mounted and exhibited in the Brussels Museum.

Mr. J. F. Whiteaves is commencing to publish the detailed descriptions of the Devonian fishes from Scaumenac Bay, Quebec.

Mr. James Wm. Davis, of Halifax, has produced a second monograph for the Royal Dublin Society. The first, which appeared in 1883, was devoted to the teeth and spines of Elasmobranch fishes from the Carboniferous limestone of Great Britain; the present monograph, illustrated by twenty-four plates, is devoted to the description of the fishes of the Cretaceous rocks of the Lebanon, and makes us acquainted with a wonderful series of Selachian fishes, representing nine genera and sixteen species, of which two genera and twelve species are new to science. The Ganoids comprise two species of Pycnodonts and two forms related to Amia; there are also a number of Teleostean fishes, amongst which are *Pagellus*, *Beryx*, *Homonotus*, *Platax*, and many other genera. Two species of eel, *Anguilla*, are the first Mesozoic examples recorded. Altogether we have ten genera and sixty-three species of fish recorded as new. The author is to be congratulated upon having contributed to fossil ichthyology one of the most extensive works published in recent years.

Mr. Arthur Smith Woodward (a former student of Owens College, Manchester) has this year also contributed numerous papers on fossil fishes: on *Ptychodus* from the Chalk; *Squaloraja* from the Lias; on the Brazilian genus *Rhacolepis*; on a Maltese *Holocentrum*; "On some Eocene Siluroid Fishes from Bracklesham"; and "On the Canal-System in the Shields of Pteraspidean Fishes."

Mr. E. T. Newton describes a *Semionotus* from the Trias of Warwickshire.

Both Mr. James W. Davis and Dr. R. H. Traquair have given us descriptions of the anatomy of *Chondrosteus acipenseroides* from the Lias of Lyme Regis.

Mr. William Davies describes two species of *Pholidophorus* from the Purbeck beds of Swanage, Dorset.

But the groups which have proved of the greatest service in the chronology of the sedimentary rocks have been the Mollusca, the Brachiopoda, and Crustacea (especially the Trilobita, Phyllopoda, and Ostracoda), the Echinodermata, Corals, Graptolites, Sponges, and Foraminifera.

It would be an interminable task merely to record the workers in the various sections of palæontology, but in glancing at these one cannot prevent many illustrious names arising in one's mind—many who have finished their work, and are reckoned among the fathers of the science, but many also who are still our companions, and from whom we may expect further important help before they lay down their hammer, their lens, and their pen.

In the Cephalopoda the task so lately left by our countryman Dr. Wright, after a long life devoted to palæontological science, has been taken up by Mr. S. S. Buckman, who has already presented one fasciculus of a monograph on the Ammonitidæ of the Inferior Oolite.

The Gasteropoda of the Oolites have an able historian in Mr. W. H. Hudleston, whose contributions on this subject enrich the pages and plates of the *Geological Magazine* and the Proceedings of the Geologists' Association; the Palæozoic forms are in the hands of Dr. Lindström.

The Lamellibranchiata cry for help at present in vain, and we regret more than ever the loss of Stoliczska, who promised such good work had his life been spared.

The Brachiopoda, so long and so well cared for by Dr. Davidson, now also demand a successor to that illustrious name.

The Polyzoa, which suffered so severe a loss in the death of Mr. Busk, have since been well cared for by Mr. Arthur W. Waters and Mr. Vine.

Until quite lately, the oldest fossil insects known were the six fragments of wings of Neuroptera, from the Devonian of New Brunswick, obtained by Mr. C. F. Hartt and described by Mr. S. H. Scudder. More lately the wing of a cockroach has been obtained from rocks of Silurian age in Calvados, France; whilst almost simultaneously fossil scorpions have been met with by Dr. Hunter, of Carluke, in the Upper Silurian of Lanark, and determined by Mr. B. N. Peach, and from the Upper Silurian of Gotland, described by Dr. Lindström.

These discoveries carry back our records of old land surfaces to a far more remote period than that of the Coal-measures, vast as its distance is removed from recent times.

Mr. B. N. Peach is the discover of several scorpions, and I have also recently figured and described three new forms of cockroach and several spined myriapods from the Coal-measures. Another cockroach, also new, which has been kindly sent me for study by Mr. Peach, brings to our knowledge a larval stage of *Blatta* from the Scottish Carboniferous.

Dr. McCook has just added a genus of spiders, *Atypus*, to our Eocene beds from the Isle of Wight.

The Crustacea have found in Mr. B. N. Peach and in Prof. Rupert Jones able and willing historians. Mr. Peach has taken up the Carboniferous Macrouran Decapods, and Prof. Rupert Jones the Palæozoic Phyllopoda, aided by myself; Prof. Jones is attacking the Tertiary and Cretaceous as well as the Palæozoic Ostracoda, so that his hands will be full for many years to come.

The Echinodermata have lost Dr. T. Wright, who for years acted as their monographer in the Palæontographical Society's volumes, but they have secured the services of other accomplished naturalists. Mr. Robert Etheridge, Jun., and Dr. P. Herbert Carpenter have produced a grand monograph on the Blastoidea in the British Museum; and no doubt this is but the beginning of good things to come, for although Mr. Etheridge has entered upon a new sphere of work in the Australian Museum, Sydney, Dr. P. Herbert Carpenter hopes to take up the stalked Crinoids before long, and Mr. Percy Sladen, who, with Prof. P. Martin Duncan, has already done so much good work amongst the Indian Echinoderms and elsewhere, promises to take the star-fishes in hand for us later on.

The Corals have many friends, chief amongst whom is Prof. P. Martin Duncan, and Prof. H. A. Nicholson, and various other excellent workers, but they are even a more difficult and a less attractive group than the Echinodermata, and their determination is not so satisfactory owing to their irregular and heteromorphic growth.

The Stromatoporoids have lost an investigator in the field in Arthur Champernowne, whose unexpected and early loss we all deplore. But in Prof. Nicholson they will find a most careful and painstaking monographer, who has already given us one fine instalment of his work in the Palæontographical volume.

In Prof. C. Lapworth we have an exponent of the structures and affinities of the Graptolites as a class and of their stratigraphical position in the rocks unsurpassed by any other worker. With him must be associated the names of Barrande, Carruthers, Hopkinson, Nicholson, and a long list of foreign workers, all of whom, however, look upon Lapworth as the highest authority in this group.

In the Spongida we are especially indebted to Dr. G. J. Hinde, first for an excellent well illustrated quarto catalogue of these organisms in the geological collection of the British Museum, and secondly for the Palæozoic part of a fine monograph of these for the Palæontographical volume just issued.

Nor must we omit to recall the names of Prof. Zittel, of Dr. Carter, of Prof. Sollas, and many other able workers in the fossil sponges.

In the Foraminifera we naturally recall the names of D'Orbigny, D'Archiac, Carpenter, Parker, Brady and Jones, and Sir William Dawson, our illustrious ex-President. Prof. Rupert Jones is still at work on this group, and has recently published a paper on *Nummulites elegans* from the Eocene beds of Hampshire and the Isle of Wight.

Of late years fossil botany, too long neglected, has taken a place of note in all those inquiries concerning the origin of floras, the age of the stratified rocks, the former distribution of land surfaces, and especially in all questions relative to the climate of the globe in past times.

Passing over the earlier period of the present century, when fossil botany was known only by the works of Artis, Witham, Schlotheim, Sternberg, Goeppert, Cotta, Lindley and Hutton, Steinhauer and Adolphe Brongniart, we have to recall the names of other workers who have only passed away in our own time, such as Binney, Bunbury, Corda, Bowerbank, Heer, Unger, Schimper, and Massalongo.

In the period of fifty years, whose completion we have just celebrated, the names of our countrymen Binney, Bowerbank, Williamson, and Hooker, stand prominently forward contemporarily with those of Geinitz, Unger, Rossmasler, and Schimper, in Germany. In 1845 Dawson and Lesquereux entered the field in America, Hooker in England, and one of the ablest writers on fossil plants, Oswald Heer, entered upon his great work in Switzerland. In 1850 Massalongo in Italy, and von Ettingshausen in Austria, were added to the roll of famous palæobotanists, and in 1853 Newberry joined the American field of research. In 1860 the work so long abandoned by Brongniart, in France, was taken up by de Saporta, and it is no small gratification to have him with us here to-day, and to welcome him amongst our distinguished foreign guests.

About the same time my friend and colleague William Carruthers commenced to write on fossil botany, and brought to bear upon the subject that accurate and careful knowledge of living forms without which such investigations must always prove but futile.

It is extremely difficult to estimate the number of species of fossil plants that had been described up to the year 1837, but it probably fell far short of a thousand. In 1828 less than 500 species were known to Brongniart.

In the first edition of "Morris's Catalogue," published in 1843, the number of British fossil plants recorded is 628.

Careful lists were published by Goeppert and by Unger in 1844 and 1845, giving a total of known species from 1600 to 1800.

In 1849 the number had increased, according to Bronn's "Index Palæontologicus" to over 2000, and the following year Unger enumerated 2421 in his "Genera et Species Plantarum," rather more than 500 of which may have been British. In 1852 Morris (2nd edition) gives the number of species as 750. Since then, chiefly through the labours of Heer, Ettingshausen, Lesquereux, Massalongo, Unger, and de Saporta, this number has been more than quadrupled. Mr. Gardner estimates that at least nine thousand species must have been described. This great increase is chiefly due to the more careful exploration of the Tertiary strata, in which the more highly organized and consequently more differentiated plant forms occur.

The number of plant remains described in Great Britain during the whole fifty years has been extremely small, but much has been accomplished in the study of fossil plants generally, and in this task no one has been more earnest than Prof. Williamson, of Owens College, Manchester.

His investigations of the plants of the Coal period have been of the most exhaustive nature, and from his researches into their microscopic structures we are almost as well acquainted with the minute tissues of these ancient denizens of the forests of the Carboniferous epoch as we are with those in the parks around Manchester to-day.

Mr. Carruthers's "Memoirs on the Coniferæ and Cycadeæ, and on the Fruiting Organs of the Lycopodiaceæ" have greatly advanced our knowledge of these interesting types, heretofore but imperfectly known from their fossil remains.

Mr. R. Kidston has devoted himself most earnestly to the investigation of the fossil plants of our British coal-fields, and he has determined not to rest satisfied merely to work out the plants obtained by others in our museums, but he has visited all our coal-fields and searched the shales on the spot for himself. The results of his collectings may now be seen in the valuable additions made to the Coal-measure series of plants in the British Museum (Natural History).

But it is more especially in reference to the Tertiary flora of Britain that progress has been made of late years.

Thanks to the labours of Mr. Starkie Gardner, who has not only obtained abundant materials for an exhaustive monograph with his own hands from Sheppey, Alum Bay, Bournemouth, Reading, Mull, Antrim, and many other localities, but has already favoured us with several memoirs in the Palæontographical Society's annual volumes and elsewhere on the British Eocene flora, we may hope before long to have a more complete history at this period of our islands than we already possess of the flora of the Carboniferous age.

Nor has any research, favoured by the aid of this Association, brought so large a return in beautiful and instructive specimens to our National Museum of Natural History as have the investigations carried out by Mr. J. S. Gardner.

We must not omit to mention Mr. Clement Reid, who has so diligently traced many of the specimens of our existing flora in the Pleistocene strata of the eastern counties.

"Large numbers of ferns and gymnosperms," says Mr. Gardner, "have been discovered in Mesozoic rocks, but remains of the interesting monocotyledons which must have accompanied them are provokingly scarce. We know that palms, grasses, &c., appear at certain definite horizons, but we are ignorant regarding their ancestry. We know that temperate floras, largely composed of dicotyledons, flourished as far north as man has been able to penetrate, in the Cretaceous and Tertiary periods, but nothing in the least suggesting a transitional form has been found amongst them. Lastly, we have learnt that floras now indigenous to Japan and the Himalayas, to Australia and South America, once inhabited Europe, groups of wholly different plants succeeding and displacing each other in such rapid succession on the same spot as to suggest that the normal condition of floras is one of slow but perpetual migration, and that the term 'indigenous' has no geological significance."

In reference to the question of geographical distribution of organized beings in geological time, the conclusion is strongly forced upon us, from a study of fossil remains, that the great zoological provinces into which the earth's surface and the seas of the globe are now subdivided have been brought about by the limitation of species at no more distant date than the Secondary period, and probably even later than this.

That in Palæozoic times there must have been a great uniformity of marine conditions, and the fauna of each of the primary formations was consequently not only of vast duration but of world-wide extent, is evident.

When, as in Carboniferous times, we are enabled to study the contemporary land conditions of the globe, we find they must also have been very uniform, at least so far as the explored parts of this hemisphere are known, both the fauna and the flora at this epoch being co-extensive with the northern hemisphere, indeed, in all probability far wider, seeing that identical species occur in the Carboniferous series of Australia and North America. Even those well-marked lines which at present follow more or less closely the isotherms of our hemisphere seem not to have exercised the same influence on the fauna and flora as they do at present. Thus in high northern latitudes and within the Arctic Circle we find abundant evidence of life in Palæozoic, Mesozoic, and even down to Tertiary times, unaffected by latitude; so that we are justified in assuming that a far milder temperature extended to much higher northern regions than that which at present exists on the globe, and consequently that a larger portion of the earth's surface (as well as its seas) was then habitable.

How great, then, is the field of research still open to our investigation, and how far distant must that day be ere the last problem shall have been solved, and the last chapter written, in the ancient life-history of our earth!

> "We write in sand, our labour grows,
> And with the tide the work o'erflows."

With unskilled hand I have struck here and there only a few chords on the many-toned harmonicon of geology. I fear they may not all have vibrated quite in unison as a perfect composition would; but, however crude the performance has been, I trust that it will not be provocative of discord. If some few ideas suggest themselves as worthy of your acceptance, I shall not have spoken altogether idly, nor you have listened so long and so patiently entirely in vain.

NOTES.

Dr. Emil Holub has arrived in Europe, after three years of adventurous exploration in Africa, and although he brings with him only a part of his scientific collections—the rest having been plundered by the Mashukulumbe, a tribe to the north of the Zambesi—yet this fragment is certain to prove of great scientific value. It includes over 2000 specimens of birds, 27,000 of insects, and 6500 of plants. Dr. Holub brings with him also many hundred observations in meteorology and measurements of all nations. The collection will be exhibited in Europe; and it is stated that, if circumstances enable them to do so, Dr. and Mrs. Holub will resume their African explorations. They are expected to arrive in Vienna on the 15th or 16th inst., and will be officially received by the Austro-Hungarian Exploration Society.

A slight shock of earthquake was felt at Bonn and in its vicinity at fifty-two minutes past four on Monday afternoon, accompanied by a dull subterranean rumbling.

The Government of India has sanctioned the purchase for the Lahore Museum of a zoological collection illustrative of Indian silk culture.

M. Pavie, who undertook an adventurous journey from Siam into Tonquin, starting from Luang Prabang, has been compelled to return to Siam, having been driven back by bands of Chinese, who are described as ravaging the country.

Mr. J. B. Lillie Mackay, of the Royal School of Mines, and at present Lecturer in Chemistry at Trinity College, Melbourne University, has been appointed Director of the School of Mines, Sandhurst, Victoria.

Mr. Omond, the Superintendent of the Ben Nevis Observatory, replying in the *Times* to criticisms in Parliament of the work of the Observatory, to which we referred in a note last week points out that the report of the Meteorological Office, on which these criticisms were based, merely states that Mr. Omond's telegrams were useless for a particular purpose, viz. forecasting storms. He then proceeds to state the position of the Observatory in the matter as follows:—"When the Ben Nevis Observatory was opened at the close of 1883, the directors offered to send the Meteorological Office daily weather telegrams—the only satisfactory way in which observations can be used for forecasting. The Office declined this offer, mainly on the ground of expense, but asked that occasional messages might be sent when any unusual or interesting events occurred, adding that these messages might be sent more frequently at first. I had some difficulty in understanding why the Office wished a record of sudden changes to be sent by wire instead of waiting till they got them in the ordinary way by post, but considered that the best way was to follow their instructions literally and send telegrams recording sudden changes or unusual occurrences, with a due regard to economy and with gradually diminishing frequency. A reiterated request for economy at the time of the introduction of sixpenny messages led to the entries sent being usually restricted to two hours' reading of the various instruments —one before the change in question had set in and one after it was fairly established. Though most sudden changes are connected with storms, yet a few of the messages recorded changes from bad to good weather, especially when the occasional great dryness characteristic of high-level stations began suddenly or in any unusual manner. How a Committee, 'composed of men of the very highest scientific standing,' had come to regard these messages, and especially this last sort, as 'storm-warnings' passes my comprehension. The question of the value to meteorological science of the Ben Nevis observations I may safely leave to abler hands—it needs no vindication of mine; but I must protest against the unfairness to me and the other members of the Ben Nevis Observatory staff of first asking for records of changes which have occurred, and then declaring them useless because they are not also forecasts of what is to be. It should be noted, however, that in two cases the record from Ben Nevis arrived at the

Meteorological Office before the warning from the sea-level stations—a fact which proves, all the more conclusively because unintentionally, the great value Ben Nevis would have if properly utilized as a forecasting station by means of daily reports sent to some one able both to interpret their indications and to compare them with similar daily reports from low-level stations all over the country. The former of these conditions can only be fulfilled by those who have studied the Ben Nevis records of the last three years and a half, while the latter in this country necessarily is limited to the Meteorological Office, which alone receives such daily telegrams. It is to this unfortunate hiatus and not to any defect in the position of Ben Nevis that the alleged uselessness for practical forecasting of its observations is due." Mr. Omond's vigorous defence is therefore the same in substance as that of Mr. Buchan which we reproduced last week. It amounts to this: the Observatory is blamed for not doing certain work which it offered to do, but which the Meteorological Office refused to permit it to do; Mr. Omond did what he was asked, and did not do that which the Meteorological Council refused to have from him on the score of expense.

THE Committee of the International Geological Congress dealing with the question of geological nomenclature is holding its meeting in Manchester at the same time as the British Association. The representatives at present in Manchester are Prof. G. Capellini, Rector of the University of Bologna, and representative of Italy (President); Prof. Dewalque, Belgium (Secretary); Prof. O. Torell, Sweden; Prof. Vilanova-y-Piera, Spain; Dr. T. Sterry Hunt, Canada; Dr. W. T. Blanford, India; and Prof. T. McK. Hughes, England. Meetings of the Committee, at which the foregoing were present, were held in the Committee-room of the Geological Section on Tuesday and Wednesday last week, at which Mr. J. E. Marr and Mr. W. Topley, of the English International Committee, were also present. The object of this Committee of the Congress is to discover the nomenclature and classification adopted by different authorities and in different countries, with the view of bringing their views into harmony, and also to lay down rules for the guidance of geological workers in the future. The subject chiefly discussed has been the classification of the Cambrian and Silurian rocks, some reference having also been made to the question of the Archæan rocks. The full Congress will meet in England next year, when these and other similar matters will be considered. Another meeting of the Congress Committee was held on Friday.

THE autumn meetings of the Iron and Steel Institute are announced to take place this year at Manchester. They will be held at Owens College, the use of which has been granted by the Governors for the occasion, and will begin on the morning of September 14. The programme is a very comprehensive one viewed from a metallurgical and manufacturing point of view, while the more strictly holiday features of such a gathering have not been neglected. The President of the Institute (Mr. Daniel Adamson), will give a paper on "Machines for the Testing of Metals," a subject to which he has devoted a great deal of attention. Mr. Thomas Ashbury is down for a paper on "Recent Metallurgical and Mechanical Progress, as illustrated at the Manchester Exhibition," and as that Exhibition, so far as relates to machinery in motion, is probably the best that has ever been held, and illustrates with unusual completeness every department of the engineering art, such a paper can scarcely fail to be an instructive and valuable record. A third paper will be read by Mr. James Johnson, on "The Mechanical Apparatus for Continuous Moulding at the Works of M. Godin, at Guise," the interest of which will be of a two-fold character—first, as regards the processes and apparatus to be described, which are of a novel and improved character; and, secondly, as regards the system of co-operation which has been adopted there on a more complete and successful scale as between employers and employed than probably in any other part of Europe. Dr. Fleming, of University College, London, will contribute a paper on "Electric Light Installations for Works and Factories," a subject with reference to which he has had a very large amount of experience as the consulting engineer for the Edison-Swan and other companies. The manufacture of ordnance, respecting which there has been so much discussion in military and political circles of late, will be brought forward by Capt. Cubillo, who occupies a responsible position at the Royal Arsenal of Trubia, in Spain. Capt. Cubillo has studied the conditions of the manufacture of ordnance at all the leading arsenals both in this country and abroad, and as the Spanish Government has recently exhibited a strong disposition to be abreast of the world in naval and military affairs, this paper is likely not only to show what they have so far achieved, but also to bring to the front comparisons with reference to ordnance that will probably be extremely useful and interesting at the present time. One other paper, that by Mr. Wailes, of the Patent Shaft and Axle-tree Company, at Wednesbury, promises to give to the world some extremely interesting data respecting the recent progress of the basic process for the manufacture of steel on the open hearth. Hitherto, as is well known, the progress of this system, both on the Continent and in England, has been chiefly in the direction of Bessemer working, but recently the open hearth has come into competition with the Bessemer process for this purpose, and Mr. Wailes's paper will probably lead to a discussion as to the comparative merits of the two systems which will be of value both to producers and consumers of steel.

THE *Times* Correspondent in Philadelphia telegraphed on Monday night that an International Medical Congress was formally opened by President Cleveland at Albaugh's Opera House in Washington on Monday. Five thousand physicians are in attendance, including over 2000 delegates sent here from nearly all parts of the globe. One of those present, Dr. Fanny Dickinson, of Chicago, is the first woman who has been admitted to an international medical gathering as delegate. Many of the most distinguished doctors of the day were present. An address of welcome was delivered by Mr. Bayard, Secretary of State. The welcome was acknowledged and responded to by Dr. William Harris Lloyd, Inspector-General R.N., on behalf of Great Britain; Dr. Léon Laforte, of Paris, on behalf of France; Prof. P. G. Unna, of Hamburg, on the part of Germany; Senator M. Semmola, of Naples, for Italy; and Dr. Charles Reyber, of St. Petersburg, representing the Government of Russia.

THE centenary of the first ascent of Mont Blanc was celebrated at Chamounix on the 28th ult., when a monument for which the various Alpine and tourist clubs of the world, as well as the Paris Academy of Sciences, contributed the funds, was at the same time unveiled to De Saussure, who made the ascent on August 28, 1787, with the guide Jacques Balmat. The monument is of bronze on a granite pedestal. The principal feature of the procession with which the proceedings opened was the band of old guides, forty in number, and all over seventy years of age. M. Spuller, French Minister of Education, unveiled the statue, and delivered an oration in honour of De Saussure.

CONSIDERABLE additions have recently been made to our knowledge of those interesting substances, the chemical products of the action of Bacteria upon the animal body, called ptomaïnes. In the last number of the *Berichte* of the German Chemical Society, p. 2217, Dr. Ladenburg clears up completely all doubt as to the composition of one of the best known of the ptomaïnes, cadaverin, which he shows is perfectly identical with artificially prepared penta-methylene-diamine. A short time ago Dr. Bocklisch published (*Ber.* 1887, p. 1441) the results of his researches upon the products of the action of Finkler's bacillus (*Vibrio proteus*) upon sterilized flesh, showing that this bacillus decomposes flesh with formation of the alkaloid cadaverin, $C_5H_{14}N_2$, which is non-poisonous, and ammonia. On repeating his experiments, however, in presence of ordinary putrefaction germs in addition to the Finkler bacilli, he made the remarkable discovery that an entirely different base, methyl-guanidine, of intensely poisonous properties, was the chief product; hence the symptoms of particular diseases may be due to the poisonous alkaloids formed by the joint action of specific bacilli and ordinary putrefaction germs. Bocklisch made several analyses of the cadaverin which he obtained in the first series of experiments, due to the action of pure cultivations of the *Vibrio proteus*, and showed that its hydrochloride forms a crystalline compound with mercuric chloride of the composition $C_5H_{14}N_2 . 2HCl . 4HgCl_2$, and as this differed somewhat from the composition formerly assigned to the artificial preparation by Ladenburg, the subject was involved in some doubt. Happily, Ladenburg has made fresh and purer preparations of his penta-methylene-diamine, and finds that its compound with mercuric chloride has precisely the composition assigned to the double chloride of mercury and cadaverin by Bocklisch. Hence cadaverin is conclusively proved to be none other than penta-methylene-diamine, and, consequently, must be added to the list of products of animal life which have been synthesized. The formation of these alkaloids, during disease or after death, has a most important bearing upon the treatment of cases of suspected poisoning, inasmuch as, whether poisonous or not, their reactions differ very little from those of the deadly alkaloids; and in the interests of justice it is to be hoped that our knowledge of this branch of organic chemistry may soon be rendered as complete as possible.

MESSRS. MACMILLAN announce the following scientific works for the forthcoming publishing season:—"Electricity and Magnetism," by Amédée Guillemin, translated and edited, with additions and notes, by Prof. Silvanus P. Thompson; "The Nervous System and the Mind," by Charles Mercier; "Popular Lectures and Addresses on Various Subjects in Physical Science," by Sir William Thomson; "Radiant Light and Heat," by Balfour Stewart, F.R.S. (the last three belonging to the Nature Series); "Kinematics and Dynamics," by J. G. Macgregor; "Geometrical Conics," by A. Cockshott and Rev. F. B. Walters; "A Treatise on Analytical Statics," by I. Todhunter, F.R.S., a new edition, revised by Prof. J. D. Everett, F.R.S.; "A Key to Mr. Todhunter's Conic Sections," by C. W. Bourne; "A Key to some Examples in Messrs. Jones and Cheyne's Algebraical Exercises," by Rev. W. Failes; "A Key to Mr. Lock's 'Arithmetic for Schools,'" by the Rev. R. G. Watson; "A Companion to 'Weekly Problem Papers,'" by the Rev. John J. Milne; "A Key to Dr. Todhunter's Treatise on the Differential Calculus," by H. St. J. Hunter; "A Treatise on Chemistry," by Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. (Vol. IV. Part I.); "Algebra for Schools and Colleges," by Charles Smith; "The Elements of Chemistry," by Ira Remsen; "Absolute Measurements in Electricity and Magnetism," by Andrew Gray; "A Practical Text-Book of Pathology," by D. J. Hamilton; "A Course of Quantitative Mineral Analysis for Students," by W. Noel Hartley, F.R.S.; "School Course of Practical Physics," by Prof. Balfour Stewart, F.R.S., and W. W. Haldane Gee (Part I. "Electricity and Magnetism); "Examples in Physics," by D. E. Jones; "Inorganic and Organic Chemistry," by Sir Henry E. Roscoe, F.R.S., and Prof. C. Schorlemmer, F.R.S. (Vol. III. "Organic Chemistry," Part IV.); "Greenland," by Baron A. E. von Nordenskjöld; and "Corea," by W. A. Carles.

MESSRS. SWAN SONNENSCHEIN AND CO.'s announcements of new books include the following works:—"The Microscope," edited from the work of Profs. Naegeli and Schwendener, by Frank Crisp and J. Mayall, Jun.; "Animal Biology," by Adam Sedgwick; "British Fishes," by F. A. Skuse; "Mammalia," by F. A. Skuse; "Reptiles," by Catherine Hopley; "Ants and Bees," by W. Harcourt Bath (the last four in the Young Collector Series); "The Solomon Islands and their Natives" and "The Geology and Physical Characteristics of the Solomon Islands," with maps, by Dr. H. B. Guppy.

MESSRS. LONGMANS announce the following works of scientific interest:—"The Literary Remains of Fleeming Jenkin, F.R.SS.L. and E., late Professor of Engineering in the University of Edinburgh," edited by Sidney Colvin, with a Memoir by Robert Louis Stevenson; "Picturesque New Guinea," by J. W. Lindt; "A Manual of Operative Surgery, having Special Reference to many of the Newer Procedures," by Arthur E. J. Barker; "A Course of Lectures on Electricity, delivered before the Society of Arts," by George Forbes, F.R.S.

MESSRS. KEGAN PAUL AND CO., London, and Messrs. Appleton and Co., New York, will publish shortly the Hon. Ralph Abercromby's work on "Weather" as a number of the International Science Series. This will be the first book in the English language which deals exclusively with the nature of weather changes from day to day, as distinguished from the climatic or statistical treatment of the subject. There will be ninety-nine charts and diagrams, of which a considerable number will relate to the United States, and others to India and Australia, so as to illustrate the nature of weather on the widest possible basis.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Sumatra, presented by Mr. B. Lynch; a Fettered Cat (*Felis maniculata*), a Spotted Eagle Owl (*Bubo maculosus*), a Hoary Snake (*Coronella cana*), four Spotted Slowworms (*Acontias meleagris*) from South Africa, presented by Dr. E. Holub, C.M.Z.S.; a Carrion Crow (*Corvus corone*), British, presented by Mrs. MacLochlin; a Martinique Gallinule (*Ionornis martinicus*), captured at sea, presented by Mr. R. Drane; two African Lepidosirens (*Protopterus annectens*) from the River Gambia, West Africa, presented by Mr. H. H. Lee; a Malabar Parrakeet (*Palæornis columboides* ♂) from Southern India, a Malaccan Parrakeet (*Palæornis longicauda* ♂) from Malacca, a Laughing Kingfisher (*Dacelo gigantea*) from Australia, deposited; a Tiger Bittern (*Tigrisoma brasiliense*) from Brazil, purchased; a Red-faced Ouakari (*Brachyurus rubicundus* ♀) from the Upper Amazons, received in exchange; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

BROOKS'S COMET.—Mr. H. V. Egbert, from observations made on August 26, 28, and 30, has computed the following elements for the comet discovered by Mr. Brooks on August 24:—

$$T = 1887 \text{ October } 6{\cdot}48\text{d. G.M.T.}$$

$$\left.\begin{aligned} \pi - \Omega &= 63^\circ\ 18' \\ \Omega &= 84\ \ 33 \\ \iota &= 44\ \ 10 \end{aligned}\right\} \text{Mean Eq. } 1887{\cdot}0.$$
$$\log q = 0{\cdot}08718$$

It will be seen that these elements bear a great resemblance to those of Olbers' comet of 1815. Dr. Holetschek, in a supplemental circular of the *Astronomische Nachrichten*, supplies the subjoined ephemeris for the comet, basing it upon the sweeping ephemeris for Olbers' comet given by Ginzel in the *Astronomische Nachrichten*, No. 2696, the comet's orbit being assumed to be a parabola for the sake of simplicity, and the following observation made by Herr Palisa at Vienna being used :—

August 27, 15h. 27m. 12s. Vienna M.T.
R.A. = 8h. 42m. 55·71s., Decl. 29° 34′ 24″·7 N.

Ephemeris for Berlin Midnight.

1887.	R.A. h. m.	Decl. ° ′	Log *r*.	Log Δ.
September 5	9 21·1	30 16 N.	0·120	0·311
9	9 39·7	30 20	0·112	0·304
13	9 58·8	30 16	0·105	0·297
17	10 18·4	30 2 N.	0·099	0·291

The Morrison Observatory.—The first number of the publications of the Morrison Observatory, Glasgow, Missouri, U.S.A., has just appeared. This Observatory was founded, in 1875, by the liberality of Miss Berenice Morrison, and possesses an equatorial refractor by Alvan Clark, of 12¼ inches aperture, and a transit-circle by Troughton and Simms, with objective of 6 inches aperture and 77 inches focal length, the circles being 24 inches in diameter. In this first volume Prof. C. W. Pritchett, the Director, gives a history and description of the Observatory, with an account of the determination of the longitude and latitude of the meridian pier, besides a selection of such observations and notes made at the Observatory as are likely to be of use to astronomers. These latter include measures of double stars, observations of occultations, of the transit of Mercury, 1878, measures of the diameter of Mars, observations of comets, of Jupiter and Saturn, and of the figure and dimensions of Uranus. Prof. Pritchett's work appears to have been seriously crippled through lack of means, and, considering the excellent use which he has made of the resources at his command, it is to be hoped that he may speedily find himself in a position to carry on the operations of the Observatory on a more extended scale.

New Observatory at Juvisy.—The current number of *L'Astronomie* contains a description of a new Observatory belonging to M. Camille Flammarion, which has just been completed. An admirer of M. Flammarion had presented him some five years ago with a little chateau and park situated on the road from Paris to Fontainebleau of historic name and interest. The house, which was built in 1730, possessed walls so thick and solid as to serve as a perfectly stable base for the equatorial and dome with which M. Flammarion has surmounted it. The dome is 5 m. in interior diameter, and covers an equatorial by Bardou of 0·24 m. aperture and 3·75 m. focal length, with clockwork by Bréguet, furnished with a Villarceau governor. Two smaller telescopes—one by Secretan of 108 mm. aperture, the other by Foucault of 160 mm., stand on the adjoining terrace. The Observatory, the co-ordinates of which are East longitude from Paris 0h. 0m. 8s., N. latitude 48° 41′ 36″, commands an uninterrupted horizon, and an atmosphere noticeably purer than that of Paris.

The Total Solar Eclipse of August 19.—We learn from the current number of *Ciel et Terre* that M. Niesten, of the Brussels Observatory, was fairly successful in his observations of the eclipse. It had been his intention to push on as far east as Perm, but a delay in the arrival of his instruments led him to accompany M. Belopolsky to Jurjewitz on the Volga. The sky was cloudy here as at most of the other stations, but cleared a little round the sun at the time of totality, and M. Niesten was able to see the chromosphere and prominences, and the appendices of the corona, and his assistant secured eight photographs, of which six were good. The exposures varied from 8 seconds to half a minute; the chromosphere and prominences were shown on all, and two gave traces of the corona and also of Regulus, which was near the sun. M. Karinne, a Moscow photographer of the same station, also secured several photographs. A drawing which M. Niesten made of the corona showed a strongly-marked coronal ray, about a degree in length, in the direction of the solar equator.

Minor Planet No. 267.—M. Charlois, of Nice, who discovered this object, has named it Tirza.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *SEPTEMBER* 11-17.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 11

Sun rises, 5h. 30m.; souths, 11h. 56m. 36·1s.; sets, 18h. 24m.; decl. on meridian, 4° 35′ N.: Sidereal Time at Sunset, 17h. 46m.

Moon (New, September 17, 14h.) rises, 22h. 27m.*; souths, 6h. 22m.; sets, 14h. 22m.; decl. on meridian, 19° 12′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	5 28	12 1	18 34	5 48 N.
Venus	7 23	12 40	17 57	9 9 S.
Mars	1 43	9 29	17 15	18 55 N.
Jupiter	9 46	14 48	19 50	12 1 S.
Saturn	1 8	9 0	16 52	19 47 N.

* Indicates that the rising is that of the preceding evening.

Sept.	h.	
14	2	Saturn in conjunction with and 1° 39′ north of the Moon.
14	18	Mars in conjunction with and 1° 48′ north of the Moon.
17	16	Venus in conjunction with and 12° 50′ south of the Moon.
17	22	Mercury in conjunction with and 2° 33′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
Algol	3 0·8	40 31 N.	Sept. 11,	2 19	*m*
			,, 13,	23 8	*m*
λ Tauri	3 54·4	12 10 N.	,, 11,	3 6	*m*
			,, 15,	1 59	*m*
δ Libræ	14 54·9	8 4 S.	,, 12,	4 5	*m*
			,, 16,	19 48	*m*
U Coronæ	15 13·6	32 4 N.	,, 16,	2 34	*m*
R Draconis	16 32·4	67 0 N.	,, 12,		*m*
U Ophiuchi	17 10·8	1 20 N.	,, 11,	22 34	*m*
			and at intervals of	20 8	
X Sagittarii	17 40·5	27 47 S.	Sept. 14,	23 0	*m*
W Sagittarii	17 57·8	29 35 S.	,, 12,	4 0	*M*
U Sagittarii	18 25·2	19 12 S.	,, 14,	0 0	*m*
R Aquilæ	19 0·9	8 4 N.	,, 16,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 14,	4 0	*M*
W Cygni	21 31·8	44 52 N.	,, 11,		*M*
δ Cephei	22 25·0	57 50 N.	,, 19,	2 0	*M*
			,, 15,	20 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near χ′ Orionis	88°	18° N.	Very swift; streaks.
,, 50 Aurigæ	98	43 N.	Very swift; streaks.
,, α Lyræ	282	42 N.	Swift; bright; long.
,, γ Piscium	346	0 N.	Slow; bright.

SCIENTIFIC SERIALS.

The most interesting item of information in the *Journal of Botany* for August is the record of an addition to the flowering plants of Great Britain, in the discovery, by Mr. H. C. Hart, of the Arctic *Arabis alpina* in Skye.—Mr. Tokutaro Ito, has an interesting paper on the history of botany in Japan.

Rendiconti del Reale Istituto Lombardo, June 30.—On the normal derivatives of the potential function of surfaces, by G. Morera. This paper forms a supplement to the author's late communication (*Rendiconti*, vol. xx. Part 8) on the derivatives of the potential function of space. The extremely simple analytical method by which he succeeded in determining general conditions for the existence of those derivatives and their effective expressions has also enabled him to solve the analogous question regarding the normal derivatives of the potential function of surfaces.—On the part played by sensuous images on the development and exercise of the reasoning faculty, by Tito Vignoli. In this paper the author investigates the actual form and genesis of perceptions acquired through the senses, from the standpoint of their efficacy in developing and sharpening the intelligence of animals. The subject is treated comparatively, it being impossible to understand any act or fact of human psychology unless studied in connexion with similar manifestations in other animals. The general conclusion is arrived at that the impression communicated through the senses is the true instrument of intellectual progress, and that in it lies the potentiality of abstract science. Pure geometry, arithmetic, and algebra are merely the last term of abstract simplification reached by the sensuous perception in its intrinsic evolution.

In the *Nuovo Giornale Botanico Italiano* for July, Sig. P. Voglino publishes critical remarks on a number of species of Fungi belonging to the Agaricini; and Prof. Caruel gives his Annual Report of the Botanical Museum of Florence for the year 1885-86.—Prof. Delpino discusses the chemical and physiological equation of the process of alcoholic fermentation, which he considers to be more simple than it has been regarded by recent writers. Succinic acid and glycerin, which are found in the liquid after fermentation, he believes to be only secondary products of the process, which consists in the simple removal from grape-sugar of a portion of its carbon and oxygen, and its consequent reduction to the constitution of alcohol.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 29.—M. Hervé Mangon in the chair.—On tornadoes in the United States, by M. H. Faye. Some observations are made in connexion with the popular work on tornadoes recently issued by Mr. Finley, of the United States Signal Service. In reply to that writer, M. Faye maintains that tornadoes are not ascending but descending movements, being whirlwinds with vertical axes due to the different velocities of moderately elevated atmospheric currents, which, like the eddies in running waters, always descend till arrested by the resistance of the ground. They penetrate like a cork-screw through the lower strata, continually contracting and tapering to a point owing to the increasing pressure of these lower strata. Their progressive movement, mainly towards the north-east, is due to the upper currents where they take their rise, and whose mean velocity and direction they retain. Their ravages are due to the violent shock of the descending spirals against the obstructions of the ground, and their fury is not spent or diminished in overcoming these obstacles, because the source of their energy is always in the upper regions, where it is constantly renewed and transmitted to the earth by the descending motion. It is further shown that the United States comply more than any other region of the globe with the conditions most favourable for the development of these destructive cyclones.—Observations of Barnard's comet, May 12, 1887, made at the Observatory of Bordeaux with the 0·38 m. equatorial, by MM. G. Rayet and Flamme.—Determination of the longitude of Haiphong, Tonquin, by telegraphy, by M. F. La Porte. Its longitude was for the first time determined in 1874 by MM. Héraud and Bouillet, who deduced it from that of Saigon. But at the beginning of this year the meridian of Haiphong was connected with that of Hong Kong by means of the submarine cable, and from the observations taken at both extremities a mean was obtained for the cathedral of Hong Kong of 7h. 27m. 20·43s., and for the Observatory of Haiphong, 6h. 57m. 22·63s. east of Paris. For the latter point Héraud's chronometric observations had given 6h. 57m. 19·8s.—Note on a projection saccharimeter, by M. Léon Laurent. The saccharimeters already constructed by the author are of two types: the rotatory polarimeter, requiring monochromatic light; and the compensating saccharimeter, more specially adapted for sugar, and using ordinary light. The present apparatus, of which a sectional view is given, has the advantage of being adapted for use with the electric light now so generally employed in large scientific establishments.—Experiments in agricultural chemistry, by M. J. Raulin. The experiments here described were carried out last year at the agricultural station of the Rhone. Their special object was to ascertain how the disturbing influences due to the varying fertility of the soil may best be obviated. The land being disposed in three equal plots, A, B, C, the extremes A and C are treated identically, while B serves as the point of comparison for the special circumstance under consideration. Normally the fertility increases or diminishes with a certain uniformity from A to C, so that half of the joint yield of A + C would be equal to that of B if the three plots were subjected to the same treatment. The cause of error due to the inequality of the soil being thus for the most part removed, the relation of the yield of $\frac{A + C}{2}$ to that of B will express the actual influence of the circumstance under consideration. In an experiment carried on according to this method, the superphosphate and precipitated phosphate used with Dattel wheat gave a very decided increase, while the result of the application of fossil phosphates and scoriæ was somewhat doubtful.—Note on the waterspout of August 19 on the Lake of Geneva, by M. Ch. Dufour. This waterspout, formed by the collision of the west and the *vaudaire* or south wind, immediately disappeared on reaching the shore half a mile west of the Rivaz railway-station on the Swiss side. From the data supplied by various observers the author calculates its height at about 106 metres. It churned up the surface of the lake, producing an effect somewhat like that of paddle-steamers, but did no damage of any kind on the land.—M. R. Guérin presented a note on a process by means of which the question of the lunar atmosphere might be elucidated. He remarked that the diurnal motion of the moon, owing to its proper motion, is not the same as that of a star. Hence, under certain conditions, a photographic lunette would give to our satellite a sharp edge, and for a star in the neighbourhood of this edge a luminous streak. It therefore seems certain that, however attenuated may be the lunar atmosphere, the photogenic conditions will be changed at the point of contact of the two heavenly bodies, and that the streak made by the image of the star should show some trace of this change.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Animal Alkaloids: Sir W. Aitken (Lewis).—City and Guilds of London Institute Programme of Technological Examinations, Session 1887-88.—First Steps in Geometry: R. A. Proctor (Longmans).—Easy Lessons in the Differential Calculus: R. A. Proctor (Longmans).—Australian Museum; Report of Trustees.—A Treatise on the Animal Alkaloids: A. M. Brown (Baillière).—Hydrophobia: R. Suzor (Chatto and Windus).—The Glasgow and West of Scotland Technical College Calendar for the year 1887-88 (Anderson, Glasgow).—Elementos de Calculo de los Cuaterniones: V. Balbin (Buenos Aires).—Les Plantes des Champs et des Bois: G. Bonnier (Baillière, Paris).—Bench Book for Test-Tube Work in Chemistry: H. T. Lilley (Hamilton).—Notes upon the History of Floods in the River Darling: H. C. Russell.—Notes upon Floods in Lake George: H. C. Russell.—Results of Rain and River Observations made in New South Wales and part of Queensland, 1886: H. C. Russell (Sydney).—Journal of Chemical Society, September (Gurney and Jackson).

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